A HISTORY OF SCIENCE

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IN FIVE VOLUMES

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A HISTORY OF SCIENCE- BOOK I

Should the story that is about to be unfolded be found to lack interest, the writers must stand convicted of unpardonable lack of art. Nothing but dulness in the telling could mar the story, for in itself it is the record of the growth of those ideas that have made our race and its civilization what they are; of ideas instinct with human interest, vital with meaning for our race; fundamental in their influence on human development; part and parcel of the mechanism of human thought on the one hand, and of practical civilization on the other. Such a phrase as "fundamental principles" may seem at first thought a hard saying, but the idea it implies is less repellent than the phrase itself, for the fundamental principles in question are so closely linked with the present interests of every one of us that they lie within the grasp of every average man and woman--nay, of every well-developed boy and girl. These principles are not merely the stepping-stones to culture, the prerequisites of knowledge--they are, in themselves, an essential part of the knowledge of every cultivated person.

It is our task, not merely to show what these principles are, but to point out how they have been discovered by our predecessors. We shall trace the growth of these ideas from their first vague beginnings. We shall see how vagueness of thought gave way to precision; how a general truth, once grasped and formulated, was found to be a stepping-stone to other truths. We shall see that there are no isolated facts, no isolated principles, in nature; that each part of our story is linked by indissoluble bands with that which goes before, and with that which comes after. For the most part the discovery of this principle or that in a given sequence is no accident. Galileo and Keppler must precede Newton. Cuvier and Lyall must come before Darwin;--Which, after all, is no more than saying that in our Temple of Science, as in any other piece of architecture, the foundation must precede the superstructure.

We shall best understand our story of the growth of science if we think of each new principle as a stepping-stone which must fit into its own particular niche; and if we reflect that the entire structure of modern civilization would be different from what it is, and less perfect than it is, had not that particular stepping-stone been found and shaped and placed in position. Taken as a whole, our stepping-stones lead us up and up towards the alluring heights of an acropolis of knowledge, on which stands the Temple of Modern Science. The story of the building of this wonderful structure is in itself fascinating and beautiful.

I. PREHISTORIC SCIENCE

To speak of a prehistoric science may seem like a contradiction of terms. The word prehistoric seems to imply barbarism, while science, clearly enough, seems the outgrowth of civilization; but rightly considered, there is no contradiction. For, on the one hand, man had ceased to be a barbarian long before the beginning of what we call the historical period; and, on the other hand, science, of a kind, is no less a precursor and a cause of civilization than it is a consequent. To get this clearly in mind, we must ask ourselves: What, then, is science? The word runs glibly enough

upon the tongue of our every-day speech, but it is not often, perhaps, that they who use it habitually ask themselves just what it means. Yet the answer is not difficult. A little attention will show that science, as the word is commonly used, implies these things: first, the gathering of knowledge through observation; second, the classification of such knowledge, and through this classification, the elaboration of general ideas or principles. In the familiar definition of Herbert Spencer, science is organized knowledge.

Now it is patent enough, at first glance, that the veriest savage must have been an observer of the phenomena of nature. But it may not be so obvious that he must also have been a classifier of his observations--an organizer of knowledge. Yet the more we consider the case, the more clear it will become that the two methods are too closely linked together to be dissevered. To observe outside phenomena is not more inherent in the nature of the mind than to draw inferences from these phenomena. A deer passing through the forest scents the ground and detects a certain odor. A sequence of ideas is generated in the mind of the deer. Nothing in the deer's experience can produce that odor but a wolf; therefore the scientific inference is drawn that wolves have passed that way. But it is a part of the deer's scientific knowledge, based on previous experience, individual and racial; that wolves are dangerous beasts, and so, combining direct observation in the present with the application of a general principle based on past experience, the deer reaches the very logical conclusion that it may wisely turn about and run in another direction. All this implies, essentially, a comprehension and use of scientific principles; and, strange as it seems to speak of a deer as possessing scientific knowledge, yet there is really no absurdity in the statement. The deer does possess scientific knowledge; knowledge differing in degree only, not in kind, from the knowledge of a Newton. Nor is the animal, within the range of its intelligence, less logical, less scientific in the application of that knowledge, than is the man. The animal that could not make accurate scientific observations of its surroundings, and deduce accurate scientific conclusions from them, would soon pay the penalty of its lack of logic.

What is true of man's precursors in the animal scale is, of course, true in a wider and fuller sense of man himself at the very lowest stage of his development. Ages before the time which the limitations of our knowledge force us to speak of as the dawn of history, man had reached a high stage of development. As a social being, he had developed all the elements of a primitive civilization. If, for convenience of classification, we speak of his state as savage, or barbaric, we use terms which, after all, are relative, and which do not shut off our primitive ancestors from a tolerably close association with our own ideals. We know that, even in the Stone Age, man had learned how to domesticate animals and make them useful to him, and that he had also learned to cultivate the soil. Later on, doubtless by slow and painful stages, he attained those wonderful elements of knowledge that enabled him to smelt metals and to produce implements of bronze, and then of iron. Even in the Stone Age he was a mechanic of marvellous skill, as any one of to-day may satisfy himself by attempting to duplicate such an implement as a chipped arrow-head. And a barbarian who could fashion an axe or a knife of bronze had certainly gone far in his knowledge of scientific principles and their practical application. The practical application was, doubtless, the only thought that our primitive ancestor had in mind; quite probably the question as to principles that might be involved troubled him not at all. Yet, in spite of himself, he knew certain rudimentary principles of science, even though he did not formulate them.

Let us inquire what some of these principles are. Such an inquiry will, as it were, clear the ground for our structure of science. It will show the plane of knowledge on which historical

investigation begins. Incidentally, perhaps, it will reveal to us unsuspected affinities between ourselves and our remote ancestor. Without attempting anything like a full analysis, we may note in passing, not merely what primitive man knew, but what he did not know; that at least a vague notion may be gained of the field for scientific research that lay open for historic man to cultivate.

It must be understood that the knowledge of primitive man, as we are about to outline it, is inferential. We cannot trace the development of these principles, much less can we say who discovered them. Some of them, as already suggested, are man's heritage from non-human ancestors. Others can only have been grasped by him after he had reached a relatively high stage of human development. But all the principles here listed must surely have been parts of our primitive ancestor's knowledge before those earliest days of Egyptian and Babylonian civilization, the records of which constitute our first introduction to the so-called historical period. Taken somewhat in the order of their probable discovery, the scientific ideas of primitive man may be roughly listed as follows:

1. Primitive man must have conceived that the earth is flat and of limitless extent. By this it is not meant to imply that he had a distinct conception of infinity, but, for that matter, it cannot be said that any one to-day has a conception of infinity that could be called definite. But, reasoning from experience and the reports of travellers, there was nothing to suggest to early man the limit of the earth. He did, indeed, find in his wanderings, that changed climatic conditions barred him from farther progress; but beyond the farthest reaches of his migrations, the seemingly flat land-surfaces and water-surfaces stretched away unbroken and, to all appearances, without end. It would require a reach of the philosophical imagination to conceive a limit to the earth, and while such imaginings may have been current in the prehistoric period, we can have no proof of them, and we may well postpone consideration of man's early dreamings as to the shape of the earth until we enter the historical epoch where we stand on firm ground.

2. Primitive man must, from a very early period, have observed that the sun gives heat and light, and that the moon and stars seem to give light only and no heat. It required but a slight extension of this observation to note that the changing phases of the seasons were associated with the seeming approach and recession of the sun. This observation, however, could not have been made until man had migrated from the tropical regions, and had reached a stage of mechanical development enabling him to live in subtropical or temperate zones. Even then it is conceivable that a long period must have elapsed before a direct causal relation was felt to exist between the shifting of the sun and the shifting of the seasons; because, as every one knows, the periods of greatest heat in summer and greatest cold in winter usually come some weeks after the time of the solstices. Yet, the fact that these extremes of temperature are associated in some way with the change of the sun's place in the heavens must, in time, have impressed itself upon even a rudimentary intelligence. It is hardly necessary to add that this is not meant to imply any definite knowledge of the real meaning of, the seeming oscillations of the sun. We shall see that, even at a relatively late period, the vaguest notions were still in vogue as to the cause of the sun's changes of position.

That the sun, moon, and stars move across the heavens must obviously have been among the earliest scientific observations. It must not be inferred, however, that this observation implied a necessary conception of the complete revolution of these bodies about the earth. It is unnecessary

to speculate here as to how the primitive intelligence conceived the transfer of the sun from the western to the eastern horizon, to be effected each night, for we shall have occasion to examine some historical speculations regarding this phenomenon. We may assume, however, that the idea of the transfer of the heavenly bodies beneath the earth (whatever the conception as to the form of that body) must early have presented itself.

It required a relatively high development of the observing faculties, yet a development which man must have attained ages before the historical period, to note that the moon has a secondary motion, which leads it to shift its relative position in the heavens, as regards the stars; that the stars themselves, on the other hand, keep a fixed relation as regards one another, with the notable exception of two or three of the most brilliant members of the galaxy, the latter being the bodies which came to be known finally as planets, or wandering stars. The wandering propensities of such brilliant bodies as Jupiter and Venus cannot well have escaped detection. We may safely assume, however, that these anomalous motions of the moon and planets found no explanation that could be called scientific until a relatively late period.

3. Turning from the heavens to the earth, and ignoring such primitive observations as that of the distinction between land and water, we may note that there was one great scientific law which must have forced itself upon the attention of primitive man. This is the law of universal terrestrial gravitation. The word gravitation suggests the name of Newton, and it may excite surprise to hear a knowledge of gravitation ascribed to men who preceded that philosopher by, say, twenty-five or fifty thousand years. Yet the slightest consideration of the facts will make it clear that the great central law that all heavy bodies fall directly towards the earth, cannot have escaped the attention of the most primitive intelligence. The arboreal habits of our primitive ancestors gave opportunities for constant observation of the practicalities of this law. And, so soon as man had developed the mental capacity to formulate ideas, one of the earliest ideas must have been the conception, however vaguely phrased in words, that all unsupported bodies fall towards the earth. The same phenomenon being observed to operate on water-surfaces, and no alteration being observed in its operation in different portions of man's habitat, the most primitive wanderer must have come to have full faith in the universal action of the observed law of gravitation. Indeed, it is inconceivable that he can have imagined a place on the earth where this law does not operate. On the other hand, of course, he never grasped the conception of the operation of this law beyond the close proximity of the earth. To extend the reach of gravitation out to the moon and to the stars, including within its compass every particle of matter in the universe, was the work of Newton, as we shall see in due course. Meantime we shall better understand that work if we recall that the mere local fact of terrestrial gravitation has been the familiar knowledge of all generations of men. It may further help to connect us in sympathy with our primeval ancestor if we recall that in the attempt to explain this fact of terrestrial gravitation Newton made no advance, and we of to-day are scarcely more enlightened than the man of the Stone Age. Like the man of the Stone Age, we know that an arrow shot into the sky falls back to the earth. We can calculate, as he could not do, the arc it will describe and the exact speed of its fall; but as to why it returns to earth at all, the greatest philosopher of to-day is almost as much in the dark as was the first primitive bowman that ever made the experiment.

Other physical facts going to make up an elementary science of mechanics, that were demonstratively known to prehistoric man, were such as these: the rigidity of solids and the mobility of liquids; the fact that changes of temperature transform solids to liquids and vice

versa--that heat, for example, melts copper and even iron, and that cold congeals water; and the fact that friction, as illustrated in the rubbing together of two sticks, may produce heat enough to cause a fire. The rationale of this last experiment did not receive an explanation until about the beginning of the nineteenth century of our own era. But the experimental fact was so well known to prehistoric man that he employed this method, as various savage tribes employ it to this day, for the altogether practical purpose of making a fire; just as he employed his practical knowledge of the mutability of solids and liquids in smelting ores, in alloying copper with tin to make bronze, and in casting this alloy in molds to make various implements and weapons. Here, then, were the germs of an elementary science of physics. Meanwhile such observations as that of the solution of salt in water may be considered as giving a first lesson in chemistry, but beyond such altogether rudimentary conceptions chemical knowledge could not have gone--unless, indeed, the practical observation of the effects of fire be included; nor can this well be overlooked, since scarcely another single line of practical observation had a more direct influence in promoting the progress of man towards the heights of civilization.

4. In the field of what we now speak of as biological knowledge, primitive man had obviously the widest opportunity for practical observation. We can hardly doubt that man attained, at an early day, to that conception of identity and of difference which Plato places at the head of his metaphysical system. We shall urge presently that it is precisely such general ideas as these that were man's earliest inductions from observation, and hence that came to seem the most universal and "innate" ideas of his mentality. It is quite inconceivable, for example, that even the most rudimentary intelligence that could be called human could fail to discriminate between living things and, let us say, the rocks of the earth. The most primitive intelligence, then, must have made a tacit classification of the natural objects about it into the grand divisions of animate and inanimate nature. Doubtless the nascent scientist may have imagined life animating many bodies that we should call inanimate--such as the sun, wandering planets, the winds, and lightning; and, on the other hand, he may quite likely have relegated such objects as trees to the ranks of the non-living; but that he recognized a fundamental distinction between, let us say, a wolf and a granite bowlder we cannot well doubt. A step beyond this--a step, however, that may have required centuries or millenniums in the taking--must have carried man to a plane of intelligence from which a primitive Aristotle or Linnaeus was enabled to note differences and resemblances connoting such groups of things as fishes, birds, and furry beasts. This conception, to be sure, is an abstraction of a relatively high order. We know that there are savage races to-day whose language contains no word for such an abstraction as bird or tree. We are bound to believe, then, that there were long ages of human progress during which the highest man had attained no such stage of abstraction; but, on the other hand, it is equally little in question that this degree of mental development had been attained long before the opening of our historical period. The primeval man, then, whose scientific knowledge we are attempting to predicate, had become, through his conception of fishes, birds, and hairy animals as separate classes, a scientific zoologist of relatively high attainments.

In the practical field of medical knowledge, a certain stage of development must have been reached at a very early day. Even animals pick and choose among the vegetables about them, and at times seek out certain herbs quite different from their ordinary food, practising a sort of instinctive therapeutics. The cat's fondness for catnip is a case in point. The most primitive man, then, must have inherited a racial or instinctive knowledge of the medicinal effects of certain herbs; in particular he must have had such elementary knowledge of toxicology as would enable

him to avoid eating certain poisonous berries. Perhaps, indeed, we are placing the effect before the cause to some extent; for, after all, the animal system possesses marvellous powers of adaption, and there is perhaps hardly any poisonous vegetable which man might not have learned to eat without deleterious effect, provided the experiment were made gradually. To a certain extent, then, the observed poisonous effects of numerous plants upon the human system are to be explained by the fact that our ancestors have avoided this particular vegetable. Certain fruits and berries might have come to have been a part of man's diet, had they grown in the regions he inhabited at an early day, which now are poisonous to his system. This thought, however, carries us too far afield. For practical purposes, it suffices that certain roots, leaves, and fruits possess principles that are poisonous to the human system, and that unless man had learned in some way to avoid these, our race must have come to disaster. In point of fact, he did learn to avoid them; and such evidence implied, as has been said, an elementary knowledge of toxicology.

Coupled with this knowledge of things dangerous to the human system, there must have grown up, at a very early day, a belief in the remedial character of various vegetables as agents to combat disease. Here, of course, was a rudimentary therapeutics, a crude principle of an empirical art of medicine. As just suggested, the lower order of animals have an instinctive knowledge that enables them to seek out remedial herbs (though we probably exaggerate the extent of this instinctive knowledge); and if this be true, man must have inherited from his prehuman ancestors this instinct along with the others. That he extended this knowledge through observation and practice, and came early to make extensive use of drugs in the treatment of disease, is placed beyond cavil through the observation of the various existing barbaric tribes, nearly all of whom practice elaborate systems of therapeutics. We shall have occasion to see that even within historic times the particular therapeutic measures employed were often crude, and, as we are accustomed to say, unscientific; but even the crudest of them are really based upon scientific principles, inasmuch as their application implies the deduction of principles of action from previous observations. Certain drugs are applied to appease certain symptoms of disease because in the belief of the medicine-man such drugs have proved beneficial in previous similar cases.

All this, however, implies an appreciation of the fact that man is subject to "natural" diseases, and that if these diseases are not combated, death may result. But it should be understood that the earliest man probably had no such conception as this. Throughout all the ages of early development, what we call "natural" disease and "natural" death meant the onslaught of a tangible enemy. A study of this question leads us to some very curious inferences. The more we look into the matter the more the thought forces itself home to us that the idea of natural death, as we now conceive it, came to primitive man as a relatively late scientific induction. This thought seems almost startling, so axiomatic has the conception "man is mortal" come to appear. Yet a study of the ideas of existing savages, combined with our knowledge of the point of view from which historical peoples regard disease, make it more probable that the primitive conception of human life did not include the idea of necessary death. We are told that the Australian savage who falls from a tree and breaks his neck is not regarded as having met a natural death, but as having been the victim of the magical practices of the "medicine-man" of some neighboring tribe. Similarly, we shall find that the Egyptian and the Babylonian of the early historical period conceived illness as being almost invariably the result of the machinations of an enemy. One need but recall the superstitious observances of the Middle Ages, and the yet more recent belief in witchcraft, to realize how generally disease has been personified as a malicious agent invoked

by an unfriendly mind. Indeed, the phraseology of our present-day speech is still reminiscent of this; as when, for example, we speak of an "attack of fever," and the like.

When, following out this idea, we picture to ourselves the conditions under which primitive man lived, it will be evident at once how relatively infrequent must have been his observation of what we usually term natural death. His world was a world of strife; he lived by the chase; he saw animals kill one another; he witnessed the death of his own fellows at the hands of enemies. Naturally enough, then, when a member of his family was "struck down" by invisible agents, he ascribed this death also to violence, even though the offensive agent was concealed. Moreover, having very little idea of the lapse of time--being quite unaccustomed, that is, to reckon events from any fixed era--primitive man cannot have gained at once a clear conception of age as applied to his fellows. Until a relatively late stage of development made tribal life possible, it cannot have been usual for man to have knowledge of his grandparents; as a rule he did not know his own parents after he had passed the adolescent stage and had been turned out upon the world to care for himself. If, then, certain of his fellow-beings showed those evidences of infirmity which we ascribe to age, it did not necessarily follow that he saw any association between such infirmities and the length of time which those persons had lived. The very fact that some barbaric nations retain the custom of killing the aged and infirm, in itself suggests the possibility that this custom arose before a clear conception had been attained that such drags upon the community would be removed presently in the natural order of things. To a person who had no clear conception of the lapse of time and no preconception as to the limited period of man's life, the infirmities of age might very naturally be ascribed to the repeated attacks of those inimical powers which were understood sooner or later to carry off most members of the race. And coupled with this thought would go the conception that inasmuch as some people through luck had escaped the vengeance of all their enemies for long periods, these same individuals might continue to escape for indefinite periods of the future. There were no written records to tell primeval man of events of long ago. He lived in the present, and his sweep of ideas scarcely carried him back beyond the limits of his individual memory. But memory is observed to be fallacious. It must early have been noted that some people recalled events which other participants in them had quite forgotten, and it may readily enough have been inferred that those members of the tribe who spoke of events which others could not recall were merely the ones who were gifted with the best memories. If these reached a period when their memories became vague, it did not follow that their recollections had carried them back to the beginnings of their lives. Indeed, it is contrary to all experience to believe that any man remembers all the things he has once known, and the observed fallaciousness and evanescence of memory would thus tend to substantiate rather than to controvert the idea that various members of a tribe had been alive for an indefinite period.

Without further elaborating the argument, it seems a justifiable inference that the first conception primitive man would have of his own life would not include the thought of natural death, but would, conversely, connote the vague conception of endless life. Our own ancestors, a few generations removed, had not got rid of this conception, as the perpetual quest of the spring of eternal youth amply testifies. A naturalist of our own day has suggested that perhaps birds never die except by violence. The thought, then, that man has a term of years beyond which "in the nature of things," as the saying goes, he may not live, would have dawned but gradually upon the developing intelligence of successive generations of men; and we cannot feel sure that he would fully have grasped the conception of a "natural" termination of human life until he had shaken

himself free from the idea that disease is always the result of the magic practice of an enemy. Our observation of historical man in antiquity makes it somewhat doubtful whether this conception had been attained before the close of the prehistoric period. If it had, this conception of the mortality of man was one of the most striking scientific inductions to which prehistoric man attained. Incidentally, it may be noted that the conception of eternal life for the human body being a more primitive idea than the conception of natural death, the idea of the immortality of the spirit would be the most natural of conceptions. The immortal spirit, indeed, would be but a correlative of the immortal body, and the idea which we shall see prevalent among the Egyptians that the soul persists only as long as the body is intact--the idea upon which the practice of mummifying the dead depended--finds a ready explanation. But this phase of the subject carries us somewhat afield. For our present purpose it suffices to have pointed out that the conception of man's mortality--a conception which now seems of all others the most natural and "innate"--was in all probability a relatively late scientific induction of our primitive ancestors.

5. Turning from the consideration of the body to its mental complement, we are forced to admit that here, also, our primitive man must have made certain elementary observations that underlie such sciences as psychology, mathematics, and political economy. The elementary emotions associated with hunger and with satiety, with love and with hatred, must have forced themselves upon the earliest intelligence that reached the plane of conscious self-observation. The capacity to count, at least to the number four or five, is within the range of even animal intelligence. Certain savages have gone scarcely farther than this; but our primeval ancestor, who was forging on towards civilization, had learned to count his fingers and toes, and to number objects about him by fives and tens in consequence, before be passed beyond the plane of numerous existing barbarians. How much beyond this he had gone we need not attempt to inquire; but the relatively high development of mathematics in the early historical period suggests that primeval man had attained a not inconsiderable knowledge of numbers. The humdrum vocation of looking after a numerous progeny must have taught the mother the rudiments of addition and subtraction; and the elements of multiplication and division are implied in the capacity to carry on even the rudest form of barter, such as the various tribes must have practised from an early day.

As to political ideas, even the crudest tribal life was based on certain conceptions of ownership, at least of tribal ownership, and the application of the principle of likeness and difference to which we have already referred. Each tribe, of course, differed in some regard from other tribes, and the recognition of these differences implied in itself a political classification. A certain tribe took possession of a particular hunting- ground, which became, for the time being, its home, and over which it came to exercise certain rights. An invasion of this territory by another tribe might lead to war, and the banding together of the members of the tribe to repel the invader implied both a recognition of communal unity and a species of prejudice in favor of that community that constituted a primitive patriotism. But this unity of action in opposing another tribe would not prevent a certain rivalry of interest between the members of the same tribe, which would show itself more and more prominently as the tribe increased in size. The association of two or more persons implies, always, the ascendency of some and the subordination of others. Leadership and subordination are necessary correlatives of difference of physical and mental endowment, and rivalry between leaders would inevitably lead to the formation of primitive political parties. With the ultimate success and ascendency of one leader, who secures either absolute power or power modified in accordance with the advice of subordinate leaders, we have the germs of an elaborate political system--an embryo science of government.

Meanwhile, the very existence of such a community implies the recognition on the part of its members of certain individual rights, the recognition of which is essential to communal harmony. The right of individual ownership of the various articles and implements of every-day life must be recognized, or all harmony would be at an end. Certain rules of justice-- primitive laws--must, by common consent, give protection to the weakest members of the community. Here are the rudiments of a system of ethics. It may seem anomalous to speak of this primitive morality, this early recognition of the principles of right and wrong, as having any relation to science. Yet, rightly considered, there is no incongruity in such a citation. There cannot well be a doubt that the adoption of those broad principles of right and wrong which underlie the entire structure of modern civilization was due to scientific induction,--in other words, to the belief, based on observation and experience, that the principles implied were essential to communal progress. He who has scanned the pageant of history knows how often these principles seem to be absent in the intercourse of men and nations. Yet the ideal is always there as a standard by which all deeds are judged.

It would appear, then, that the entire superstructure of later science had its foundation in the knowledge and practice of prehistoric man. The civilization of the historical period could not have advanced as it has had there not been countless generations of culture back of it. The new principles of science could not have been evolved had there not been great basal principles which ages of unconscious experiment had impressed upon the mind of our race. Due meed of praise must be given, then, to our primitive ancestor for his scientific accomplishments; but justice demands that we should look a little farther and consider the reverse side of the picture. We have had to do, thus far, chiefly with the positive side of accomplishment. We have pointed out what our primitive ancestor knew, intimating, perhaps, the limitations of his knowledge; but we have had little to say of one all-important feature of his scientific theorizing. The feature in question is based on the highly scientific desire and propensity to find explanations for the phenomena of nature. Without such desire no progress could be made. It is, as we have seen, the generalizing from experience that constitutes real scientific progress; and yet, just as most other good things can be overdone, this scientific propensity may be carried to a disastrous excess.

Primeval man did not escape this danger. He observed, he reasoned, he found explanations; but he did not always discriminate as to the logicality of his reasonings. He failed to recognize the limitations of his knowledge. The observed uniformity in the sequence of certain events impressed on his mind the idea of cause and effect. Proximate causes known, he sought remoter causes; childlike, his inquiring mind was always asking, Why? and, childlike, he demanded an explicit answer. If the forces of nature seemed to combat him, if wind and rain opposed his progress and thunder and lightning seemed to menace his existence, he was led irrevocably to think of those human foes who warred with him, and to see, back of the warfare of the elements, an inscrutable malevolent intelligence which took this method to express its displeasure. But every other line of scientific observation leads equally, following back a sequence of events, to seemingly causeless beginnings. Modern science can explain the lightning, as it can explain a great number of the mysteries which the primeval intelligence could not penetrate. But the primordial man could not wait for the revelations of scientific investigation: he must vault at once to a final solution of all scientific problems. He found his solution by peopling the world with invisible forces, anthropomorphic in their conception, like himself in their thought and action, differing only in the limitations of their powers. His own dream existence gave him seeming proof of the existence of an alter ego, a spiritual portion of himself that could dissever

itself from his body and wander at will; his scientific inductions seemed to tell him of a world of invisible beings, capable of influencing him for good or ill. From the scientific exercise of his faculties he evolved the all-encompassing generalizations of invisible and all-powerful causes back of the phenomena of nature. These generalizations, early developed and seemingly supported by the observations of countless generations, came to be among the most firmly established scientific inductions of our primeval ancestor. They obtained a hold upon the mentality of our race that led subsequent generations to think of them, sometimes to speak of them, as "innate" ideas. The observations upon which they were based are now, for the most part, susceptible of other interpretations; but the old interpretations have precedent and prejudice back of them, and they represent ideas that are more difficult than almost any others to eradicate. Always, and everywhere, superstitions based upon unwarranted early scientific deductions have been the most implacable foes to the progress of science. Men have built systems of philosophy around their conception of anthropomorphic deities; they have linked to these systems of philosophy the allied conception of the immutability of man's spirit, and they have asked that scientific progress should stop short at the brink of these systems of philosophy and accept their dictates as final. Yet there is not to-day in existence, and there never has been, one jot of scientific evidence for the existence of these intangible anthropomorphic powers back of nature that is not susceptible of scientific challenge and of more logical interpretation. In despite of which the superstitious beliefs are still as firmly fixed in the minds of a large majority of our race as they were in the mind of our prehistoric ancestor. The fact of this baleful heritage must not be forgotten in estimating the debt of gratitude which historic man owes to his barbaric predecessor.

II. EGYPTIAN SCIENCE

In the previous chapter we have purposely refrained from referring to any particular tribe or race of historical man. Now, however, we are at the beginnings of national existence, and we have to consider the accomplishments of an individual race; or rather, perhaps, of two or more races that occupied successively the same geographical territory. But even now our studies must for a time remain very general; we shall see little or nothing of the deeds of individual scientists in the course of our study of Egyptian culture. We are still, it must be understood, at the beginnings of history; indeed, we must first bridge over the gap from the prehistoric before we may find ourselves fairly on the line of march of historical science.

At the very outset we may well ask what constitutes the distinction between prehistoric and historic epochs --a distinction which has been constantly implied in much that we have said. The reply savors somewhat of vagueness. It is a distinction having to do, not so much with facts of human progress as with our interpretation of these facts. When we speak of the dawn of history we must not be understood to imply that, at the period in question, there was any sudden change in the intellectual status of the human race or in the status of any individual tribe or nation of men. What we mean is that modern knowledge has penetrated the mists of the past for the period we term historical with something more of clearness and precision than it has been able to bring to bear upon yet earlier periods. New accessions of knowledge may thus shift from time to time the bounds of the so-called historical period. The clearest illustration of this is furnished by our interpretation of Egyptian history. Until recently the biblical records of the Hebrew captivity or

service, together with the similar account of Josephus, furnished about all that was known of Egyptian history even of so comparatively recent a time as that of Ramses II. (fifteenth century B.C.), and from that period on there was almost a complete gap until the story was taken up by the Greek historians Herodotus and Diodorus. It is true that the king-lists of the Alexandrian historian, Manetho, were all along accessible in somewhat garbled copies. But at best they seemed to supply unintelligible lists of names and dates which no one was disposed to take seriously. That they were, broadly speaking, true historical records, and most important historical records at that, was not recognized by modern scholars until fresh light had been thrown on the subject from altogether new sources.

These new sources of knowledge of ancient history demand a moment's consideration. They are all-important because they have been the means of extending the historical period of Egyptian history (using the word history in the way just explained) by three or four thousand years. As just suggested, that historical period carried the scholarship of the early nineteenth century scarcely beyond the fifteenth century B.C., but to-day's vision extends with tolerable clearness to about the middle of the fifth millennium B.C. This change has been brought about chiefly through study of the Egyptian hieroglyphics. These hieroglyphics constitute, as we now know, a highly developed system of writing; a system that was practised for some thousands of years, but which fell utterly into disuse in the later Roman period, and the knowledge of which passed absolutely from the mind of man. For about two thousand years no one was able to read, with any degree of explicitness, a single character of this strange script, and the idea became prevalent that it did not constitute a real system of writing, but only a more or less barbaric system of religious symbolism. The falsity of this view was shown early in the nineteenth century when Dr. Thomas Young was led, through study of the famous trilingual inscription of the Rosetta stone, to make the first successful attempt at clearing up the mysteries of the hieroglyphics.

This is not the place to tell the story of his fascinating discoveries and those of his successors. That story belongs to nineteenth-century science, not to the science of the Egyptians. Suffice it here that Young gained the first clew to a few of the phonetic values of the Egyptian symbols, and that the work of discovery was carried on and vastly extended by the Frenchman Champollion, a little later, with the result that the firm foundations of the modern science of Egyptology were laid. Subsequently such students as Rosellini the Italian, Lepsius the German, and Wilkinson the Englishman, entered the field, which in due course was cultivated by De Rouge in France and Birch in England, and by such distinguished latter-day workers as Chabas, Mariette, Maspero, Amelineau, and De Morgan among the Frenchmen; Professor Petrie and Dr. Budge in England; and Brugsch Pasha and Professor Erman in Germany, not to mention a large coterie of somewhat less familiar names. These men working, some of them in the field of practical exploration, some as students of the Egyptian language and writing, have restored to us a tolerably precise knowledge of the history of Egypt from the time of the first historical king, Mena, whose date is placed at about the middle of the fifth century B.C. We know not merely the names of most of the subsequent rulers, but some thing of the deeds of many of them; and, what is vastly more important, we know, thanks to the modern interpretation of the old literature, many things concerning the life of the people, and in particular concerning their highest culture, their methods of thought, and their scientific attainments, which might well have been supposed to be past finding out. Nor has modern investigation halted with the time of the first kings; the recent explorations of such archaeologists as Amelineau, De Morgan, and Petrie have brought to light numerous remains of what is now spoken of as the predynastic period--a period when the

inhabitants of the Nile Valley used implements of chipped stone, when their pottery was made without the use of the potter's wheel, and when they buried their dead in curiously cramped attitudes without attempt at mummification. These aboriginal inhabitants of Egypt cannot perhaps with strict propriety be spoken of as living within the historical period, since we cannot date their relics with any accuracy. But they give us glimpses of the early stages of civilization upon which the Egyptians of the dynastic period were to advance.

It is held that the nascent civilization of these Egyptians of the Neolithic, or late Stone Age, was overthrown by the invading hosts of a more highly civilized race which probably came from the East, and which may have been of a Semitic stock. The presumption is that this invading people brought with it a knowledge of the arts of war and peace, developed or adopted in its old home. The introduction of these arts served to bridge somewhat suddenly, so far as Egypt is concerned, that gap between the prehistoric and the historic stage of culture to which we have all along referred. The essential structure of that bridge, let it now be clearly understood, consisted of a single element. That element is the capacity to make written records: a knowledge of the art of writing. Clearly understood, it is this element of knowledge that forms the line bounding the historical period. Numberless mementos are in existence that tell of the intellectual activities of prehistoric man; such mementos as flint implements, pieces of pottery, and fragments of bone, inscribed with pictures that may fairly be spoken of as works of art; but so long as no written word accompanies these records, so long as no name of king or scribe comes down to us, we feel that these records belong to the domain of archaeology rather than to that of history. Yet it must be understood all along that these two domains shade one into the other and, it has already been urged, that the distinction between them is one that pertains rather to modern scholarship than to the development of civilization itself. Bearing this distinction still in mind, and recalling that the historical period, which is to be the field of our observation throughout the rest of our studies, extends for Egypt well back into the fifth millennium B.C., let us briefly review the practical phases of that civilization to which the Egyptian had attained before the beginning of the dynastic period. Since theoretical science is everywhere linked with the mechanical arts, this survey will give us a clear comprehension of the field that lies open for the progress of science in the long stages of historical time upon which we are just entering.

We may pass over such rudimentary advances in the direction of civilization as are implied in the use of articulate language, the application of fire to the uses of man, and the systematic making of dwellings of one sort or another, since all of these are stages of progress that were reached very early in the prehistoric period. What more directly concerns us is to note that a really high stage of mechanical development had been reached before the dawnings of Egyptian history proper. All manner of household utensils were employed; the potter's wheel aided in the construction of a great variety of earthen vessels; weaving had become a fine art, and weapons of bronze, including axes, spears, knives, and arrow-heads, were in constant use. Animals had long been domesticated, in particular the dog, the cat, and the ox; the horse was introduced later from the East. The practical arts of agriculture were practised almost as they are at the present day in Egypt, there being, of course, the same dependence then as now upon the inundations of the Nile.

As to government, the Egyptian of the first dynasty regarded his king as a demi-god to be actually deified after his death, and this point of view was not changed throughout the stages of later Egyptian history. In point of art, marvellous advances upon the skill of the prehistoric man had been made, probably in part under Asiatic influences, and that unique style of stilted yet

expressive drawing had come into vogue, which was to be remembered in after times as typically Egyptian. More important than all else, our Egyptian of the earliest historical period was in possession of the art of writing. He had begun to make those specific records which were impossible to the man of the Stone Age, and thus he had entered fully upon the way of historical progress which, as already pointed out, has its very foundation in written records. From now on the deeds of individual kings could find specific record. It began to be possible to fix the chronology of remote events with some accuracy; and with this same fixing of chronologies came the advent of true history. The period which precedes what is usually spoken of as the first dynasty in Egypt is one into which the present-day searcher is still able to see but darkly. The evidence seems to suggest than an invasion of relatively cultured people from the East overthrew, and in time supplanted, the Neolithic civilization of the Nile Valley. It is impossible to date this invasion accurately, but it cannot well have been later than the year 5000 B.C., and it may have been a great many centuries earlier than this. Be the exact dates what they may, we find the Egyptian of the fifth millennium B.C. in full possession of a highly organized civilization.

All subsequent ages have marvelled at the pyramids, some of which date from about the year 4000 B.C., though we may note in passing that these dates must not be taken too literally. The chronology of ancient Egypt cannot as yet be fixed with exact accuracy, but the disagreements between the various students of the subject need give us little concern. For our present purpose it does not in the least matter whether the pyramids were built three thousand or four thousand years before the beginning of our era. It suffices that they date back to a period long antecedent to the beginnings of civilization in Western Europe. They prove that the Egyptian of that early day had attained a knowledge of practical mechanics which, even from the twentieth-century point of view, is not to be spoken of lightly. It has sometimes been suggested that these mighty pyramids, built as they are of great blocks of stone, speak for an almost miraculous knowledge on the part of their builders; but a saner view of the conditions gives no warrant for this thought. Diodoras, the Sicilian, in his famous World's History, written about the beginning of our era, explains the building of the pyramids by suggesting that great quantities of earth were piled against the side of the rising structure to form an inclined plane up which the blocks of stone were dragged. He gives us certain figures, based, doubtless, on reports made to him by Egyptian priests, who in turn drew upon the traditions of their country, perhaps even upon written records no longer preserved. He says that one hundred and twenty thousand men were employed in the construction of the largest pyramid, and that, notwithstanding the size of this host of workers, the task occupied twenty years. We must not place too much dependence upon such figures as these, for the ancient historians are notoriously given to exaggeration in recording numbers; yet we need not doubt that the report given by Diodorus is substantially accurate in its main outlines as to the method through which the pyramids were constructed. A host of men putting their added weight and strength to the task, with the aid of ropes, pulleys, rollers, and levers, and utilizing the principle of the inclined plane, could undoubtedly move and elevate and place in position the largest blocks that enter into the pyramids or--what seems even more wonderful--the most gigantic obelisks, without the aid of any other kind of mechanism or of any more occult power. The same hands could, as Diodorus suggests, remove all trace of the debris of construction and leave the pyramids and obelisks standing in weird isolation, as if sprung into being through a miracle.

ASTRONOMICAL SCIENCE

It has been necessary to bear in mind these phases of practical civilization because much that we know of the purely scientific attainments of the Egyptians is based upon modern observation of their pyramids and temples. It was early observed, for example, that the pyramids are obviously oriented as regards the direction in which they face, in strict accordance with some astronomical principle. Early in the nineteenth century the Frenchman Biot made interesting studies in regard to this subject, and a hundred years later, in our own time, Sir Joseph Norman Lockyer, following up the work of various intermediary observers, has given the subject much attention, making it the central theme of his work on The Dawn of Astronomy.[1] Lockyer's researches make it clear that in the main the temples of Egypt were oriented with reference to the point at which the sun rises on the day of the summer solstice. The time of the solstice had peculiar interest for the Egyptians, because it corresponded rather closely with the time of the rising of the Nile. The floods of that river appear with very great regularity; the on-rushing tide reaches the region of Heliopolis and Memphis almost precisely on the day of the summer solstice. The time varies at different stages of the river's course, but as the civilization of the early dynasties centred at Memphis, observations made at this place had widest vogue.

Considering the all-essential character of the Nile floods-without which civilization would be impossible in Egypt--it is not strange that the time of their appearance should be taken as marking the beginning of a new year. The fact that their coming coincides with the solstice makes such a division of the calendar perfectly natural. In point of fact, from the earliest periods of which records have come down to us, the new year of the Egyptians dates from the summer solstice. It is certain that from the earliest historical periods the Egyptians were aware of the approximate length of the year. It would be strange were it otherwise, considering the ease with which a record of days could be kept from Nile flood to Nile flood, or from solstice to solstice. But this, of course, applies only to an approximate count. There is some reason to believe that in the earliest period the Egyptians made this count only 360 days. The fact that their year was divided into twelve months of thirty days each lends color to this belief; but, in any event, the mistake was discovered in due time and a partial remedy was applied through the interpolation of a "little month" of five days between the end of the twelfth month and the new year. This nearly but not quite remedied the matter. What it obviously failed to do was to take account of that additional quarter of a day which really rounds out the actual year.

It would have been a vastly convenient thing for humanity had it chanced that the earth had so accommodated its rotary motion with its speed of transit about the sun as to make its annual flight in precisely 360 days. Twelve lunar months of thirty days each would then have coincided exactly with the solar year, and most of the complexities of the calendar, which have so puzzled historical students, would have been avoided; but, on the other hand, perhaps this very simplicity would have proved detrimental to astronomical science by preventing men from searching the heavens as carefully as they have done. Be that as it may, the complexity exists. The actual year of three hundred and sixty-five and (about) one-quarter days cannot be divided evenly into months, and some such expedient as the intercalation of days here and there is essential, else the calendar will become absolutely out of harmony with the seasons.

In the case of the Egyptians, the attempt at adjustment was made, as just noted, by the introduction of the five days, constituting what the Egyptians themselves termed "the five days over and above the year." These so-called epagomenal days were undoubtedly introduced at a very early period. Maspero holds that they were in use before the first Thinite dynasty, citing in

evidence the fact that the legend of Osiris explains these days as having been created by the god Thot in order to permit Nuit to give birth to all her children; this expedient being necessary to overcome a ban which had been pronounced against Nuit, according to which she could not give birth to children on any day of the year. But, of course, the five additional days do not suffice fully to rectify the calendar. There remains the additional quarter of a day to be accounted for. This, of course, amounts to a full day every fourth year. We shall see that later Alexandrian science hit upon the expedient of adding a day to every fourth year; an expedient which the Julian calendar adopted and which still gives us our familiar leap-year. But, unfortunately, the ancient Egyptian failed to recognize the need of this additional day, or if he did recognize it he failed to act on his knowledge, and so it happened that, starting somewhere back in the remote past with a new year's day that coincided with the inundation of the Nile, there was a constantly shifting maladjustment of calendar and seasons as time went on.

The Egyptian seasons, it should be explained, were three in number: the season of the inundation, the season of the seed-time, and the season of the harvest; each season being, of course, four months in extent. Originally, as just mentioned, the season of the inundations began and coincided with the actual time of inundation. The more precise fixing of new year's day was accomplished through observation of the time of the so-called heliacal rising of the dog-star, Sirius, which bore the Egyptian name Sothis. It chances that, as viewed from about the region of Heliopolis, the sun at the time of the summer solstice occupies an apparent position in the heavens close to the dog-star. Now, as is well known, the Egyptians, seeing divinity back of almost every phenomenon of nature, very naturally paid particular reverence to so obviously influential a personage as the sun-god. In particular they thought it fitting to do homage to him just as he was starting out on his tour of Egypt in the morning; and that they might know the precise moment of his coming, the Egyptian astronomer priests, perched on the hill-tops near their temples, were wont to scan the eastern horizon with reference to some star which had been observed to precede the solar luminary. Of course the precession of the equinoxes, due to that axial wobble in which our clumsy earth indulges, would change the apparent position of the fixed stars in reference to the sun, so that the same star could not do service as heliacal messenger indefinitely; but, on the other hand, these changes are so slow that observations by many generations of astronomers would be required to detect the shifting. It is believed by Lockyer, though the evidence is not quite demonstrative, that the astronomical observations of the Egyptians date back to a period when Sothis, the dog-star, was not in close association with the sun on the morning of the summer solstice. Yet, according to the calculations of Biot, the heliacal rising of Sothis at the solstice was noted as early as the year 3285 B.C., and it is certain that this star continued throughout subsequent centuries to keep this position of peculiar prestige. Hence it was that Sothis came to be associated with Isis, one of the most important divinities of Egypt, and that the day in which Sothis was first visible in the morning sky marked the beginning of the new year; that day coinciding, as already noted, with the summer solstice and with the beginning of the Nile flow.

But now for the difficulties introduced by that unreckoned quarter of a day. Obviously with a calendar of 365 days only, at the end of four years, the calendar year, or vague year, as the Egyptians came to call it, had gained by one full day upon the actual solar year-- that is to say, the heliacal rising of Sothis, the dog- star, would not occur on new year's day of the faulty calendar, but a day later. And with each succeeding period of four years the day of heliacal rising, which marked the true beginning of the year--and which still, of course, coincided with

the inundation--would have fallen another day behind the calendar. In the course of 120 years an entire month would be lost; and in 480 years so great would become the shifting that the seasons would be altogether misplaced; the actual time of inundations corresponding with what the calendar registered as the seed-time, and the actual seed-time in turn corresponding with the harvest-time of the calendar.

At first thought this seems very awkward and confusing, but in all probability the effects were by no means so much so in actual practice. We need go no farther than to our own experience to know that the names of seasons, as of months and days, come to have in the minds of most of us a purely conventional significance. Few of us stop to give a thought to the meaning of the words January, February, etc., except as they connote certain climatic conditions. If, then, our own calendar were so defective that in the course of 120 years the month of February had shifted back to occupy the position of the original January, the change would have been so gradual, covering the period of two life-times or of four or five average generations, that it might well escape general observation.

Each succeeding generation of Egyptians, then, may not improbably have associated the names of the seasons with the contemporary climatic conditions, troubling themselves little with the thought that in an earlier age the climatic conditions for each period of the calendar were quite different. We cannot well suppose, however, that the astronomer priests were oblivious to the true state of things. Upon them devolved the duty of predicting the time of the Nile flood; a duty they were enabled to perform without difficulty through observation of the rising of the solstitial sun and its Sothic messenger. To these observers it must finally have been apparent that the shifting of the seasons was at the rate of one day in four years; this known, it required no great mathematical skill to compute that this shifting would finally effect a complete circuit of the calendar, so that after (4 X 365 =) 1460 years the first day of the calendar year would again coincide with the heliacal rising of Sothis and with the coming of the Nile flood. In other words, 1461 vague years or Egyptian calendar years Of 365 days each correspond to 1460 actual solar years of 365 1/4 days each. This period, measured thus by the heliacal rising of Sothis, is spoken of as the Sothic cycle.

To us who are trained from childhood to understand that the year consists of (approximately) 365 1/4 days, and to know that the calendar may be regulated approximately by the introduction of an extra day every fourth year, this recognition of the Sothic cycle seems simple enough. Yet if the average man of us will reflect how little he knows, of his own knowledge, of the exact length of the year, it will soon become evident that the appreciation of the faults of the calendar and the knowledge of its periodical adjustment constituted a relatively high development of scientific knowledge on the part of the Egyptian astronomer. It may be added that various efforts to reform the calendar were made by the ancient Egyptians, but that they cannot be credited with a satisfactory solution of the problem; for, of course, the Alexandrian scientists of the Ptolemaic period (whose work we shall have occasion to review presently) were not Egyptians in any proper sense of the word, but Greeks.

Since so much of the time of the astronomer priests was devoted to observation of the heavenly bodies, it is not surprising that they should have mapped out the apparent course of the moon and the visible planets in their nightly tour of the heavens, and that they should have divided the stars of the firmament into more or less arbitrary groups or constellations. That they did so is

evidenced by various sculptured representations of constellations corresponding to signs of the zodiac which still ornament the ceilings of various ancient temples. Unfortunately the decorative sense, which was always predominant with the Egyptian sculptor, led him to take various liberties with the distribution of figures in these representations of the constellations, so that the inferences drawn from them as to the exact map of the heavens as the Egyptians conceived it cannot be fully relied upon. It appears, however, that the Egyptian astronomer divided the zodiac into twenty-four decani, or constellations. The arbitrary groupings of figures, with the aid of which these are delineated, bear a close resemblance to the equally arbitrary outlines which we are still accustomed to use for the same purpose.

IDEAS OF COSMOLOGY

In viewing this astronomical system of the Egyptians one cannot avoid the question as to just what interpretation was placed upon it as regards the actual mechanical structure of the universe. A proximal answer to the question is supplied us with a good deal of clearness. It appears that the Egyptian conceived the sky as a sort of tangible or material roof placed above the world, and supported at each of its four corners by a column or pillar, which was later on conceived as a great mountain. The earth itself was conceived to be a rectangular box, longer from north to south than from east to west; the upper surface of this box, upon which man lived, being slightly concave and having, of course, the valley of the Nile as its centre. The pillars of support were situated at the points of the compass; the northern one being located beyond the Mediterranean Sea; the southern one away beyond the habitable regions towards the source of the Nile, and the eastern and western ones in equally inaccessible regions. Circling about the southern side of the, world was a great river suspended in mid-air on something comparable to mountain cliffs; on which river the sun-god made his daily course in a boat, fighting day by day his ever-recurring battle against Set, the demon of darkness. The wide channel of this river enabled the sun-god to alter his course from time to time, as he is observed to do; in winter directing his bark towards the farther bank of the channel; in summer gliding close to the nearer bank. As to the stars, they were similar lights, suspended from the vault of the heaven; but just how their observed motion of translation across the heavens was explained is not apparent. It is more than probable that no one explanation was, universally accepted.

In explaining the origin of this mechanism of the heavens, the Egyptian imagination ran riot. Each separate part of Egypt had its own hierarchy of gods, and more or less its own explanations of cosmogony. There does not appear to have been any one central story of creation that found universal acceptance, any more than there was one specific deity everywhere recognized as supreme among the gods. Perhaps the most interesting of the cosmogonic myths was that which conceived that Nuit, the goddess of night, had been torn from the arms of her husband, Sibu the earth-god, and elevated to the sky despite her protests and her husband's struggles, there to remain supported by her four limbs, which became metamorphosed into the pillars, or mountains, already mentioned. The forcible elevation of Nuit had been effected on the day of creation by a new god, Shu, who came forth from the primeval waters. A painting on the mummy case of one Betuhamon, now in the Turin Museum, illustrates, in the graphic manner so characteristic of the Egyptians, this act of creation. As Maspero[2] points out, the struggle of Sibu resulted in contorted attitudes to which the irregularities of the earth's surface are to be ascribed.

In contemplating such a scheme of celestial mechanics as that just outlined, one cannot avoid raising the question as to just the degree of literalness which the Egyptians themselves put upon it. We know how essentially eye-minded the Egyptian was, to use a modern psychological phrase--that is to say, how essential to him it seemed that all his conceptions should be visualized. The evidences of this are everywhere: all his gods were made tangible; he believed in the immortality of the soul, yet he could not conceive of such immortality except in association with an immortal body; he must mummify the body of the dead, else, as he firmly believed, the dissolution of the spirit would take place along with the dissolution of the body itself. His world was peopled everywhere with spirits, but they were spirits associated always with corporeal bodies; his gods found lodgment in sun and moon and stars; in earth and water; in the bodies of reptiles and birds and mammals. He worshipped all of these things: the sun, the moon, water, earth, the spirit of the Nile, the ibis, the cat, the ram, and apis the bull; but, so far as we can judge, his imagination did not reach to the idea of an absolutely incorporeal deity. Similarly his conception of the mechanism of the heavens must be a tangibly mechanical one. He must think of the starry firmament as a substantial entity which could not defy the law of gravitation, and which, therefore, must have the same manner of support as is required by the roof of a house or temple. We know that this idea of the materiality of the firmament found elaborate expression in those later cosmological guesses which were to dominate the thought of Europe until the time of Newton. We need not doubt, therefore, that for the Egyptian this solid vault of the heavens had a very real existence. If now and then some dreamer conceived the great bodies of the firmament as floating in a less material plenum--and such iconoclastic dreamers there are in all ages--no record of his musings has come down to us, and we must freely admit that if such thoughts existed they were alien to the character of the Egyptian mind as a whole.

While the Egyptians conceived the heavenly bodies as the abiding-place of various of their deities, it does not appear that they practised astrology in the later acceptance of that word. This is the more remarkable since the conception of lucky and unlucky days was carried by the Egyptians to the extremes of absurdity. "One day was lucky or unlucky," says Erman,[3] "according as a good or bad mythological incident took place on that day. For instance, the 1st of Mechir, on which day the sky was raised, and the 27th of Athyr, when Horus and, Set concluded peace together and divided the world between them, were lucky days; on the other hand, the 14th of Tybi, on which Isis and Nephthys mourned for Osiris, was an unlucky day. With the unlucky days, which, fortunately, were less in number than the lucky days, they distinguished different degrees of ill-luck. Some were very unlucky, others only threatened ill-luck, and many, like the 17th and the 27th Choiakh, were partly good and partly bad according to the time of day. Lucky days might, as a rule, be disregarded. At most it might be as well to visit some specially renowned temple, or to 'celebrate a joyful day at home,' but no particular precautions were really necessary; and, above all, it was said, 'what thou also seest on the day is lucky.' It was quite otherwise with the unlucky and dangerous days, which imposed so many and such great limitations on people that those who wished to be prudent were always obliged to bear them in mind when determining on any course of action. Certain conditions were easy to carry out. Music and singing were to be avoided on the 14th Tybi, the day of the mourning of Osiris, and no one was allowed to wash on the 16th Tybi; whilst the name of Set might not be pronounced on the 24th of Pharmuthi. Fish was forbidden on certain days; and what was still more difficult in a country so rich in mice, on the 12th of Tybi no mouse might be seen. The most tiresome prohibitions, however, were those which occurred not infrequently, namely, those concerning work and going out: for instance, four times in Paophi the people had to 'do nothing at all,' and

five times to sit the whole day or half the day in the house; and the same rule had to be observed each month. It was impossible to rejoice if a child was born on the 23d of Thoth; the parents knew it could not live. Those born on the 20th of Choiakh would become blind, and those born on the 3d of Choiakh, deaf."

CHARMS AND INCANTATIONS

Where such conceptions as these pertained, it goes without saying that charms and incantations intended to break the spell of the unlucky omens were equally prevalent. Such incantations consisted usually of the recitation of certain phrases based originally, it would appear, upon incidents in the history of the gods. The words which the god had spoken in connection with some lucky incident would, it was thought, prove effective now in bringing good luck to the human supplicant--that is to say, the magician hoped through repeating the words of the god to exercise the magic power of the god. It was even possible, with the aid of the magical observances, partly to balk fate itself. Thus the person predestined through birth on an unlucky day to die of a serpent bite might postpone the time of this fateful visitation to extreme old age. The like uncertainty attached to those spells which one person was supposed to be able to exercise over another. It was held, for example, that if something belonging to an individual, such as a lock of hair or a paring of the nails, could be secured and incorporated in a waxen figure, this figure would be intimately associated with the personality of that individual. An enemy might thus secure occult power over one; any indignity practised upon the waxen figure would result in like injury to its human prototype. If the figure were bruised or beaten, some accident would overtake its double; if the image were placed over a fire, the human being would fall into a fever, and so on. But, of course, such mysterious evils as these would be met and combated by equally mysterious processes; and so it was that the entire art of medicine was closely linked with magical practices. It was not, indeed, held, according to Maspero, that the magical spells of enemies were the sole sources of human ailments, but one could never be sure to what extent such spells entered into the affliction; and so closely were the human activities associated in the mind of the Egyptian with one form or another of occult influences that purely physical conditions were at a discount. In the later times, at any rate, the physician was usually a priest, and there was a close association between the material and spiritual phases of therapeutics. Erman[4] tells us that the following formula had to be recited at the preparation of all medicaments: "That Isis might make free, make free. That Isis might make Horus free from all evil that his brother Set had done to him when he slew his father, Osiris. O Isis, great enchantress, free me, release me from all evil red things, from the fever of the god, and the fever of the goddess, from death and death from pain, and the pain which comes over me; as thou hast freed, as thou hast released thy son Horus, whilst I enter into the fire and come forth from the water," etc. Again, when the invalid took the medicine, an incantation had to be said which began thus: "Come remedy, come drive it out of my heart, out of these limbs strong in magic power with the remedy." He adds: "There may have been a few rationalists amongst the Egyptian doctors, for the number of magic formulae varies much in the different books. The book that we have specially taken for a foundation for this account of Egyptian medicine-- the great papyrus of the eighteenth dynasty edited by Ebers[5]--contains, for instance, far fewer exorcisms than some later writings with similar contents, probably because the doctor who compiled this book of recipes from older sources had very little liking for magic."

It must be understood, however--indeed, what has just been said implies as much--that the

physician by no means relied upon incantations alone; on the contrary, he equipped himself with an astonishing variety of medicaments. He had a particular fondness for what the modern physician speaks of as a "shot-gun" prescription--one containing a great variety of ingredients. Not only did herbs of many kinds enter into this, but such substances as lizard's blood, the teeth of swine, putrid meat, the moisture from pigs' ears, boiled horn, and numerous other even more repellent ingredients. Whoever is familiar with the formulae employed by European physicians even so recently as the eighteenth century will note a striking similarity here. Erman points out that the modern Egyptian even of this day holds closely to many of the practices of his remote ancestor. In particular, the efficacy of the beetle as a medicinal agent has stood the test of ages of practice. "Against all kinds of witchcraft," says an ancient formula, "a great scarabaeus beetle; cut off his head and wings, boil him; put him in oil and lay him out; then cook his head and wings, put them in snake fat, boil, and let the patient drink the mixture." The modern Egyptian, says Erman, uses almost precisely the same recipe, except that the snake fat is replaced by modern oil.

In evidence of the importance which was attached to practical medicine in the Egypt of an early day, the names of several physicians have come down to us from an age which has preserved very few names indeed, save those of kings. In reference to this Erman says[6]: "We still know the names of some of the early body physicians of this time; Sechmetna'eonch, 'chief physician of the Pharaoh,' and Nesmenan his chief, the 'superintendent of the physicians of the Pharaoh.' The priests also of the lioness-headed goddess Sechmet seem to have been famed for their medical wisdom, whilst the son of this goddess, the demi-god Imhotep, was in later times considered to be the creator of medical knowledge. These ancient doctors of the New Empire do not seem to have improved upon the older conceptions about the construction of the human body."

As to the actual scientific attainments of the Egyptian physician, it is difficult to speak with precision. Despite the cumbersome formulae and the grotesque incantations, we need not doubt that a certain practical value attended his therapeutics. He practised almost pure empiricism, however, and certainly it must have been almost impossible to determine which ones, if any, of the numerous ingredients of the prescription had real efficacy.

The practical anatomical knowledge of the physician, there is every reason to believe, was extremely limited. At first thought it might seem that the practice of embalming would have led to the custom of dissecting human bodies, and that the Egyptians, as a result of this, would have excelled in the knowledge of anatomy. But the actual results were rather the reverse of this. Embalming the dead, it must be recalled, was a purely religious observance. It took place under the superintendence of the priests, but so great was the reverence for the human body that the priests themselves were not permitted to make the abdominal incision which was a necessary preliminary of the process. This incision, as we are informed by both Herodotus[7] and Diodorus[8], was made by a special officer, whose status, if we may believe the explicit statement of Diodorus, was quite comparable to that of the modern hangman. The paraschistas, as he was called, having performed his necessary but obnoxious function, with the aid of a sharp Ethiopian stone, retired hastily, leaving the remaining processes to the priests. These, however, confined their observations to the abdominal viscera; under no consideration did they make other incisions in the body. It follows, therefore, that their opportunity for anatomical observations was most limited.

Since even the necessary mutilation inflicted on the corpse was regarded with such horror, it follows that anything in the way of dissection for a less sacred purpose was absolutely prohibited. Probably the same prohibition extended to a large number of animals, since most of these were held sacred in one part of Egypt or another. Moreover, there is nothing in what we know of the Egyptian mind to suggest the probability that any Egyptian physician would make extensive anatomical observations for the love of pure knowledge. All Egyptian science is eminently practical. If we think of the Egyptian as mysterious, it is because of the superstitious observances that we everywhere associate with his daily acts; but these, as we have already tried to make clear, were really based on scientific observations of a kind, and the attempt at true inferences from these observations. But whether or not the Egyptian physician desired anatomical knowledge, the results of his inquiries were certainly most meagre. The essentials of his system had to do with a series of vessels, alleged to be twenty-two or twenty-four in number, which penetrated the head and were distributed in pairs to the various members of the body, and which were vaguely thought of as carriers of water, air, excretory fluids, etc. Yet back of this vagueness, as must not be overlooked, there was an all-essential recognition of the heart as the central vascular organ. The heart is called the beginning of all the members. Its vessels, we are told, "lead to all the members; whether the doctor lays his finger on the forehead, on the back of the head, on the hands, on the place of the stomach (?), on the arms, or on the feet, everywhere he meets with the heart, because its vessels lead to all the members."[9] This recognition of the pulse must be credited to the Egyptian physician as a piece of practical knowledge, in some measure off-setting the vagueness of his anatomical theories.

ABSTRACT SCIENCE

But, indeed, practical knowledge was, as has been said over and over, the essential characteristic of Egyptian science. Yet another illustration of this is furnished us if we turn to the more abstract departments of thought and inquire what were the Egyptian attempts in such a field as mathematics. The answer does not tend greatly to increase our admiration for the Egyptian mind. We are led to see, indeed, that the Egyptian merchant was able to perform all the computations necessary to his craft, but we are forced to conclude that the knowledge of numbers scarcely extended beyond this, and that even here the methods of reckoning were tedious and cumbersome. Our knowledge of the subject rests largely upon the so- called papyrus Rhind,[10] which is a sort of mythological hand-book of the ancient Egyptians. Analyzing this document, Professor Erman concludes that the knowledge of the Egyptians was adequate to all practical requirements. Their mathematics taught them "how in the exchange of bread for beer the respective value was to be determined when converted into a quantity of corn; how to reckon the size of a field; how to determine how a given quantity of corn would go into a granary of a certain size," and like every-day problems. Yet they were obliged to make some of their simple computations in a very roundabout way. It would appear, for example, that their mental arithmetic did not enable them to multiply by a number larger than two, and that they did not reach a clear conception of complex fractional numbers. They did, indeed, recognize that each part of an object divided into 10 pieces became 1/10 of that object; they even grasped the idea of 2/3 this being a conception easily visualized; but they apparently did not visualize such a conception as 3/10 except in the crude form of 1/10 plus 1/10 plus 1/10. Their entire idea of division seems defective. They viewed the subject from the more elementary stand-point of multiplication. Thus, in order to find out how many times 7 is contained in 77, an existing example shows that the numbers representing 1 times 7, 2 times 7, 4 times 7, 8 times 7 were set

down successively and various experimental additions made to find out which sets of these numbers aggregated 77.

--1 7 --2 14 --4 28 --8 56

A line before the first, second, and fourth of these numbers indicated that it is necessary to multiply 7 by 1 plus 2 plus 8--that is, by 11, in order to obtain 77; that is to say, 7 goes 11 times in 77. All this seems very cumbersome indeed, yet we must not overlook the fact that the process which goes on in our own minds in performing such a problem as this is precisely similar, except that we have learned to slur over certain of the intermediate steps with the aid of a memorized multiplication table. In the last analysis, division is only the obverse side of multiplication, and any one who has not learned his multiplication table is reduced to some such expedient as that of the Egyptian. Indeed, whenever we pass beyond the range of our memorized multiplication table-which for most of us ends with the twelves--the experimental character of the trial multiplication through which division is finally effected does not so greatly differ from the experimental efforts which the Egyptian was obliged to apply to smaller numbers.

Despite his defective comprehension of fractions, the Egyptian was able to work out problems of relative complexity; for example, he could determine the answer of such a problem as this: a number together with its fifth part makes 21; what is the number? The process by which the Egyptian solved this problem seems very cumbersome to any one for whom a rudimentary knowledge of algebra makes it simple, yet the method which we employ differs only in that we are enabled, thanks to our hypothetical x, to make a short cut, and the essential fact must not be overlooked that the Egyptian reached a correct solution of the problem. With all due desire to give credit, however, the fact remains that the Egyptian was but a crude mathematician. Here, as elsewhere, it is impossible to admire him for any high development of theoretical science. First, last, and all the time, he was practical, and there is nothing to show that the thought of science for its own sake, for the mere love of knowing, ever entered his head.

In general, then, we must admit that the Egyptian had not progressed far in the hard way of abstract thinking. He worshipped everything about him because he feared the result of failing to do so. He embalmed the dead lest the spirit of the neglected one might come to torment him. Eye-minded as he was, he came to have an artistic sense, to love decorative effects. But he let these always take precedence over his sense of truth; as, for example, when he modified his lists of kings at Abydos to fit the space which the architect had left to be filled; he had no historical sense to show to him that truth should take precedence over mere decoration. And everywhere he lived in the same happy-go-lucky way. He loved personal ease, the pleasures of the table, the luxuries of life, games, recreations, festivals. He took no heed for the morrow, except as the morrow might minister to his personal needs. Essentially a sensual being, he scarcely conceived the meaning of the intellectual life in the modern sense of the term. He had perforce learned some things about astronomy, because these were necessary to his worship of the gods; about practical medicine, because this ministered to his material needs; about practical arithmetic, because this aided him in every-day affairs. The bare rudiments of an historical science may be said to be crudely outlined in his defective lists of kings. But beyond this he did not go. Science as science, and for its own sake, was unknown to him. He had gods for all material functions, and festivals in honor of every god; but there was no goddess of mere wisdom in his pantheon. The conception of Minerva was reserved for the creative genius of another people.

III. SCIENCE OF BABYLONIA AND ASSYRIA

Throughout classical antiquity Egyptian science was famous. We know that Plato spent some years in Egypt in the hope of penetrating the alleged mysteries of its fabled learning; and the story of the Egyptian priest who patronizingly assured Solon that the Greeks were but babes was quoted everywhere without disapproval. Even so late as the time of Augustus, we find Diodorus, the Sicilian, looking back with veneration upon the Oriental learning, to which Pliny also refers with unbounded respect. From what we have seen of Egyptian science, all this furnishes us with a somewhat striking commentary upon the attainments of the Greeks and Romans themselves. To refer at length to this would be to anticipate our purpose; what now concerns us is to recall that all along there was another nation, or group of nations, that disputed the palm for scientific attainments. This group of nations found a home in the valley of the Tigris and Euphrates. Their land was named Mesopotamia by the Greeks, because a large part of it lay between the two rivers just mentioned. The peoples themselves are familiar to every one as the Babylonians and the Assyrians. These peoples were of Semitic stock--allied, therefore, to the ancient Hebrews and Phoenicians and of the same racial stem with the Arameans and Arabs.

The great capital of the Babylonians during the later period of their history was the famed city of Babylon itself; the most famous capital of the Assyrians was Nineveh, that city to which, as every Bible- student will recall, the prophet Jonah was journeying when he had a much-exploited experience, the record of which forms no part of scientific annals. It was the kings of Assyria, issuing from their palaces in Nineveh, who dominated the civilization of Western Asia during the heyday of Hebrew history, and whose deeds are so frequently mentioned in the Hebrew chronicles. Later on, in the year 606 B.C., Nineveh was overthrown by the Medes[1] and Babylonians. The famous city was completely destroyed, never to be rebuilt. Babylon, however, though conquered subsequently by Cyrus and held in subjection by Darius,[2] the Persian kings, continued to hold sway as a great world-capital for some centuries. The last great historical event that occurred within its walls was the death of Alexander the Great, which took place there in the year 322 B.C.

In the time of Herodotus the fame of Babylon was at its height, and the father of history has left us a most entertaining account of what he saw when he visited the wonderful capital. Unfortunately, Herodotus was not a scholar in the proper acceptance of the term. He probably had no inkling of the Babylonian language, so the voluminous records of its literature were entirely shut off from his observation. He therefore enlightens us but little regarding the science of the Babylonians, though his observations on their practical civilization give us incidental references of no small importance. Somewhat more detailed references to the scientific attainments of the Babylonians are found in the fragments that have come down to us of the writings of the great Babylonian historian, Berosus,[3] who was born in Babylon about 330 B.C., and who was, therefore, a contemporary of Alexander the Great. But the writings of Berosus also, or at least such parts of them as have come down to us, leave very much to be desired in point of explicitness. They give some glimpses of Babylonian history, and they detail at some length the strange mythical tales of creation that entered into the Babylonian conception of cosmogony--details which find their counterpart in the allied recitals of the Hebrews. But taken all in all, the glimpses of the actual state of Chaldean[4] learning, as it was commonly called, amounted to scarcely more than vague wonder-tales. No one really knew just what interpretation to put upon these tales until the explorers of the nineteenth century had excavated the ruins of the

Babylonian and Assyrian cities, bringing to light the relics of their wonderful civilization. But these relics fortunately included vast numbers of written documents, inscribed on tablets, prisms, and cylinders of terra-cotta. When nineteenth-century scholarship had penetrated the mysteries of the strange script, and ferreted out the secrets of an unknown tongue, the world at last was in possession of authentic records by which the traditions regarding the Babylonians and Assyrians could be tested. Thanks to these materials, a new science commonly spoken of as Assyriology came into being, and a most important chapter of human history was brought to light. It became apparent that the Greek ideas concerning Mesopotamia, though vague in the extreme, were founded on fact. No one any longer questions that the Mesopotamian civilization was fully on a par with that of Egypt; indeed, it is rather held that superiority lay with the Asiatics. Certainly, in point of purely scientific attainments, the Babylonians passed somewhat beyond their Egyptian competitors. All the evidence seems to suggest also that the Babylonian civilization was even more ancient than that of Egypt. The precise dates are here in dispute; nor for our present purpose need they greatly concern us. But the Assyrio-Babylonian records have much greater historical accuracy as regards matters of chronology than have the Egyptian, and it is believed that our knowledge of the early Babylonian history is carried back, with some certainty, to King Sargon of Agade,[5] for whom the date 3800 B.C. is generally accepted; while somewhat vaguer records give us glimpses of periods as remote as the sixth, perhaps even the seventh or eighth millenniums before our era.

At a very early period Babylon itself was not a capital and Nineveh had not come into existence. The important cities, such as Nippur and Shirpurla, were situated farther to the south. It is on the site of these cities that the recent excavations have been made, such as those of the University of Pennsylvania expeditions at Nippur,[6] which are giving us glimpses into remoter recesses of the historical period.

Even if we disregard the more problematical early dates, we are still concerned with the records of a civilization extending unbroken throughout a period of about four thousand years; the actual period is in all probability twice or thrice that. Naturally enough, the current of history is not an unbroken stream throughout this long epoch. It appears that at least two utterly different ethnic elements are involved. A preponderance of evidence seems to show that the earliest civilized inhabitants of Mesopotamia were not Semitic, but an alien race, which is now commonly spoken of as Sumerian. This people, of whom we catch glimpses chiefly through the records of its successors, appears to have been subjugated or overthrown by Semitic invaders, who, coming perhaps from Arabia (their origin is in dispute), took possession of the region of the Tigris and Euphrates, learned from the Sumerians many of the useful arts, and, partly perhaps because of their mixed lineage, were enabled to develop the most wonderful civilization of antiquity. Could we analyze the details of this civilization from its earliest to its latest period we should of course find the same changes which always attend racial progress and decay. We should then be able, no doubt, to speak of certain golden epochs and their periods of decline. To a certain meagre extent we are able to do this now. We know, for example, that King Khammurabi, who lived about 2200 B.C., was a great law-giver, the ancient prototype of Justinian; and the epochs of such Assyrian kings as Sargon II., Asshurnazirpal, Sennacherib, and Asshurbanapal stand out with much distinctness. Yet, as a whole, the record does not enable us to trace with clearness the progress of scientific thought. At best we can gain fewer glimpses in this direction than in almost any other, for it is the record of war and conquest rather than of the peaceful arts that commanded the attention of the ancient scribe. So in dealing with the scientific achievements of

these peoples, we shall perforce consider their varied civilizations as a unity, and attempt, as best we may, to summarize their achievements as a whole. For the most part, we shall not attempt to discriminate as to what share in the final product was due to Sumerian, what to Babylonian, and what to Assyrian. We shall speak of Babylonian science as including all these elements; and drawing our information chiefly from the relatively late Assyrian and Babylonian sources, which, therefore, represent the culminating achievements of all these ages of effort, we shall attempt to discover what was the actual status of Mesopotamian science at its climax. In so far as we succeed, we shall be able to judge what scientific heritage Europe received from the Orient; for in the records of Babylonian science we have to do with the Eastern mind at its best. Let us turn to the specific inquiry as to the achievements of the Chaldean scientist whose fame so dazzled the eyes of his contemporaries of the classic world.

BABYLONIAN ASTRONOMY

Our first concern naturally is astronomy, this being here, as in Egypt, the first-born and the most important of the sciences. The fame of the Chaldean astronomer was indeed what chiefly commanded the admiration of the Greeks, and it was through the results of astronomical observations that Babylonia transmitted her most important influences to the Western world. "Our division of time is of Babylonian origin," says Hornmel;[7] "to Babylonia we owe the week of seven days, with the names of the planets for the days of the week, and the division into hours and months." Hence the almost personal interest which we of to-day must needs feel in the efforts of the Babylonian star-gazer.

It must not be supposed, however, that the Chaldean astronomer had made any very extraordinary advances upon the knowledge of the Egyptian "watchers of the night." After all, it required patient observation rather than any peculiar genius in the observer to note in the course of time such broad astronomical conditions as the regularity of the moon's phases, and the relation of the lunar periods to the longer periodical oscillations of the sun. Nor could the curious wanderings of the planets escape the attention of even a moderately keen observer. The chief distinction between the Chaldean and Egyptian astronomers appears to have consisted in the relative importance they attached to various of the phenomena which they both observed. The Egyptian, as we have seen, centred his attention upon the sun. That luminary was the abode of one of his most important gods. His worship was essentially solar. The Babylonian, on the other hand, appears to have been peculiarly impressed with the importance of the moon. He could not, of course, overlook the attention-compelling fact of the solar year; but his unit of time was the lunar period of thirty days, and his year consisted of twelve lunar periods, or 360 days. He was perfectly aware, however, that this period did not coincide with the actual year; but the relative unimportance which he ascribed to the solar year is evidenced by the fact that he interpolated an added month to adjust the calendar only once in six years. Indeed, it would appear that the Babylonians and Assyrians did not adopt precisely the same method of adjusting the calendar, since the Babylonians had two intercular months called Elul and Adar, whereas the Assyrians had only a single such month, called the second Adar.[8] (The Ve'Adar of the Hebrews.) This diversity further emphasizes the fact that it was the lunar period which received chief attention, the adjustment of this period with the solar seasons being a necessary expedient of secondary importance. It is held that these lunar periods have often been made to do service for years in the Babylonian computations and in the allied computations of the early Hebrews. The lives of the Hebrew patriarchs, for example, as recorded in the Bible, are perhaps reckoned in lunar "years."

Divided by twelve, the "years" of Methuselah accord fairly with the usual experience of mankind.

Yet, on the other hand, the convenience of the solar year in computing long periods of time was not unrecognized, since this period is utilized in reckoning the reigns of the Assyrian kings. It may be added that the reign of a king "was not reckoned from the day of his accession, but from the Assyrian new year's day, either before or after the day of accession. There does not appear to have been any fixed rule as to which new year's day should be chosen; but from the number of known cases, it appears to have been the general practice to count the reigning years from the new year's day nearest the accession, and to call the period between the accession day and the first new year's day 'the beginning of the reign,' when the year from the new year's day was called the first year, and the following ones were brought successively from it. Notwithstanding, in the dates of several Assyrian and Babylonian sovereigns there are cases of the year of accession being considered as the first year, thus giving two reckonings for the reigns of various monarchs, among others, Shalmaneser, Sennacherib, Nebuchadrezzar."[9] This uncertainty as to the years of reckoning again emphasizes the fact that the solar year did not have for the Assyrian chronology quite the same significance that it has for us.

The Assyrian month commenced on the evening when the new moon was first observed, or, in case the moon was not visible, the new month started thirty days after the last month. Since the actual lunar period is about twenty-nine and one-half days, a practical adjustment was required between the months themselves, and this was probably effected by counting alternate months as Only 29 days in length. Mr. R. Campbell Thompson[10] is led by his studies of the astrological tablets to emphasize this fact. He believes that "the object of the astrological reports which related to the appearance of the moon and sun was to help determine and foretell the length of the lunar month." Mr. Thompson believes also that there is evidence to show that the interculary month was added at a period less than six years. In point of fact, it does not appear to be quite clearly established as to precisely how the adjustment of days with the lunar months, and lunar months with the solar year, was effected. It is clear, however, according to Smith, "that the first 28 days of every month were divided into four weeks of seven days each; the seventh, fourteenth, twenty-first, twenty-eighth days respectively being Sabbaths, and that there was a general prohibition of work on these days." Here, of course, is the foundation of the Hebrew system of Sabbatical days which we have inherited. The sacredness of the number seven itself--the belief in which has not been quite shaken off even to this day --was deduced by the Assyrian astronomer from his observation of the seven planetary bodies--namely, Sin (the moon), Samas (the sun), Umunpawddu (Jupiter), Dilbat (Venus), Kaimanu (Saturn), Gudud (Mercury), Mustabarru-mutanu (Mars).[11] Twelve lunar periods, making up approximately the solar year, gave peculiar importance to the number twelve also. Thus the zodiac was divided into twelve signs which astronomers of all subsequent times have continued to recognize; and the duodecimal system of counting took precedence with the Babylonian mathematicians over the more primitive and, as it seems to us, more satisfactory decimal system.

Another discrepancy between the Babylonian and Egyptian years appears in the fact that the Babylonian new year dates from about the period of the vernal equinox and not from the solstice. Lockyer associates this with the fact that the periodical inundation of the Tigris and Euphrates occurs about the equinoctial period, whereas, as we have seen, the Nile flood comes at the time of the solstice. It is but natural that so important a phenomenon as the Nile flood should make a

strong impression upon the minds of a people living in a valley. The fact that occasional excessive inundations have led to most disastrous results is evidenced in the incorporation of stories of the almost total destruction of mankind by such floods among the myth tales of all peoples who reside in valley countries. The flooding of the Tigris and Euphrates had not, it is true, quite the same significance for the Mesopotamians that the Nile flood had for the Egyptians. Nevertheless it was a most important phenomenon, and may very readily be imagined to have been the most tangible index to the seasons. But in recognizing the time of the inundations and the vernal equinox, the Assyrians did not dethrone the moon from its accustomed precedence, for the year was reckoned as commencing not precisely at the vernal equinox, but at the new moon next before the equinox.

ASTROLOGY

Beyond marking the seasons, the chief interests that actuated the Babylonian astronomer in his observations were astrological. After quoting Diodorus to the effect that the Babylonian priests observed the position of certain stars in order to cast horoscopes, Thompson tells us that from a very early day the very name Chaldean became synonymous with magician. He adds that "from Mesopotamia, by way of Greece and Rome, a certain amount of Babylonian astrology made its way among the nations of the west, and it is quite probable that many superstitions which we commonly record as the peculiar product of western civilization took their origin from those of the early dwellers on the alluvial lands of Mesopotamia. One Assurbanipal, king of Assyria B.C. 668-626, added to the royal library at Nineveh his contribution of tablets, which included many series of documents which related exclusively to the astrology of the ancient Babylonians, who in turn had borrowed it with modifications from the Sumerian invaders of the country. Among these must be mentioned the series which was commonly called 'the Day of Bel,' and which was decreed by the learned to have been written in the time of the great Sargon I., king of Agade, 3800 B.C. With such ancient works as these to guide them, the profession of deducing omens from daily events reached such a pitch of importance in the last Assyrian Empire that a system of making periodical reports came into being. By these the king was informed of all the occurrences in the heavens and on earth, and the results of astrological studies in respect to after events. The heads of the astrological profession were men of high rank and position, and their office was hereditary. The variety of information contained in these reports is best gathered from the fact that they were sent from cities as far removed from each other as Assur in the north and Erech in the south, and it can only be assumed that they were despatched by runners, or men mounted on swift horses. As reports also came from Dilbat, Kutba, Nippur, and Bursippa, all cities of ancient foundation, the king was probably well acquainted with the general course of events in his empire."[12]

From certain passages in the astrological tablets, Thompson draws the interesting conclusion that the Chaldean astronomers were acquainted with some kind of a machine for reckoning time. He finds in one of the tablets a phrase which he interprets to mean measure-governor, and he infers from this the existence of a kind of a calculator. He calls attention also to the fact that Sextus Empiricus[13] states that the clepsydra was known to the Chaldeans, and that Herodotus asserts that the Greeks borrowed certain measures of time from the Babylonians. He finds further corroboration in the fact that the Babylonians had a time-measure by which they divided the day and the night; a measure called kasbu, which contained two hours. In a report relating to the day of the vernal equinox, it is stated that there are six kasbu of the day and six kasbu of the night.

While the astrologers deduced their omens from all the celestial bodies known to them, they chiefly gave attention to the moon, noting with great care the shape of its horns, and deducing such a conclusion as that "if the horns are pointed the king will overcome whatever he goreth," and that "when the moon is low at its appearance, the submission (of the people) of a far country will come."[14] The relations of the moon and sun were a source of constant observation, it being noted whether the sun and moon were seen together above the horizon; whether one set as the other rose, and the like. And whatever the phenomena, there was always, of course, a direct association between such phenomena and the well-being of human kind--in particular the king, at whose instance, and doubtless at whose expense, the observations were carried out.

From omens associated with the heavenly bodies it is but a step to omens based upon other phenomena of nature, and we, shall see in a moment that the Babylonian prophets made free use of their opportunities in this direction also. But before we turn from the field of astronomy, it will be well to inform ourselves as to what system the Chaldean astronomer had invented in explanation of the mechanics of the universe. Our answer to this inquiry is not quite as definite as could be desired, the vagueness of the records, no doubt, coinciding with the like vagueness in the minds of the Chaldeans themselves. So far as we can interpret the somewhat mystical references that have come down to us, however, the Babylonian cosmology would seem to have represented the earth as a circular plane surrounded by a great circular river, beyond which rose an impregnable barrier of mountains, and resting upon an infinite sea of waters. The material vault of the heavens was supposed to find support upon the outlying circle of mountains. But the precise mechanism through which the observed revolution of the heavenly bodies was effected remains here, as with the Egyptian cosmology, somewhat conjectural. The simple fact would appear to be that, for the Chaldeans as for the Egyptians, despite their most careful observations of the tangible phenomena of the heavens, no really satisfactory mechanical conception of the cosmos was attainable. We shall see in due course by what faltering steps the European imagination advanced from the crude ideas of Egypt and Babylonia to the relatively clear vision of Newton and Laplace.

CHALDEAN MAGIC

We turn now from the field of the astrologer to the closely allied province of Chaldean magic--a province which includes the other; which, indeed, is so all- encompassing as scarcely to leave any phase of Babylonian thought outside its bounds.

The tablets having to do with omens, exorcisms, and the like magic practices make up an astonishingly large proportion of the Babylonian records. In viewing them it is hard to avoid the conclusion that the superstitions which they evidenced absolutely dominated the life of the Babylonians of every degree. Yet it must not be forgotten that the greatest inconsistencies everywhere exist between the superstitious beliefs of a people and the practical observances of that people. No other problem is so difficult for the historian as that which confronts him when he endeavors to penetrate the mysteries of an alien religion; and when, as in the present case, the superstitions involved have been transmitted from generation to generation, their exact practical phases as interpreted by any particular generation must be somewhat problematical. The tablets upon which our knowledge of these omens is based are many of them from the libraries of the later kings of Nineveh; but the omens themselves are, in such cases, inscribed in the original Accadian form in which they have come down from remote ages, accompanied by an Assyrian

translation. Thus the superstitions involved had back of them hundreds of years, even thousands of years, of precedent; and we need not doubt that the ideas with which they are associated were interwoven with almost every thought and deed of the life of the people. Professor Sayce assures us that the Assyrians and Babylonians counted no fewer than three hundred spirits of heaven, and six hundred spirits of earth. "Like the Jews of the Talmud," he says, "they believed that the world was swarming with noxious spirits, who produced the various diseases to which man is liable, and might be swallowed with the food and drink which support life." Fox Talbot was inclined to believe that exorcisms were the exclusive means used to drive away the tormenting spirits. This seems unlikely, considering the uniform association of drugs with the magical practices among their people. Yet there is certainly a strange silence of the tablets in regard to medicine. Talbot tells us that sometimes divine images were brought into the sick-chamber, and written texts taken from holy books were placed on the walls and bound around the sick man's members. If these failed, recourse was had to the influence of the mamit, which the evil powers were unable to resist. On a tablet, written in the Accadian language only, the Assyrian version being taken, however, was found the following:

1. Take a white cloth. In it place the mamit, 2. in the sick man's right hand. 3. Take a black cloth, 4. wrap it around his left hand. 5. Then all the evil spirits (a long list of them is given) 6. and the sins which he has committed 7. shall quit their hold of him 8. and shall never return.

The symbolism of the black cloth in the left hand seems evident. The dying man repents of his former evil deeds, and he puts his trust in holiness, symbolized by the white cloth in his right hand. Then follow some obscure lines about the spirits:

1. Their heads shall remove from his head. 2. Their heads shall let go his hands. 3. Their feet shall depart from his feet.

Which perhaps may be explained thus: we learn from another tablet that the various classes of evil spirits troubled different parts of the body; some injured the head, some the hands and the feet, etc., therefore the passage before may mean "the spirits whose power is over the hand shall loose their hands from his," etc. "But," concludes Talbot, "I can offer no decided opinion upon such obscure points of their superstition."[15]

In regard to evil spirits, as elsewhere, the number seven had a peculiar significance, it being held that that number of spirits might enter into a man together. Talbot has translated[16] a "wild chant" which he names "The Song of the Seven Spirits."

1. There are seven! There are seven! 2. In the depths of the ocean there are seven! 3. In the heights of the heaven there are seven! 4. In the ocean stream in a palace they were born. 5. Male they are not: female they are not! 6. Wives they have not! Children are not born to them! 7. Rules they have not! Government they know not! 8. Prayers they hear not! 9. There are seven! There are seven! Twice over there are seven!

The tablets make frequent allusion to these seven spirits. One starts thus:

1. The god (---) shall stand by his bedside; 2. These seven evil spirits he shall root out and shall expel them from his body, 3. and these seven shall never return to the sick man again.[17]

Altogether similar are the exorcisms intended to ward off disease. Professor Sayce has published

translations of some of these.[18] Each of these ends with the same phrase, and they differ only in regard to the particular maladies from which freedom is desired. One reads:

"From wasting, from want of health, from the evil spirit of the ulcer, from the spreading quinsy of the gullet, from the violent ulcer, from the noxious ulcer, may the king of heaven preserve, may the king of earth preserve."

Another is phrased thus:

"From the cruel spirit of the head, from the strong spirit of the head, from the head spirit that departs not, from the head spirit that comes not forth, from the head spirit that will not go, from the noxious head spirit, may the king of heaven preserve, may the king of earth preserve."

As to omens having to do with the affairs of everyday life the number is legion. For example, Moppert has published, in the Journal Asiatique,[19] the translation of a tablet which contains on its two sides several scores of birth-portents, a few of which maybe quoted at random:

"When a woman bears a child and it has the ears of a lion, a strong king is in the country." "When a woman bears a child and it has a bird's beak, that country is oppressed." "When a woman bears a child and its right hand is wanting, that country goes to destruction." "When a woman bears a child and its feet are wanting, the roads of the country are cut; that house is destroyed." "When a woman bears a child and at the time of its birth its beard is grown, floods are in the country." "When a woman bears a child and at the time of its birth its mouth is open and speaks, there is pestilence in the country, the Air-god inundates the crops of the country, injury in the country is caused."

Some of these portents, it will be observed, are not in much danger of realization, and it is curious to surmise by what stretch of the imagination they can have been invented. There is, for example, on the same tablet just quoted, one reference which assures us that "when a sheep bears a lion the forces march multitudinously; the king has not a rival." There are other omens, however, that are so easy of realization as to lead one to suppose that any Babylonian who regarded all the superstitious signs must have been in constant terror. Thus a tablet translated by Professor Sayce[20] gives a long list of omens furnished by dogs, in which we are assured that:

1. If a yellow dog enters into the palace, exit from that palace will be baleful. 2. If a dog to the palace goes, and on a throne lies down, that palace is burned. 3. if a black dog into a temple enters, the foundation of that temple is not stable. 4. If female dogs one litter bear, destruction to the city.

It is needless to continue these citations, since they but reiterate endlessly the same story. It is interesting to recall, however, that the observations of animate nature, which were doubtless superstitious in their motive, had given the Babylonians some inklings of a knowledge of classification. Thus, according to Menant,[21] some of the tablets from Nineveh, which are written, as usual, in both the Sumerian and Assyrian languages, and which, therefore, like practically all Assyrian books, draw upon the knowledge of old Babylonia, give lists of animals, making an attempt at classification. The dog, lion, and wolf are placed in one category; the ox, sheep, and goat in another; the dog family itself is divided into various races, as the domestic dog, the coursing dog, the small dog, the dog of Elan, etc. Similar attempts at classification of

birds are found. Thus, birds of rapid flight, sea-birds, and marsh-birds are differentiated. Insects are classified according to habit; those that attack plants, animals, clothing, or wood. Vegetables seem to be classified according to their usefulness. One tablet enumerates the uses of wood according to its adaptability for timber-work of palaces, or construction of vessels, the making of implements of husbandry, or even furniture. Minerals occupy a long series in these tablets. They are classed according to their qualities, gold and silver occupying a division apart; precious stones forming another series. Our Babylonians, then, must be credited with the development of a rudimentary science of natural history.

BABYLONIAN MEDICINE

We have just seen that medical practice in the Babylonian world was strangely under the cloud of superstition. But it should be understood that our estimate, through lack of correct data, probably does much less than justice to the attainments of the physician of the time. As already noted, the existing tablets chance not to throw much light on the subject. It is known, however, that the practitioner of medicine occupied a position of some, authority and responsibility. The proof of this is found in the clauses relating to the legal status of the physician which are contained in the now famous code[22] of the Babylonian King Khamurabi, who reigned about 2300 years before our era. These clauses, though throwing no light on the scientific attainments of the physician of the period, are too curious to be omitted. They are clauses 215 to 227 of the celebrated code, and are as follows:

215. If a doctor has treated a man for a severe wound with a lancet of bronze and has cured the man, or has opened a tumor with a bronze lancet and has cured the man's eye, he shall receive ten shekels of silver.

216. If it was a freedman, he shall receive five shekels of silver.

217. If it was a man's slave, the owner of the slave shall give the doctor two shekels of silver.

218. If a physician has treated a free-born man for a severe wound with a lancet of bronze and has caused the man to die, or has opened a tumor of the man with a lancet of bronze and has destroyed his eye, his hands one shall cut off.

219. If the doctor has treated the slave of a freedman for a severe wound with a bronze lancet and has caused him to die, he shall give back slave for slave.

220. If he has opened his tumor with a bronze lancet and has ruined his eye, he shall pay the half of his price in money.

221. If a doctor has cured the broken limb of a man, or has healed his sick body, the patient shall pay the doctor five shekels of silver.

222. If it was a freedman, he shall give three shekels of silver.

223. If it was a man's slave, the owner of the slave shall give two shekels of silver to the doctor.

224. If the doctor of oxen and asses has treated an ox or an ass for a grave wound and has cured it, the owner of the ox or the ass shall give to the doctor as his pay one-sixth of a shekel of silver.

225. If he has treated an ox or an ass for a severe wound and has caused its death, he shall pay one-fourth of its price to the owner of the ox or the ass.

226. If a barber-surgeon, without consent of the owner of a slave, has branded the slave with an indelible mark, one shall cut off the hands of that barber.

227. If any one deceive the surgeon-barber and make him brand a slave with an indelible mark, one shall kill that man and bury him in his house. The barber shall swear, "I did not mark him wittingly," and he shall be guiltless.

ESTIMATES OF BABYLONIAN SCIENCE

Before turning from the Oriental world it is perhaps worth while to attempt to estimate somewhat specifically the world-influence of the name, Babylonian science. Perhaps we cannot better gain an idea as to the estimate put upon that science by the classical world than through a somewhat extended quotation from a classical author. Diodorus Siculus, who, as already noted, lived at about the time of Augustus, and who, therefore, scanned in perspective the entire sweep of classical Greek history, has left us a striking summary which is doubly valuable because of its comparisons of Babylonian with Greek influence. Having viewed the science of Babylonia in the light of the interpretations made possible by the recent study of original documents, we are prepared to draw our own conclusions from the statements of the Greek historian. Here is his estimate in the words of the quaint translation made by Philemon Holland in the year 1700:[23]

"They being the most ancient Babylonians, hold the same station and dignity in the Common-wealth as the Egyptian Priests do in Egypt: For being deputed to Divine Offices, they spend all their Time in the study of Philosophy, and are especially famous for the Art of Astrology. They are mightily given to Divination, and foretel future Events, and imploy themselves either by Purifications, Sacrifices, or other Inchantments to avert Evils, or procure good Fortune and Success. They are skilful likewise in the Art of Divination, by the flying of Birds, and interpreting of Dreams and Prodigies: And are reputed as true Oracles (in declaring what will come to pass) by their exact and diligent viewing the Intrals of the Sacrifices. But they attain not to this Knowledge in the same manner as the Grecians do; for the Chaldeans learn it by Tradition from their Ancestors, the Son from the Father, who are all in the mean time free from all other publick Offices and Attendances; and because their Parents are their Tutors, they both learn every thing without Envy, and rely with more confidence upon the truth of what is taught them; and being train'd up in this Learning, from their very Childhood, they become most famous Philosophers, (that Age being most capable of Learning, wherein they spend much of their time). But the Grecians for the most part come raw to this study, unfitted and unprepar'd, and are long before they attain to the Knowledge of this Philosophy: And after they have spent some small time in this Study, they are many times call'd off and forc'd to leave it, in order to get a Livelihood and Subsistence. And although some, few do industriously apply themselves to Philosophy, yet for the sake of Gain, these very Men are opinionative, and ever and anon starting new and high Points, and never fix in the steps of their Ancestors. But the Barbarians keeping constantly close to the same thing, attain to a perfect and distinct Knowledge in every particular.

"But the Grecians, cunningly catching at all Opportunities of Gain, make new Sects and Parties, and by their contrary Opinions wrangling and quarelling concerning the chiefest Points, lead their Scholars into a Maze; and being uncertain and doubtful what to pitch upon for certain truth,

their Minds are fluctuating and in suspence all the days of their Lives, and unable to give a certain assent unto any thing. For if any Man will but examine the most eminent Sects of the Philosophers, he shall find them much differing among themselves, and even opposing one another in the most weighty parts of their Philosophy. But to return to the Chaldeans, they hold that the World is eternal, which had neither any certain Beginning, nor shall have any End; but all agree, that all things are order'd, and this beautiful Fabrick is supported by a Divine Providence, and that the Motions of the Heavens are not perform'd by chance and of their own accord, but by a certain and determinate Will and Appointment of the Gods.

"Therefore from a long observation of the Stars, and an exact Knowledge of the motions and influences of every one of them, wherein they excel all others, they fortel many things that are to come to pass.

"They say that the Five Stars which some call Planets, but they Interpreters, are most worthy of Consideration, both for their motions and their remarkable influences, especially that which the Grecians call Saturn. The brightest of them all, and which often portends many and great Events, they call Sol, the other Four they name Mars, Venus, Mercury, and Jupiter, with our own Country Astrologers. They give the Name of Interpreters to these Stars, because these only by a peculiar Motion do portend things to come, and instead of Jupiters, do declare to Men before-hand the good- will of the Gods; whereas the other Stars (not being of the number of the Planets) have a constant ordinary motion. Future Events (they say) are pointed at sometimes by their Rising, and sometimes by their Setting, and at other times by their Colour, as may be experienc'd by those that will diligently observe it; sometimes foreshewing Hurricanes, at other times Tempestuous Rains, and then again exceeding Droughts. By these, they say, are often portended the appearance of Comets, Eclipses of the Sun and Moon, Earthquakes and all other the various Changes and remarkable effects in the Air, boding good and bad, not only to Nations in general, but to Kings and Private Persons in particular. Under the course of these Planets, they say are Thirty Stars, which they call Counselling Gods, half of whom observe what is done under the Earth, and the other half take notice of the actions of Men upon the Earth, and what is transacted in the Heavens. Once every Ten Days space (they say) one of the highest Order of these Stars descends to them that are of the lowest, like a Messenger sent from them above; and then again another ascends from those below to them above, and that this is their constant natural motion to continue for ever. The chief of these Gods, they say, are Twelve in number, to each of which they attribute a Month, and one Sign of the Twelve in the Zodiack.

"Through these Twelve Signs the Sun, Moon, and the other Five Planets run their Course. The Sun in a Years time, and the Moon in the space of a Month. To every one of the Planets they assign their own proper Courses, which are perform'd variously in lesser or shorter time according as their several motions are quicker or slower. These Stars, they say, have a great influence both as to good and bad in Mens Nativities; and from the consideration of their several Natures, may be foreknown what will befal Men afterwards. As they foretold things to come to other Kings formerly, so they did to Alexander who conquer'd Darius, and to his Successors Antigonus and Seleucus Nicator; and accordingly things fell out as they declar'd; which we shall relate particularly hereafter in a more convenient time. They tell likewise private Men their Fortunes so certainly, that those who have found the thing true by Experience, have esteem'd it a Miracle, and above the reach of man to perform. Out of the Circle of the Zodiack they describe Four and Twenty Stars, Twelve towards the North Pole, and as many to the South.

"Those which we see, they assign to the living; and the other that do not appear, they conceive are Constellations for the Dead; and they term them Judges of all things. The Moon, they say, is in the lowest Orb; and being therefore next to the Earth (because she is so small), she finishes her Course in a little time, not through the swiftness of her Motion, but the shortness of her Sphear. In that which they affirm (that she has but a borrow'd light, and that when she is eclips'd, it's caus'd by the interposition of the shadow of the Earth) they agree with the Grecians.

"Their Rules and Notions concerning the Eclipses of the Sun are but weak and mean, which they dare not positively foretel, nor fix a certain time for them. They have likewise Opinions concerning the Earth peculiar to themselves, affirming it to resemble a Boat, and to be hollow, to prove which, and other things relating to the frame of the World, they abound in Arguments; but to give a particular Account of 'em, we conceive would be a thing foreign to our History. But this any Man may justly and truly say, That the Chaldeans far exceed all other Men in the Knowledge of Astrology, and have study'd it most of any other Art or Science: But the number of years during which the Chaldeans say, those of their Profession have given themselves to the study of this natural Philosophy, is incredible; for when Alexander was in Asia, they reckon'd up Four Hundred and Seventy Thousand Years since they first began to observe the Motions of the Stars."

Let us now supplement this estimate of Babylonian influence with another estimate written in our own day, and quoted by one of the most recent historians of Babylonia and Assyria.[24] The estimate in question is that of Canon Rawlinson in his Great Oriental Monarchies.[25] Of Babylonia he says:

"Hers was apparently the genius which excogitated an alphabet; worked out the simpler problems of arithmetic; invented implements for measuring the lapse of time; conceived the idea of raising enormous structures with the poorest of all materials, clay; discovered the art of polishing, boring, and engraving gems; reproduced with truthfulness the outlines of human and animal forms; attained to high perfection in textile fabrics; studied with success the motions of the heavenly bodies; conceived of grammar as a science; elaborated a system of law; saw the value of an exact chronology--in almost every branch of science made a beginning, thus rendering it comparatively easy for other nations to proceed with the superstructure.... It was from the East, not from Egypt, that Greece derived her architecture, her sculpture, her science, her philosophy, her mathematical knowledge--in a word, her intellectual life. And Babylon was the source to which the entire stream of Eastern civilization may be traced. It is scarcely too much to say that, but for Babylon, real civilization might not yet have dawned upon the earth."

Considering that a period of almost two thousand years separates the times of writing of these two estimates, the estimates themselves are singularly in unison. They show that the greatest of Oriental nations has not suffered in reputation at the hands of posterity. It is indeed almost impossible to contemplate the monuments of Babylonian and Assyrian civilization that are now preserved in the European and American museums without becoming enthusiastic. That certainly was a wonderful civilization which has left us the tablets on which are inscribed the laws of a Khamurabi on the one hand, and the art treasures of the palace of an Asshurbanipal on the other. Yet a candid consideration of the scientific attainments of the Babylonians and Assyrians can scarcely arouse us to a like enthusiasm. In considering the subject we have seen that, so far as pure science is concerned, the efforts of the Babylonians and Assyrians chiefly centred about the

subjects of astrology and magic. With the records of their ghost-haunted science fresh in mind, one might be forgiven for a momentary desire to take issue with Canon Rawlinson's words. We are assured that the scientific attainments of Europe are almost solely to be credited to Babylonia and not to Egypt, but we should not forget that Plato, the greatest of the Greek thinkers, went to Egypt and not to Babylonia to pursue his studies when he wished to penetrate the secrets of Oriental science and philosophy. Clearly, then, classical Greece did not consider Babylonia as having a monopoly of scientific knowledge, and we of to-day, when we attempt to weigh the new evidence that has come to us in recent generations with the Babylonian records themselves, find that some, at least, of the heritages for which Babylonia has been praised are of more than doubtful value. Babylonia, for example, gave us our seven-day week and our system of computing by twelves. But surely the world could have got on as well without that magic number seven; and after some hundreds of generations we are coming to feel that the decimal system of the Egyptians has advantages over the duodecimal system of the Babylonians. Again, the Babylonians did not invent the alphabet; they did not even accept it when all the rest of the world had recognized its value. In grammar and arithmetic, as with astronomy, they seemed not to have advanced greatly, if at all, upon the Egyptians. One field in which they stand out in startling pre- eminence is the field of astrology; but this, in the estimate of modern thought, is the very negation of science. Babylonia impressed her superstitions on the Western world, and when we consider the baleful influence of these superstitions, we may almost question whether we might not reverse Canon Rawlinson's estimate and say that perhaps but for Babylonia real civilization, based on the application of true science, might have dawned upon the earth a score of centuries before it did. Yet, after all, perhaps this estimate is unjust. Society, like an individual organism, must creep before it can walk, and perhaps the Babylonian experiments in astrology and magic, which European civilization was destined to copy for some three or four thousand years, must have been made a part of the necessary evolution of our race in one place or in another. That thought, however, need not blind us to the essential fact, which the historian of science must needs admit, that for the Babylonian, despite his boasted culture, science spelled superstition.

IV. THE DEVELOPMENT OF THE ALPHABET

Before we turn specifically to the new world of the west, it remains to take note of what may perhaps be regarded as the very greatest achievement of ancient science. This was the analysis of speech sounds, and the resulting development of a system of alphabetical writing. To comprehend the series of scientific inductions which led to this result, we must go back in imagination and trace briefly the development of the methods of recording thought by means of graphic symbols. In other words, we must trace the evolution of the art of writing. In doing so we cannot hold to national lines as we have done in the preceding two chapters, though the efforts of the two great scientific nations just considered will enter prominently into the story.

The familiar Greek legend assures us that a Phoenician named Kadmus was the first to bring a knowledge of letters into Europe. An elaboration of the story, current throughout classical times, offered the further explanation that the Phoenicians had in turn acquired the art of writing from the Egyptians or Babylonians. Knowledge as to the true origin and development of the art of

writing did not extend in antiquity beyond such vagaries as these. Nineteenth-century studies gave the first real clews to an understanding of the subject. These studies tended to authenticate the essential fact on which the legend of Kadmus was founded; to the extent, at least, of making it probable that the later Grecian alphabet was introduced from Phoenicia--though not, of course, by any individual named Kadmus, the latter being, indeed, a name of purely Greek origin. Further studies of the past generation tended to corroborate the ancient belief as to the original source of the Phoenician alphabet, but divided scholars between two opinions: the one contending that the Egyptian hieroglyphics were the source upon which the Phoenicians drew; and the other contending with equal fervor that the Babylonian wedge character must be conceded that honor.

But, as has often happened in other fields after years of acrimonious controversy, a new discovery or two may suffice to show that neither contestant was right. After the Egyptologists of the school of De Rouge[1] thought they had demonstrated that the familiar symbols of the Phoenician alphabet had been copied from that modified form of Egyptian hieroglyphics known as the hieratic writing, the Assyriologists came forward to prove that certain characters of the Babylonian syllabary also show a likeness to the alphabetical characters that seemingly could not be due to chance. And then, when a settlement of the dispute seemed almost hopeless, it was shown through the Egyptian excavations that characters even more closely resembling those in dispute had been in use all about the shores of the Mediterranean, quite independently of either Egyptian or Assyrian writings, from periods so ancient as to be virtually prehistoric.

Coupled with this disconcerting discovery are the revelations brought to light by the excavations at the sites of Knossos and other long-buried cities of the island of Crete.[2] These excavations, which are still in progress, show that the art of writing was known and practised independently in Crete before that cataclysmic overthrow of the early Greek civilization which archaeologists are accustomed to ascribe to the hypothetical invasion of the Dorians. The significance of this is that the art of writing was known in Europe long before the advent of the mythical Kadmus. But since the early Cretan scripts are not to be identified with the scripts used in Greece in historical times, whereas the latter are undoubtedly of lineal descent from the Phoenician alphabet, the validity of the Kadmus legend, in a modified form, must still be admitted.

As has just been suggested, the new knowledge, particularly that which related to the great antiquity of characters similar to the Phoenician alphabetical signs, is somewhat disconcerting. Its general trend, however, is quite in the same direction with most of the new archaeological knowledge of recent decades---that is to say, it tends to emphasize the idea that human civilization in most of its important elaborations is vastly older than has hitherto been supposed. It may be added, however, that no definite clews are as yet available that enable us to fix even an approximate date for the origin of the Phoenician alphabet. The signs, to which reference has been made, may well have been in existence for thousands of years, utilized merely as property marks, symbols for counting and the like, before the idea of setting them aside as phonetic symbols was ever conceived. Nothing is more certain, in the judgment of the present-day investigator, than that man learned to write by slow and painful stages. It is probable that the conception of such an analysis of speech sounds as would make the idea of an alphabet possible came at a very late stage of social evolution, and as the culminating achievement of a long series of improvements in the art of writing. The precise steps that marked this path of intellectual development can for the most part be known only by inference; yet it is probable that the main

chapters of the story may be reproduced with essential accuracy.

FIRST STEPS

For the very first chapters of the story we must go back in imagination to the prehistoric period. Even barbaric man feels the need of self-expression, and strives to make his ideas manifest to other men by pictorial signs. The cave-dwellers scratched pictures of men and animals on the surface of a reindeer horn or mammoth tusk as mementos of his prowess. The American Indian does essentially the same thing to-day, making pictures that crudely record his successes in war and the chase. The Northern Indian had got no farther than this when the white man discovered America; but the Aztecs of the Southwest and the Maya people of Yucatan had carried their picture- making to a much higher state of elaboration.[3] They had developed systems of pictographs or hieroglyphics that would doubtless in the course of generations have been elaborated into alphabetical systems, had not the Europeans cut off the civilization of which they were the highest exponents.

What the Aztec and Maya were striving towards in the sixteenth century A.D., various Oriental nations had attained at least five or six thousand years earlier. In Egypt at the time of the pyramid-builders, and in Babylonia at the same epoch, the people had developed systems of writing that enabled them not merely to present a limited range of ideas pictorially, but to express in full elaboration and with finer shades of meaning all the ideas that pertain to highly cultured existence. The man of that time made records of military achievements, recorded the transactions of every-day business life, and gave expression to his moral and spiritual aspirations in a way strangely comparable to the manner of our own time. He had perfected highly elaborate systems of writing.

EGYPTIAN WRITING

Of the two ancient systems of writing just referred to as being in vogue at the so-called dawnings of history, the more picturesque and suggestive was the hieroglyphic system of the Egyptians. This is a curiously conglomerate system of writing, made up in part of symbols reminiscent of the crudest stages of picture-writing, in part of symbols having the phonetic value of syllables, and in part of true alphabetical letters. In a word, the Egyptian writing represents in itself the elements of the various stages through which the art of writing has developed.[4] We must conceive that new features were from time to time added to it, while the old features, curiously enough, were not given up.

Here, for example, in the midst of unintelligible lines and pot-hooks, are various pictures that are instantly recognizable as representations of hawks, lions, ibises, and the like. It can hardly be questioned that when these pictures were first used calligraphically they were meant to represent the idea of a bird or animal. In other words, the first stage of picture-writing did not go beyond the mere representation of an eagle by the picture of an eagle. But this, obviously, would confine the presentation of ideas within very narrow limits. In due course some inventive genius conceived the thought of symbolizing a picture. To him the outline of an eagle might represent not merely an actual bird, but the thought of strength, of courage, or of swift progress. Such a use of symbols obviously extends the range of utility of a nascent art of writing. Then in due course some wonderful psychologist--or perhaps the joint efforts of many generations of psychologists--made the astounding discovery that the human voice, which seems to flow on in an unbroken

stream of endlessly varied modulations and intonations, may really be analyzed into a comparatively limited number of component sounds--into a few hundreds of syllables. That wonderful idea conceived, it was only a matter of time until it would occur to some other enterprising genius that by selecting an arbitrary symbol to represent each one of these elementary sounds it would be possible to make a written record of the words of human speech which could be reproduced--rephonated--by some one who had never heard the words and did not know in advance what this written record contained. This, of course, is what every child learns to do now in the primer class, but we may feel assured that such an idea never occurred to any human being until the peculiar forms of pictographic writing just referred to had been practised for many centuries. Yet, as we have said, some genius of prehistoric Egypt conceived the idea and put it into practical execution, and the hieroglyphic writing of which the Egyptians were in full possession at the very beginning of what we term the historical period made use of this phonetic system along with the ideographic system already described.

So fond were the Egyptians of their pictorial symbols used ideographically that they clung to them persistently throughout the entire period of Egyptian history. They used symbols as phonetic equivalents very frequently, but they never learned to depend upon them exclusively. The scribe always interspersed his phonetic signs with some other signs intended as graphic aids. After spelling a word out in full, he added a picture, sometimes even two or three pictures, representative of the individual thing, or at least of the type of thing to which the word belongs. Two or three illustrations will make this clear.

Thus qeften, monkey, is spelled out in full, but the picture of a monkey is added as a determinative; second, qenu, cavalry, after being spelled, is made unequivocal by the introduction of a picture of a horse; third, temati, wings, though spelled elaborately, has pictures of wings added; and fourth, tatu, quadrupeds, after being spelled, has a picture of a quadruped, and then the picture of a hide, which is the usual determinative of a quadruped, followed by three dashes to indicate the plural number.

It must not be supposed, however, that it was a mere whim which led the Egyptians to the use of this system of determinatives. There was sound reason back of it. It amounted to no more than the expedient we adopt when we spell "to," "two," or "too," in indication of a single sound with three different meanings. The Egyptian language abounds in words having more than one meaning, and in writing these it is obvious that some means of distinction is desirable. The same thing occurs even more frequently in the Chinese language, which is monosyllabic. The Chinese adopt a more clumsy expedient, supplying a different symbol for each of the meanings of a syllable; so that while the actual word-sounds of their speech are only a few hundreds in number, the characters of their written language mount high into the thousands.

BABYLONIAN WRITING

While the civilization of the Nile Valley was developing this extraordinary system of hieroglyphics, the inhabitants of Babylonia were practising the art of writing along somewhat different lines. It is certain that they began with picture-making, and that in due course they advanced to the development of the syllabary; but, unlike their Egyptian cousins, the men of Babylonia saw fit to discard the old system when they had perfected a better one.[5] So at a very early day their writing--as revealed to us now through the recent excavations--had ceased to have that pictorial aspect which distinguishes the Egyptian script. What had originally been pictures of

objects--fish, houses, and the like--had come to be represented by mere aggregations of wedge-shaped marks. As the writing of the Babvlonians was chiefly inscribed on soft clay, the adaptation of this wedge-shaped mark in lieu of an ordinary line was probably a mere matter of convenience, since the sharp-cornered implement used in making the inscription naturally made a wedge-shaped impression in the clay. That, however, is a detail. The essential thing is that the Babylonian had so fully analyzed the speech-sounds that he felt entire confidence in them, and having selected a sufficient number of conventional characters--each made up of wedge-shaped lines--to represent all the phonetic sounds of his language, spelled the words out in syllables and to some extent dispensed with the determinative signs which, as we have seen, played so prominent a part in the Egyptian writing. His cousins the Assyrians used habitually a system of writing the foundation of which was an elaborate phonetic syllabary; a system, therefore, far removed from the old crude pictograph, and in some respects much more developed than the complicated Egyptian method; yet, after all, a system that stopped short of perfection by the wide gap that separates the syllabary from the true alphabet.

A brief analysis of speech sounds will aid us in understanding the real nature of the syllabary. Let us take for consideration the consonantal sound represented by the letter b. A moment's consideration will make it clear that this sound enters into a large number of syllables. There are, for example, at least twenty vowel sounds in the English language, not to speak of certain digraphs; that is to say, each of the important vowels has from two to six sounds. Each of these vowel sounds may enter into combination with the b sound alone to form three syllables; as ba, ab, bal, be, eb, bel, etc. Thus there are at least sixty b-sound syllables. But this is not the end, for other consonantal sounds may be associated in the syllables in such combinations as bad, bed, bar, bark, cab, etc. As each of the other twenty odd consonantal sounds may enter into similar combinations, it is obvious that there are several hundreds of fundamental syllables to be taken into account in any syllabic system of writing. For each of these syllables a symbol must be set aside and held in reserve as the representative of that particular sound. A perfect syllabary, then, would require some hundred or more of symbols to represent b sounds alone; and since the sounds for c, d, f, and the rest are equally varied, the entire syllabary would run into thousands of characters, almost rivalling in complexity the Chinese system. But in practice the most perfect syllabary, Such as that of the Babylonians, fell short of this degree of precision through ignoring the minor shades of sound; just as our own alphabet is content to represent some thirty vowel sounds by five letters, ignoring the fact that a, for example, has really half a dozen distinct phonetic values. By such slurring of sounds the syllabary is reduced far below its ideal limits; yet even so it retains three or four hundred characters.

In point of fact, such a work as Professor Delitzsch's Assyrian Grammar[6] presents signs for three hundred and thirty-four syllables, together with sundry alternative signs and determinatives to tax the memory of the would-be reader of Assyrian. Let us take for example a few of the b sounds. It has been explained that the basis of the Assyrian written character is a simple wedge-shaped or arrow-head mark. Variously repeated and grouped, these marks make up the syllabic characters.

To learn some four hundred such signs as these was the task set, as an equivalent of learning the a b c's, to any primer class in old Assyria in the long generations when that land was the culture Centre of the world. Nor was the task confined to the natives of Babylonia and Assyria alone. About the fifteenth century B.C., and probably for a long time before and after that period, the

exceedingly complex syllabary of the Babylonians was the official means of communication throughout western Asia and between Asia and Egypt, as we know from the chance discovery of a collection of letters belonging to the Egyptian king Khun-aten, preserved at Tel-el-Amarna. In the time of Ramses the Great the Babylonian writing was in all probability considered by a majority of the most highly civilized people in the world to be the most perfect script practicable. Doubtless the average scribe of the time did not in the least realize the waste of energy involved in his labors, or ever suspect that there could be any better way of writing.

Yet the analysis of any one of these hundreds of syllables into its component phonetic elements--had any one been genius enough to make such analysis-- ould have given the key to simpler and better things. But such an analysis was very hard to make, as the sequel shows. Nor is the utility of such an analysis self-evident, as the experience of the Egyptians proved. The vowel sound is so intimately linked with the consonant--the con-sonant, implying this intimate relation in its very name--that it seemed extremely difficult to give it individual recognition. To set off the mere labial beginning of the sound by itself, and to recognize it as an all-essential element of phonation, was the feat at which human intelligence so long balked. The germ of great things lay in that analysis. It was a process of simplification, and all art development is from the complex to the simple. Unfortunately, however, it did not seem a simplification, but rather quite the reverse. We may well suppose that the idea of wresting from the syllabary its secret of consonants and vowels, and giving to each consonantal sound a distinct sign, seemed a most cumbersome and embarrassing complication to the ancient scholars--that is to say, after the time arrived when any one gave such an idea expression. We can imagine them saying: "You will oblige us to use four signs instead of one to write such an elementary syllable as 'bard,' for example. Out upon such endless perplexity!" Nor is such a suggestion purely gratuitous, for it is an historical fact that the old syllabary continued to be used in Babylon hundreds of years after the alphabetical system had been introduced.[7] Custom is everything in establishing our prejudices. The Japanese to-day rebel against the introduction of an alphabet, thinking it ambiguous.

Yet, in the end, conservatism always yields, and so it was with opposition to the alphabet. Once the idea of the consonant had been firmly grasped, the old syllabary was doomed, though generations of time might be required to complete the obsequies--generations of time and the influence of a new nation. We have now to inquire how and by whom this advance was made.

THE ALPHABET ACHIEVED

We cannot believe that any nation could have vaulted to the final stage of the simple alphabetical writing without tracing the devious and difficult way of the pictograph and the syllabary. It is possible, however, for a cultivated nation to build upon the shoulders of its neighbors, and, profiting by the experience of others, to make sudden leaps upward and onward. And this is seemingly what happened in the final development of the art of writing. For while the Babylonians and Assyrians rested content with their elaborate syllabary, a nation on either side of them, geographically speaking, solved the problem, which they perhaps did not even recognize as a problem; wrested from their syllabary its secret of consonants and vowels, and by adopting an arbitrary sign for each consonantal sound, produced that most wonderful of human inventions, the alphabet.

The two nations credited with this wonderful achievement are the Phoenicians and the Persians. But it is not usually conceded that the two are entitled to anything like equal credit. The Persians,

probably in the time of Cyrus the Great, used certain characters of the Babylonian script for the construction of an alphabet; but at this time the Phoenician alphabet had undoubtedly been in use for some centuries, and it is more than probable that the Persian borrowed his idea of an alphabet from a Phoenician source. And that, of course, makes all the difference. Granted the idea of an alphabet, it requires no great reach of constructive genius to supply a set of alphabetical characters; though even here, it may be added parenthetically, a study of the development of alphabets will show that mankind has all along had a characteristic propensity to copy rather than to invent.

Regarding the Persian alphabet-maker, then, as a copyist rather than a true inventor, it remains to turn attention to the Phoenician source whence, as is commonly believed, the original alphabet which became "the mother of all existing alphabets" came into being. It must be admitted at the outset that evidence for the Phoenician origin of this alphabet is traditional rather than demonstrative. The Phoenicians were the great traders of antiquity; undoubtedly they were largely responsible for the transmission of the alphabet from one part of the world to another, once it had been invented. Too much credit cannot be given them for this; and as the world always honors him who makes an idea fertile rather than the originator of the idea, there can be little injustice in continuing to speak of the Phoenicians as the inventors of the alphabet. But the actual facts of the case will probably never be known. For aught we know, it may have been some dreamy-eyed Israelite, some Babylonian philosopher, some Egyptian mystic, perhaps even some obscure Cretan, who gave to the hard-headed Phoenician trader this conception of a dismembered syllable with its all-essential, elemental, wonder-working consonant. But it is futile now to attempt even to surmise on such unfathomable details as these. Suffice it that the analysis was made; that one sign and no more was adopted for each consonantal sound of the Semitic tongue, and that the entire cumbersome mechanism of the Egyptian and Babylonian writing systems was rendered obsolescent. These systems did not yield at once, to be sure; all human experience would have been set at naught had they done so. They held their own, and much more than held their own, for many centuries. After the Phoenicians as a nation had ceased to have importance; after their original script had been endlessly modified by many alien nations; after the original alphabet had made the conquest of all civilized Europe and of far outlying portions of the Orient--the Egyptian and Babylonian scribes continued to indite their missives in the same old pictographs and syllables.

The inventive thinker must have been struck with amazement when, after making the fullest analysis of speech-sounds of which he was capable, he found himself supplied with only a score or so of symbols. Yet as regards the consonantal sounds he had exhausted the resources of the Semitic tongue. As to vowels, he scarcely considered them at all. It seemed to him sufficient to use one symbol for each consonantal sound. This reduced the hitherto complex mechanism of writing to so simple a system that the inventor must have regarded it with sheer delight. On the other hand, the conservative scholar doubtless thought it distinctly ambiguous. In truth, it must be admitted that the system was imperfect. It was a vast improvement on the old syllabary, but it had its drawbacks. Perhaps it had been made a bit too simple; certainly it should have had symbols for the vowel sounds as well as for the consonants. Nevertheless, the vowel-lacking alphabet seems to have taken the popular fancy, and to this day Semitic people have never supplied its deficiencies save with certain dots and points.

Peoples using the Aryan speech soon saw the defect, and the Greeks supplied symbols for

several new sounds at a very early day.[8] But there the matter rested, and the alphabet has remained imperfect. For the purposes of the English language there should certainly have been added a dozen or more new characters. It is clear, for example, that, in the interest of explicitness, we should have a separate symbol for the vowel sound in each of the following syllables: bar, bay, bann, ball, to cite a single illustration.

There is, to be sure, a seemingly valid reason for not extending our alphabet, in the fact that in multiplying syllables it would be difficult to select characters at once easy to make and unambiguous. Moreover, the conservatives might point out, with telling effect, that the present alphabet has proved admirably effective for about three thousand years. Yet the fact that our dictionaries supply diacritical marks for some thirty vowels sounds to indicate the pronunciation of the words of our every-day speech, shows how we let memory and guessing do the work that might reasonably be demanded of a really complete alphabet. But, whatever its defects, the existing alphabet is a marvellous piece of mechanism, the result of thousands of years of intellectual effort. It is, perhaps without exception, the most stupendous invention of the human intellect within historical times--an achievement taking rank with such great prehistoric discoveries as the use of articulate speech, the making of a fire, and the invention of stone implements, of the wheel and axle, and of picture-writing. It made possible for the first time that education of the masses upon which all later progress of civilization was so largely to depend.

V. THE BEGINNINGS OF GREEK SCIENCE

Herodotus, the Father of History, tells us that once upon a time--which time, as the modern computator shows us, was about the year 590 B.C. --a war had risen between the Lydians and the Medes and continued five years. "In these years the Medes often discomfited the Lydians and the Lydians often discomfited the Medes (and among other things they fought a battle by night); and yet they still carried on the war with equally balanced fortitude. In the sixth year a battle took place in which it happened, when the fight had begun, that suddenly the day became night. And this change of the day Thales, the Milesian, had foretold to the Ionians, laying down as a limit this very year in which the change took place. The Lydians, however, and the Medes, when they saw that it had become night instead of day, ceased from their fighting and were much more eager, both of them, that peace should be made between them."

This memorable incident occurred while Alyattus, father of Croesus, was king of the Lydians. The modern astronomer, reckoning backward, estimates this eclipse as occurring probably May 25th, 585 B.C. The date is important as fixing a mile-stone in the chronology of ancient history, but it is doubly memorable because it is the first recorded instance of a predicted eclipse. Herodotus, who tells the story, was not born until about one hundred years after the incident occurred, but time had not dimmed the fame of the man who had performed the necromantic feat of prophecy. Thales, the Milesian, thanks in part at least to this accomplishment, had been known in life as first on the list of the Seven Wise Men of Greece, and had passed into history as the father of Greek philosophy. We may add that he had even found wider popular fame through being named by Hippolytus, and then by Father aesop, as the philosopher who, intent on studying the heavens, fell into a well; "whereupon," says Hippolytus, "a maid-servant named

Thratta laughed at him and said, 'In his search for things in the sky he does not see what is at his feet.' "

Such citations as these serve to bring vividly to mind the fact that we are entering a new epoch of thought. Hitherto our studies have been impersonal. Among Egyptians and Babylonians alike we have had to deal with classes of scientific records, but we have scarcely come across a single name. Now, however, we shall begin to find records of the work of individual investigators. In general, from now on, we shall be able to trace each great idea, if not to its originator, at least to some one man of genius who was prominent in bringing it before the world. The first of these vitalizers of thought, who stands out at the beginnings of Greek history, is this same Thales, of Miletus. His is not a very sharply defined personality as we look back upon it, and we can by no means be certain that all the discoveries which are ascribed to him are specifically his. Of his individuality as a man we know very little. It is not even quite certain as to where he was born; Miletus is usually accepted as his birthplace, but one tradition makes him by birth a Phenician. It is not at all in question, however, that by blood he was at least in part an Ionian Greek. It will be recalled that in the seventh century B.C., when Thales was born--and for a long time thereafter--the eastern shores of the aegean Sea were quite as prominently the centre of Greek influence as was the peninsula of Greece itself. Not merely Thales, but his followers and disciples, Anaximander and Anaximenes, were born there. So also was Herodotas, the Father of History, not to extend the list. There is nothing anomalous, then, in the fact that Thales, the father of Greek thought, was born and passed his life on soil that was not geographically a part of Greece; but the fact has an important significance of another kind. Thanks to his environment, Thales was necessarily brought more or less in contact with Oriental ideas. There was close commercial contact between the land of his nativity and the great Babylonian capital off to the east, as also with Egypt. Doubtless this association was of influence in shaping the development of Thales's mind. Indeed, it was an accepted tradition throughout classical times that the Milesian philosopher had travelled in Egypt, and had there gained at least the rudiments of his knowledge of geometry. In the fullest sense, then, Thales may be regarded as representing a link in the chain of thought connecting the learning of the old Orient with the nascent scholarship of the new Occident. Occupying this position, it is fitting that the personality of Thales should partake somewhat of mystery; that the scene may not be shifted too suddenly from the vague, impersonal East to the individualism of Europe.

All of this, however, must not be taken as casting any doubt upon the existence of Thales as a real person. Even the dates of his life--640 to 546 B.C.--may be accepted as at least approximately trustworthy; and the specific discoveries ascribed to him illustrate equally well the stage of development of Greek thought, whether Thales himself or one of his immediate disciples were the discoverer. We have already mentioned the feat which was said to have given Thales his great reputation. That Thales was universally credited with having predicted the famous eclipse is beyond question. That he actually did predict it in any precise sense of the word is open to doubt. At all events, his prediction was not based upon any such precise knowledge as that of the modern astronomer. There is, indeed, only one way in which he could have foretold the eclipse, and that is through knowledge of the regular succession of preceding eclipses. But that knowledge implies access on the part of some one to long series of records of practical observations of the heavens. Such records, as we have seen, existed in Egypt and even more notably in Babylonia. That these records were the source of the information which established the reputation of Thales is an unavoidable inference. In other words, the magical

prevision of the father of Greek thought was but a reflex of Oriental wisdom. Nevertheless, it sufficed to establish Thales as the father of Greek astronomy. In point of fact, his actual astronomical attainments would appear to have been meagre enough. There is nothing to show that he gained an inkling of the true character of the solar system. He did not even recognize the sphericity of the earth, but held, still following the Oriental authorities, that the world is a flat disk. Even his famous cosmogonic guess, according to which water is the essence of all things and the primordial element out of which the earth was developed, is but an elaboration of the Babylonian conception.

When we turn to the other field of thought with which the name of Thales is associated--namely, geometry--we again find evidence of the Oriental influence. The science of geometry, Herodotus assures us, was invented in Egypt. It was there an eminently practical science, being applied, as the name literally suggests, to the measurement of the earth's surface. Herodotus tells us that the Egyptians were obliged to cultivate the science because the periodical inundations washed away the boundary-lines between their farms. The primitive geometer, then, was a surveyor. The Egyptian records, as now revealed to us, show that the science had not been carried far in the land of its birth. The Egyptian geometer was able to measure irregular pieces of land only approximately. He never fully grasped the idea of the perpendicular as the true index of measurement for the triangle, but based his calculations upon measurements of the actual side of that figure. Nevertheless, he had learned to square the circle with a close approximation to the truth, and, in general, his measurement sufficed for all his practical needs. Just how much of the geometrical knowledge which added to the fame of Thales was borrowed directly from the Egyptians, and how much he actually created we cannot be sure. Nor is the question raised in disparagement of his genius. Receptivity is the first prerequisite to progressive thinking, and that Thales reached out after and imbibed portions of Oriental wisdom argues in itself for the creative character of his genius. Whether borrower of originator, however, Thales is credited with the expression of the following geometrical truths:

1. That the circle is bisected by its diameter.

2. That the angles at the base of an isosceles triangle are equal.

3. That when two straight lines cut each other the vertical opposite angles are equal.

4. That the angle in a semicircle is a right angle.

5. That one side and one acute angle of a right-angle triangle determine the other sides of the triangle.

It was by the application of the last of these principles that Thales is said to have performed the really notable feat of measuring the distance of a ship from the shore, his method being precisely the same in principle as that by which the guns are sighted on a modern man-of-war. Another practical demonstration which Thales was credited with making, and to which also his geometrical studies led him, was the measurement of any tall object, such as a pyramid or building or tree, by means of its shadow. The method, though simple enough, was ingenious. It consisted merely in observing the moment of the day when a perpendicular stick casts a shadow equal to its own length. Obviously the tree or monument would also cast a shadow equal to its own height at the same moment. It remains then but to measure the length of this shadow to

determine the height of the object. Such feats as this evidence the practicality of the genius of Thales. They suggest that Greek science, guided by imagination, was starting on the high-road of observation. We are told that Thales conceived for the first time the geometry of lines, and that this, indeed, constituted his real advance upon the Egyptians. We are told also that he conceived the eclipse of the sun as a purely natural phenomenon, and that herein lay his advance upon the Chaldean point of view. But if this be true Thales was greatly in advance of his time, for it will be recalled that fully two hundred years later the Greeks under Nicias before Syracuse were so disconcerted by the appearance of an eclipse, which was interpreted as a direct omen and warning, that Nicias threw away the last opportunity to rescue his army. Thucydides, it is true, in recording this fact speaks disparagingly of the superstitious bent of the mind of Nicias, but Thucydides also was a man far in advance of his time.

All that we know of the psychology of Thales is summed up in the famous maxim, "Know thyself," a maxim which, taken in connection with the proven receptivity of the philosopher's mind, suggests to us a marvellously rounded personality.

The disciples or successors of Thales, Anaximander and Anaximenes, were credited with advancing knowledge through the invention or introduction of the sundial. We may be sure, however, that the gnomon, which is the rudimentary sundial, had been known and used from remote periods in the Orient, and the most that is probable is that Anaximander may have elaborated some special design, possibly the bowl- shaped sundial, through which the shadow of the gnomon would indicate the time. The same philosopher is said to have made the first sketch of a geographical map, but this again is a statement which modern researches have shown to be fallacious, since a Babylonian attempt at depicting the geography of the world is still preserved to us on a clay tablet. Anaximander may, however, have been the first Greek to make an attempt of this kind. Here again the influence of Babylonian science upon the germinating Western thought is suggested.

It is said that Anaximander departed from Thales's conception of the earth, and, it may be added, from the Babylonian conception also, in that he conceived it as a cylinder, or rather as a truncated cone, the upper end of which is the habitable portion. This conception is perhaps the first of these guesses through which the Greek mind attempted to explain the apparent fixity of the earth. To ask what supports the earth in space is most natural, but the answer given by Anaximander, like that more familiar Greek solution which transformed the cone, or cylinder, into the giant Atlas, is but another illustration of that substitution of unwarranted inference for scientific induction which we have already so often pointed out as characteristic of the primitive stages of thought.

Anaximander held at least one theory which, as vouched for by various copyists and commentators, entitles him to be considered perhaps the first teacher of the idea of organic evolution. According to this idea, man developed from a fishlike ancestor, "growing up as sharks do until able to help himself and then coming forth on dry land."[1] The thought here expressed finds its germ, perhaps, in the Babylonian conception that everything came forth from a chaos of waters. Yet the fact that the thought of Anaximander has come down to posterity through such various channels suggests that the Greek thinker had got far enough away from the Oriental conception to make his view seem to his contemporaries a novel and individual one. Indeed, nothing we know of the Oriental line of thought conveys any suggestion of the idea of

transformation of species, whereas that idea is distinctly formulated in the traditional views of Anaximander.

VI. THE EARLY GREEK PHILOSOPHERS IN ITALY

Diogenes Laertius tells a story about a youth who, clad in a purple toga, entered the arena at the Olympian games and asked to compete with the other youths in boxing. He was derisively denied admission, presumably because he was beyond the legitimate age for juvenile contestants. Nothing daunted, the youth entered the lists of men, and turned the laugh on his critics by coming off victor. The youth who performed this feat was named Pythagoras. He was the same man, if we may credit the story, who afterwards migrated to Italy and became the founder of the famous Crotonian School of Philosophy; the man who developed the religion of the Orphic mysteries; who conceived the idea of the music of the spheres; who promulgated the doctrine of metempsychosis; who first, perhaps, of all men clearly conceived the notion that this world on which we live is a ball which moves in space and which may be habitable on every side.

A strange development that for a stripling pugilist. But we must not forget that in the Greek world athletics held a peculiar place. The chief winner of Olympian games gave his name to an epoch (the ensuing Olympiad of four years), and was honored almost before all others in the land. A sound mind in a sound body was the motto of the day. To excel in feats of strength and dexterity was an accomplishment that even a philosopher need not scorn. It will be recalled that aeschylus distinguished himself at the battle of Marathon; that Thucydides, the greatest of Greek historians, was a general in the Peloponnesian War; that Xenophon, the pupil and biographer of Socrates, was chiefly famed for having led the Ten Thousand in the memorable campaign of Cyrus the Younger; that Plato himself was credited with having shown great aptitude in early life as a wrestler. If, then, Pythagoras the philosopher was really the Pythagoras who won the boxing contest, we may suppose that in looking back upon this athletic feat from the heights of his priesthood--for he came to be almost deified--he regarded it not as an indiscretion of his youth, but as one of the greatest achievements of his life. Not unlikely he recalled with pride that he was credited with being no less an innovator in athletics than in philosophy. At all events, tradition credits him with the invention of "scientific" boxing. Was it he, perhaps, who taught the Greeks to strike a rising and swinging blow from the hip, as depicted in the famous metopes of the Parthenon? If so, the innovation of Pythagoras was as little heeded in this regard in a subsequent age as was his theory of the motion of the earth; for to strike a swinging blow from the hip, rather than from the shoulder, is a trick which the pugilist learned anew in our own day.

But enough of pugilism and of what, at best, is a doubtful tradition. Our concern is with another "science" than that of the arena. We must follow the purple-robed victor to Italy--if, indeed, we be not over-credulous in accepting the tradition--and learn of triumphs of a different kind that have placed the name of Pythagoras high on the list of the fathers of Grecian thought. To Italy? Yes, to the western limits of the Greek world. Here it was, beyond the confines of actual Greek territory, that Hellenic thought found its second home, its first home being, as we have seen, in Asia Minor. Pythagoras, indeed, to whom we have just been introduced, was born on the island of Samos, which lies near the coast of Asia Minor, but he probably migrated at an early day to

Crotona, in Italy. There he lived, taught, and developed his philosophy until rather late in life, when, having incurred the displeasure of his fellow-citizens, he suffered the not unusual penalty of banishment.

Of the three other great Italic leaders of thought of the early period, Xenophanes came rather late in life to Elea and founded the famous Eleatic School, of which Parmenides became the most distinguished ornament. These two were Ionians, and they lived in the sixth century before our era. Empedocles, the Sicilian, was of Doric origin. He lived about the middle of the fifth century B.C., at a time, therefore, when Athens had attained a position of chief glory among the Greek states; but there is no evidence that Empedocles ever visited that city, though it was rumored that he returned to the Peloponnesus to die. The other great Italic philosophers just named, living, as we have seen, in the previous century, can scarcely have thought of Athens as a centre of Greek thought. Indeed, the very fact that these men lived in Italy made that peninsula, rather than the mother-land of Greece, the centre of Hellenic influence. But all these men, it must constantly be borne in mind, were Greeks by birth and language, fully recognized as such in their own time and by posterity. Yet the fact that they lived in a land which was at no time a part of the geographical territory of Greece must not be forgotten. They, or their ancestors of recent generations, had been pioneers among those venturesome colonists who reached out into distant portions of the world, and made homes for themselves in much the same spirit in which colonists from Europe began to populate America some two thousand years later. In general, colonists from the different parts of Greece localized themselves somewhat definitely in their new homes; yet there must naturally have been a good deal of commingling among the various families of pioneers, and, to a certain extent, a mingling also with the earlier inhabitants of the country. This racial mingling, combined with the well-known vitalizing influence of the pioneer life, led, we may suppose, to a more rapid and more varied development than occurred among the home-staying Greeks. In proof of this, witness the remarkable schools of philosophy which, as we have seen, were thus developed at the confines of the Greek world, and which were presently to invade and, as it were, take by storm the mother-country itself.

As to the personality of these pioneer philosophers of the West, our knowledge is for the most part more or less traditional. What has been said of Thales may be repeated, in the main, regarding Pythagoras, Parmenides, and Empedocles. That they were real persons is not at all in question, but much that is merely traditional has come to be associated with their names. Pythagoras was the senior, and doubtless his ideas may have influenced the others more or less, though each is usually spoken of as the founder of an independent school. Much confusion has all along existed, however, as to the precise ideas which were to be ascribed to each of the leaders. Numberless commentators, indeed, have endeavored to pick out from among the traditions of antiquity, aided by such fragments, of the writing of the philosophers as have come down to us, the particular ideas that characterized each thinker, and to weave these ideas into systems. But such efforts, notwithstanding the mental energy that has been expended upon them, were, of necessity, futile, since, in the first place, the ancient philosophers themselves did not specialize and systematize their ideas according to modern notions, and, in the second place, the records of their individual teachings have been too scantily preserved to serve for the purpose of classification. It is freely admitted that fable has woven an impenetrable mesh of contradictions about the personalities of these ancient thinkers, and it would be folly to hope that this same artificer had been less busy with their beliefs and theories. When one reads that Pythagoras advocated an exclusively vegetable diet, yet that he was the first to train athletes on meat diet;

that he sacrificed only inanimate things, yet that he offered up a hundred oxen in honor of his great discovery regarding the sides of a triangle, and such like inconsistencies in the same biography, one gains a realizing sense of the extent to which diverse traditions enter into the story as it has come down to us. And yet we must reflect that most men change their opinions in the course of a long lifetime, and that the antagonistic reports may both be true.

True or false, these fables have an abiding interest, since they prove the unique and extraordinary character of the personality about which they are woven. The alleged witticisms of a Whistler, in our own day, were doubtless, for the most part, quite unknown to Whistler himself, yet they never would have been ascribed to him were they not akin to witticisms that he did originate--were they not, in short, typical expressions of his personality. And so of the heroes of the past. "It is no ordinary man," said George Henry Lewes, speaking of Pythagoras, "whom fable exalts into the poetic region. Whenever you find romantic or miraculous deeds attributed, be certain that the hero was great enough to maintain the weight of the crown of this fabulous glory."[1] We may not doubt, then, that Pythagoras, Parmenides, and Empedocles, with whose names fable was so busy throughout antiquity, were men of extraordinary personality. We are here chiefly concerned, however, neither with the personality of the man nor yet with the precise doctrines which each one of them taught. A knowledge of the latter would be interesting were it attainable, but in the confused state of the reports that have come down to us we cannot hope to be able to ascribe each idea with precision to its proper source. At best we can merely outline, even here not too precisely, the scientific doctrines which the Italic philosophers as a whole seem to have advocated.

First and foremost, there is the doctrine that the earth is a sphere. Pythagoras is said to have been the first advocate of this theory; but, unfortunately, it is reported also that Parmenides was its author. This rivalship for the discovery of an important truth we shall see repeated over and over in more recent times. Could we know the whole truth, it would perhaps appear that the idea of the sphericity of the earth was originated long before the time of the Greek philosophers. But it must be admitted that there is no record of any sort to give tangible support to such an assumption. So far as we can ascertain, no Egyptian or Babylonian astronomer ever grasped the wonderful conception that the earth is round. That the Italic Greeks should have conceived that idea was perhaps not so much because they were astronomers as because they were practical geographers and geometers. Pythagoras, as we have noted, was born at Samos, and, therefore, made a relatively long sea voyage in passing to Italy. Now, as every one knows, the most simple and tangible demonstration of the convexity of the earth's surface is furnished by observation of an approaching ship at sea. On a clear day a keen eye may discern the mast and sails rising gradually above the horizon, to be followed in due course by the hull. Similarly, on approaching the shore, high objects become visible before those that lie nearer the water. It is at least a plausible supposition that Pythagoras may have made such observations as these during the voyage in question, and that therein may lie the germ of that wonderful conception of the world as a sphere.

To what extent further proof, based on the fact that the earth's shadow when the moon is eclipsed is always convex, may have been known to Pythagoras we cannot say. There is no proof that any of the Italic philosophers made extensive records of astronomical observations as did the Egyptians and Babylonians; but we must constantly recall that the writings of classical antiquity have been almost altogether destroyed. The absence of astronomical records is, therefore, no

proof that such records never existed. Pythagoras, it should be said, is reported to have travelled in Egypt, and he must there have gained an inkling of astronomical methods. Indeed, he speaks of himself specifically, in a letter quoted by Diogenes, as one who is accustomed to study astronomy. Yet a later sentence of the letter, which asserts that the philosopher is not always occupied about speculations of his own fancy, suggesting, as it does, the dreamer rather than the observer, gives us probably a truer glimpse into the philosopher's mind. There is, indeed, reason to suppose that the doctrine of the sphericity of the earth appealed to Pythagoras chiefly because it accorded with his conception that the sphere is the most perfect solid, just as the circle is the most perfect plane surface. Be that as it may, the fact remains that we have here, as far as we can trace its origin, the first expression of the scientific theory that the earth is round. Had the Italic philosophers accomplished nothing more than this, their accomplishment would none the less mark an epoch in the progress of thought.

That Pythagoras was an observer of the heavens is further evidenced by the statement made by Diogenes, on the authority of Parmenides, that Pythagoras was the first person who discovered or asserted the identity of Hesperus and Lucifer--that is to say, of the morning and the evening star. This was really a remarkable discovery, and one that was no doubt instrumental later on in determining that theory of the mechanics of the heavens which we shall see elaborated presently. To have made such a discovery argues again for the practicality of the mind of Pythagoras. His, indeed, would seem to have been a mind in which practical common-sense was strangely blended with the capacity for wide and imaginative generalization. As further evidence of his practicality, it is asserted that he was the first person who introduced measures and weights among the Greeks, this assertion being made on the authority of Aristoxenus. It will be observed that he is said to have introduced, not to have invented, weights and measures, a statement which suggests a knowledge on the part of the Greeks that weights and measures were previously employed in Egypt and Babylonia.

The mind that could conceive the world as a sphere and that interested itself in weights and measures was, obviously, a mind of the visualizing type. It is characteristic of this type of mind to be interested in the tangibilities of geometry, hence it is not surprising to be told that Pythagoras "carried that science to perfection." The most famous discovery of Pythagoras in this field was that the square of the hypotenuse of a right-angled triangle is equal to the squares of the other sides of the triangle. We have already noted the fable that his enthusiasm over this discovery led him to sacrifice a hecatomb. Doubtless the story is apocryphal, but doubtless, also, it expresses the truth as to the fervid joy with which the philosopher must have contemplated the results of his creative imagination.

No line alleged to have been written by Pythagoras has come down to us. We are told that he refrained from publishing his doctrines, except by word of mouth. "The Lucanians and the Peucetians, and the Messapians and the Romans," we are assured, "flocked around him, coming with eagerness to hear his discourses; no fewer than six hundred came to him every night; and if any one of them had ever been permitted to see the master, they wrote of it to their friends as if they had gained some great advantage." Nevertheless, we are assured that until the time of Philolaus no doctrines of Pythagoras were ever published, to which statement it is added that "when the three celebrated books were published, Plato wrote to have them purchased for him for a hundred minas."[2] But if such books existed, they are lost to the modern world, and we are obliged to accept the assertions of relatively late writers as to the theories of the great Crotonian.

Perhaps we cannot do better than quote at length from an important summary of the remaining doctrines of Pythagoras, which Diogenes himself quoted from the work of a predecessor.[3] Despite its somewhat inchoate character, this summary is a most remarkable one, as a brief analysis of its contents will show. It should be explained that Alexander (whose work is now lost) is said to have found these dogmas set down in the commentaries of Pythagoras. If this assertion be accepted, we are brought one step nearer the philosopher himself. The summary is as follows:

"That the monad was the beginning of everything. From the monad proceeds an indefinite duad, which is subordinate to the monad as to its cause. That from the monad and the indefinite duad proceed numbers. And from numbers signs. And from these last, lines of which plane figures consist. And from plane figures are derived solid bodies. And from solid bodies sensible bodies, of which last there are four elements--fire, water, earth, and air. And that the world, which is indued with life and intellect, and which is of a spherical figure, having the earth, which is also spherical, and inhabited all over in its centre,[4] results from a combination of these elements, and derives its motion from them; and also that there are antipodes, and that what is below, as respects us, is above in respect of them.

"He also taught that light and darkness, and cold and heat, and dryness and moisture, were equally divided in the world; and that while heat was predominant it was summer; while cold had the mastery, it was winter; when dryness prevailed, it was spring; and when moisture preponderated, winter. And while all these qualities were on a level, then was the loveliest season of the year; of which the flourishing spring was the wholesome period, and the season of autumn the most pernicious one. Of the day, he said that the flourishing period was the morning, and the fading one the evening; on which account that also was the least healthy time.

"Another of his theories was that the air around the earth was immovable and pregnant with disease, and that everything in it was mortal; but that the upper air was in perpetual motion, and pure and salubrious, and that everything in that was immortal, and on that account divine. And that the sun and the moon and the stars were all gods; for in them the warm principle predominates which is the cause of life. And that the moon derives its light from the sun. And that there is a relationship between men and the gods, because men partake of the divine principle; on which account, also, God exercises his providence for our advantage. Also, that Fate is the cause of the arrangement of the world both generally and particularly. Moreover, that a ray from the sun penetrated both the cold aether and the dense aether; and they call the air the cold aether, and the sea and moisture they call the dense aether. And this ray descends into the depths, and in this way vivifies everything. And everything which partakes of the principle of heat lives, on which account, also, plants are animated beings; but that all living things have not necessarily souls. And that the soul is a something tom off from the aether, both warm and cold, from its partaking of the cold aether. And that the soul is something different from life. Also, that it is immortal, because that from which it has been detached is immortal.

"Also, that animals are born from one another by seeds, and that it is impossible for there to be any spontaneous production by the earth. And that seed is a drop from the brain which contains in itself a warm vapor; and that when this is applied to the womb it transmits virtue and moisture and blood from the brain, from which flesh and sinews and bones and hair and the whole body are produced. And from the vapor is produced the soul, and also sensation. And that the infant

first becomes a solid body at the end of forty days; but, according to the principles of harmony, it is not perfect till seven, or perhaps nine, or at most ten months, and then it is brought forth. And that it contains in itself all the principles of life, which are all connected together, and by their union and combination form a harmonious whole, each of them developing itself at the appointed time.

"The senses in general, and especially the sight, are a vapor of excessive warmth, and on this account a man is said to see through air and through water. For the hot principle is opposed by the cold one; since, if the vapor in the eyes were cold, it would have the same temperature as the air, and so would be dissipated. As it is, in some passages he calls the eyes the gates of the sun; and he speaks in a similar manner of hearing and of the other senses.

"He also says that the soul of man is divided into three parts: into intuition and reason and mind, and that the first and last divisions are found also in other animals, but that the middle one, reason, is only found in man. And that the chief abode of the soul is in those parts of the body which are between the heart and the brain. And that that portion of it which is in the heart is the mind; but that deliberation and reason reside in the brain.

Moreover, that the senses are drops from them; and that the reasoning sense is immortal, but the others are mortal. And that the soul is nourished by the blood; and that reasons are the winds of the soul. That it is invisible, and so are its reasons, since the aether itself is invisible. That the links of the soul are the veins and the arteries and the nerves. But that when it is vigorous, and is by itself in a quiescent state, then its links are words and actions. That when it is cast forth upon the earth it wanders about, resembling the body. Moreover, that Mercury is the steward of the souls, and that on this account he has the name of Conductor, and Commercial, and Infernal, since it is he who conducts the souls from their bodies, and from earth and sea; and that he conducts the pure souls to the highest region, and that he does not allow the impure ones to approach them, nor to come near one another, but commits them to be bound in indissoluble fetters by the Furies. The Pythagoreans also assert that the whole air is full of souls, and that these are those which are accounted daemons and heroes. Also, that it is by them that dreams are sent among men, and also the tokens of disease and health; these last, too, being sent not only to men, but to sheep also, and other cattle. Also that it is they who are concerned with purifications and expiations and all kinds of divination and oracular predictions, and things of that kind."[5]

A brief consideration of this summary of the doctrines of Pythagoras will show that it at least outlines a most extraordinary variety of scientific ideas. (1) There is suggested a theory of monads and the conception of the development from simple to more complex bodies, passing through the stages of lines, plain figures, and solids to sensible bodies. (2) The doctrine of the four elements--fire, water, earth, and air--as the basis of all organisms is put forward. (3) The idea, not merely of the sphericity of the earth, but an explicit conception of the antipodes, is expressed. (4) A conception of the sanitary influence of the air is clearly expressed. (5) An idea of the problems of generation and heredity is shown, together with a distinct disavowal of the doctrine of spontaneous generation-- a doctrine which, it may be added, remained in vogue, nevertheless, for some twenty-four hundred years after the time of Pythagoras. (6) A remarkable analysis of mind is made, and a distinction between animal minds and the human mind is based on this analysis. The physiological doctrine that the heart is the organ of one department of mind is offset by the clear statement that the remaining factors of mind reside in the brain. This early

recognition of brain as the organ of mind must not be forgotten in our later studies. It should be recalled, however, that a Crotonian physician, Alemaean, a younger contemporary of Pythagoras, is also credited with the same theory. (7) A knowledge of anatomy is at least vaguely foreshadowed in the assertion that veins, arteries, and nerves are the links of the soul. In this connection it should be recalled that Pythagoras was a practical physician.

As against these scientific doctrines, however, some of them being at least remarkable guesses at the truth, attention must be called to the concluding paragraph of our quotation, in which the old familiar daemonology is outlined, quite after the Oriental fashion. We shall have occasion to say more as to this phase of the subject later on. Meantime, before leaving Pythagoras, let us note that his practical studies of humanity led him to assert the doctrine that "the property of friends is common, and that friendship is equality." His disciples, we are told, used to put all their possessions together in one store and use them in common. Here, then, seemingly, is the doctrine of communism put to the test of experiment at this early day. If it seem that reference to this carries us beyond the bounds of science, it may be replied that questions such as this will not lie beyond the bounds of the science of the near future.

XENOPHANES AND PARMENIDES

There is a whimsical tale about Pythagoras, according to which the philosopher was wont to declare that in an earlier state he had visited Hades, and had there seen Homer and Hesiod tortured because of the absurd things they had said about the gods. Apocrypbal or otherwise, the tale suggests that Pythagoras was an agnostic as regards the current Greek religion of his time. The same thing is perhaps true of most of the great thinkers of this earliest period. But one among them was remembered in later times as having had a peculiar aversion to the anthropomorphic conceptions of his fellows. This was Xenophanes, who was born at Colophon probably about the year 580 B.C., and who, after a life of wandering, settled finally in Italy and became the founder of the so-called Eleatic School.

A few fragments of the philosophical poem in which Xenophanes expressed his views have come down to us, and these fragments include a tolerably definite avowal of his faith. "God is one supreme among gods and men, and not like mortals in body or in mind," says Xenophanes. Again he asserts that "mortals suppose that the gods are born (as they themselves are), that they wear man's clothing and have human voice and body; but," he continues, "if cattle or lions had hands so as to paint with their hands and produce works of art as men do, they would paint their gods and give them bodies in form like their own--horses like horses, cattle like cattle." Elsewhere he says, with great acumen: "There has not been a man, nor will there be, who knows distinctly what I say about the gods or in regard to all things. For even if one chance for the most part to say what is true, still he would not know; but every one thinks that he knows."[6]

In the same spirit Xenophanes speaks of the battles of Titans, of giants, and of centaurs as "fictions of former ages." All this tells of the questioning spirit which distinguishes the scientific investigator. Precisely whither this spirit led him we do not know, but the writers of a later time have preserved a tradition regarding a belief of Xenophanes that perhaps entitles him to be considered the father of geology. Thus Hippolytus records that Xenophanes studied the fossils to be found in quarries, and drew from their observation remarkable conclusions. His words are as follows: "Xenophanes believes that once the earth was mingled with the sea, but in the course of time it became freed from moisture; and his proofs are such as these: that shells are found in the

midst of the land and among the mountains, that in the quarries of Syracuse the imprints of a fish and of seals had been found, and in Paros the imprint of an anchovy at some depth in the stone, and in Melite shallow impressions of all sorts of sea products. He says that these imprints were made when everything long ago was covered with mud, and then the imprint dried in the mud. Further, he says that all men will be destroyed when the earth sinks into the sea and becomes mud, and that the race will begin anew from the beginning; and this transformation takes place for all worlds."[7] Here, then, we see this earliest of paleontologists studying the fossil-bearing strata of the earth, and drawing from his observations a marvellously scientific induction. Almost two thousand years later another famous citizen of Italy, Leonardo da Vinci, was independently to think out similar conclusions from like observations. But not until the nineteenth century of our era, some twenty-four hundred years after the time of Xenophanes, was the old Greek's doctrine to be accepted by the scientific world. The ideas of Xenophanes were known to his contemporaries and, as we see, quoted for a few centuries by his successors, then they were ignored or quite forgotten; and if any philosopher of an ensuing age before the time of Leonardo championed a like rational explanation of the fossils, we have no record of the fact. The geological doctrine of Xenophanes, then, must be listed among those remarkable Greek anticipations of nineteenth -century science which suffered almost total eclipse in the intervening centuries.

Among the pupils of Xenophanes was Parmenides, the thinker who was destined to carry on the work of his master along the same scientific lines, though at the same time mingling his scientific conceptions with the mysticism of the poet. We have already had occasion to mention that Parmenides championed the idea that the earth is round; noting also that doubts exist as to whether he or Pythagoras originated this doctrine. No explicit answer to this question can possibly be hoped for. It seems clear, however, that for a long time the Italic School, to which both these philosophers belonged, had a monopoly of the belief in question. Parmenides, like Pythagoras, is credited with having believed in the motion of the earth, though the evidence furnished by the writings of the philosopher himself is not as demonstrative as one could wish. Unfortunately, the copyists of a later age were more concerned with metaphysical speculations than with more tangible things. But as far as the fragmentary references to the ideas of Parmenides may be accepted, they do not support the idea of the earth's motion. Indeed, Parmenides is made to say explicitly, in preserved fragments, that "the world is immovable, limited, and spheroidal in form."[8]

Nevertheless, some modern interpreters have found an opposite meaning in Parmenides. Thus Ritter interprets him as supposing "that the earth is in the centre spherical, and maintained in rotary motion by its equiponderance; around it lie certain rings, the highest composed of the rare element fire, the next lower a compound of light and darkness, and lowest of all one wholly of night, which probably indicated to his mind the surface of the earth, the centre of which again he probably considered to be fire."[9] But this, like too many interpretations of ancient thought, appears to read into the fragments ideas which the words themselves do not warrant. There seems no reason to doubt, however, that Parmenides actually held the doctrine of the earth's sphericity. Another glimpse of his astronomical doctrines is furnished us by a fragment which tells us that he conceived the morning and the evening stars to be the same, a doctrine which, as we have seen, was ascribed also to Pythagoras. Indeed, we may repeat that it is quite impossible to distinguish between the astronomical doctrines of these two philosophers.

The poem of Parmenides in which the cosmogonic speculations occur treats also of the origin of man. The author seems to have had a clear conception that intelligence depends on bodily organism, and that the more elaborately developed the organism the higher the intelligence. But in the interpretation of this thought we are hampered by the characteristic vagueness of expression, which may best be evidenced by putting before the reader two English translations of the same stanza. Here is Ritter's rendering, as made into English by his translator, Morrison:

"For exactly as each has the state of his limbs many-jointed, So invariably stands it with men in their mind and their reason; For the system of limbs is that which thinketh in mankind Alike in all and in each: for thought is the fulness."[10]

The same stanza is given thus by George Henry Lewes:

"Such as to each man is the nature of his many-jointed limbs, Such also is the intelligence of each man; for it is The nature of limbs (organization) which thinketh in men, Both in one and in all; for the highest degree of organization gives the highest degree of thought."[11]

Here it will be observed that there is virtual agreement between the translators except as to the last clause, but that clause is most essential. The Greek phrase is <gr to gar pleon esti nohma>. Ritter, it will be observed, renders this, "for thought is the fulness." Lewes paraphrases it, "for the highest degree of organization gives the highest degree of thought." The difference is intentional, since Lewes himself criticises the translation of Ritter. Ritter's translation is certainly the more literal, but the fact that such diversity is possible suggests one of the chief elements of uncertainty that hamper our interpretation of the thought of antiquity. Unfortunately, the mind of the commentator has usually been directed towards such subtleties, rather than towards the expression of precise knowledge. Hence it is that the philosophers of Greece are usually thought of as mere dreamers, and that their true status as scientific discoverers is so often overlooked. With these intangibilities we have no present concern beyond this bare mention; for us it suffices to gain as clear an idea as we may of the really scientific conceptions of these thinkers, leaving the subtleties of their deductive reasoning for the most part untouched.

EMPEDOCLES

The latest of the important pre-Socratic philosophers of the Italic school was Empedocles, who was born about 494 B.C. and lived to the age of sixty. These dates make Empedocles strictly contemporary with Anaxagoras, a fact which we shall do well to bear in mind when we come to consider the latter's philosophy in the succeeding chapter. Like Pythagoras, Empedocles is an imposing figure. Indeed, there is much of similarity between the personalities, as between the doctrines, of the two men. Empedocles, like Pythagoras, was a physician; like him also he was the founder of a cult. As statesman, prophet, physicist, physician, reformer, and poet he showed a versatility that, coupled with profundity, marks the highest genius. In point of versatility we shall perhaps hardly find his equal at a later day--unless, indeed, an exception be made of Eratosthenes. The myths that have grown about the name of Empedocles show that he was a remarkable personality. He is said to have been an awe-inspiring figure, clothing himself in Oriental splendor and moving among mankind as a superior being. Tradition has it that he threw himself into the crater of a volcano that his otherwise unexplained disappearance might lead his disciples to believe that he had been miraculously translated; but tradition goes on to say that one of the brazen slippers of the philosopher was thrown up by the volcano, thus revealing his

subterfuge. Another tradition of far more credible aspect asserts that Empedocles retreated from Italy, returning to the home of his fathers in Peloponnesus to die there obscurely. It seems odd that the facts regarding the death of so great a man, at so comparatively late a period, should be obscure; but this, perhaps, is in keeping with the personality of the man himself. His disciples would hesitate to ascribe a merely natural death to so inspired a prophet.

Empedocles appears to have been at once an observer and a dreamer. He is credited with noting that the pressure of air will sustain the weight of water in an inverted tube; with divining, without the possibility of proof, that light has actual motion in space; and with asserting that centrifugal motion must keep the heavens from falling. He is credited with a great sanitary feat in the draining of a marsh, and his knowledge of medicine was held to be supernatural. Fortunately, some fragments of the writings of Empedocles have come down to us, enabling us to judge at first hand as to part of his doctrines; while still more is known through the references made to him by Plato, Aristotle, and other commentators. Empedocles was a poet whose verses stood the test of criticism. In this regard he is in a like position with Parmenides; but in neither case are the preserved fragments sufficient to enable us fully to estimate their author's scientific attainments. Philosophical writings are obscure enough at the best, and they perforce become doubly so when expressed in verse. Yet there are certain passages of Empedocles that are unequivocal and full of interest. Perhaps the most important conception which the works of Empedocles reveal to us is the denial of anthropomorphism as applied to deity. We have seen how early the anthropomorphic conception was developed and how closely it was all along clung to; to shake the mind free from it then was a remarkable feat, in accomplishing which Empedocles took a long step in the direction of rationalism. His conception is paralleled by that of another physician, Alcmaeon, of Proton, who contended that man's ideas of the gods amounted to mere suppositions at the very most. A rationalistic or sceptical tendency has been the accompaniment of medical training in all ages.

The words in which Empedocles expresses his conception of deity have been preserved and are well worth quoting: "It is not impossible," he says, "to draw near (to god) even with the eyes or to take hold of him with our hands, which in truth is the best highway of persuasion in the mind of man; for he has no human head fitted to a body, nor do two shoots branch out from the trunk, nor has he feet, nor swift legs, nor hairy parts, but he is sacred and ineffable mind alone, darting through the whole world with swift thoughts."[8]

How far Empedocles carried his denial of anthropomorphism is illustrated by a reference of Aristotle, who asserts "that Empedocles regards god as most lacking in the power of perception; for he alone does not know one of the elements, Strife (hence), of perishable things." It is difficult to avoid the feeling that Empedocles here approaches the modern philosophical conception that God, however postulated as immutable, must also be postulated as unconscious, since intelligence, as we know it, is dependent upon the transmutations of matter. But to urge this thought would be to yield to that philosophizing tendency which has been the bane of interpretation as applied to the ancient thinkers.

Considering for a moment the more tangible accomplishments of Empedocles, we find it alleged that one of his "miracles" consisted of the preservation of a dead body without putrefaction for some weeks after death. We may assume from this that he had gained in some way a knowledge of embalming. As he was notoriously fond of experiment, and as the body in question (assuming

for the moment the authenticity of the legend) must have been preserved without disfigurement, it is conceivable even that he had hit upon the idea of injecting the arteries. This, of course, is pure conjecture; yet it finds a certain warrant, both in the fact that the words of Pythagoras lead us to believe that the arteries were known and studied, and in the fact that Empedocles' own words reveal him also as a student of the vascular system. Thus Plutarch cites Empedocles as believing "that the ruling part is not in the head or in the breast, but in the blood; wherefore in whatever part of the body the more of this is spread in that part men excel."[13] And Empedocles' own words, as preserved by Stobaeus, assert "(the heart) lies in seas of blood which dart in opposite directions, and there most of all intelligence centres for men; for blood about the heart is intelligence in the case of man." All this implies a really remarkable appreciation of the dependence of vital activities upon the blood.

This correct physiological conception, however, was by no means the most remarkable of the ideas to which Empedoeles was led by his anatomical studies. His greatest accomplishment was to have conceived and clearly expressed an idea which the modern evolutionist connotes when he speaks of homologous parts--an idea which found a famous modern expositor in Goethe, as we shall see when we come to deal with eighteenth-century science. Empedocles expresses the idea in these words: "Hair, and leaves, and thick feathers of birds, are the same thing in origin, and reptile scales too on strong limbs. But on hedgehogs sharp-pointed hair bristles on their backs."[14] That the idea of transmutation of parts, as well as of mere homology, was in mind is evidenced by a very remarkable sentence in which Aristotle asserts, "Empedocles says that fingernails rise from sinew from hardening." Nor is this quite all, for surely we find the germ of the Lamarckian conception of evolution through the transmission of acquired characters in the assertion that "many characteristics appear in animals because it happened to be thus in their birth, as that they have such a spine because they happen to be descended from one that bent itself backward."[15] Aristotle, in quoting this remark, asserts, with the dogmatism which characterizes the philosophical commentators of every age, that "Empedocles is wrong," in making this assertion; but Lamarck, who lived twenty-three hundred years after Empedocles, is famous in the history of the doctrine of evolution for elaborating this very idea.

It is fair to add, however, that the dreamings of Empedocles regarding the origin of living organisms led him to some conceptions that were much less luminous. On occasion, Empedocles the poet got the better of Empedocles the scientist, and we are presented with a conception of creation as grotesque as that which delighted the readers of Paradise Lost at a later day. Empedocles assures us that "many heads grow up without necks, and arms were wandering about, necks bereft of shoulders, and eyes roamed about alone with no foreheads."[16] This chaotic condition, so the poet dreamed, led to the union of many incongruous parts, producing "creatures with double faces, offspring of oxen with human faces, and children of men with oxen heads." But out of this chaos came, finally, we are led to infer, a harmonious aggregation of parts, producing ultimately the perfected organisms that we see. Unfortunately the preserved portions of the writings of Empedocles do not enlighten us as to the precise way in which final evolution was supposed to be effected; although the idea of endless experimentation until natural selection resulted in survival of the fittest seems not far afield from certain of the poetical assertions. Thus: "As divinity was mingled yet more with divinity, these things (the various members) kept coming together in whatever way each might chance." Again: "At one time all the limbs which form the body united into one by love grew vigorously in the prime of life; but yet at another time, separated by evil Strife, they wander each in different directions along the

breakers of the sea of life. Just so is it with plants, and with fishes dwelling in watery halls, and beasts whose lair is in the mountains, and birds borne on wings."[17]

All this is poetry rather than science, yet such imaginings could come only to one who was groping towards what we moderns should term an evolutionary conception of the origins of organic life; and however grotesque some of these expressions may appear, it must be admitted that the morphological ideas of Empedocles, as above quoted, give the Sicilian philosopher a secure place among the anticipators of the modern evolutionist.

VII. GREEK SCIENCE IN THE EARLY ATTIC PERIOD

We have travelled rather far in our study of Greek science, and yet we have not until now come to Greece itself. And even now, the men whose names we are to consider were, for the most part, born in out- lying portions of the empire; they differed from the others we have considered only in the fact that they were drawn presently to the capital. The change is due to a most interesting sequence of historical events. In the day when Thales and his immediate successors taught in Miletus, when the great men of the Italic school were in their prime, there was no single undisputed Centre of Greek influence. The Greeks were a disorganized company of petty nations, welded together chiefly by unity of speech; but now, early in the fifth century B.C., occurred that famous attack upon the Western world by the Persians under Darius and his son and successor Xerxes. A few months of battling determined the fate of the Western world. The Orientals were hurled back; the glorious memories of Marathon, Salamis, and Plataea stimulated the patriotism and enthusiasm of all children of the Greek race. The Greeks, for the first time, occupied the centre of the historical stage; for the brief interval of about half a century the different Grecian principalities lived together in relative harmony. One city was recognized as the metropolis of the loosely bound empire; one city became the home of culture and the Mecca towards which all eyes turned; that city, of course, was Athens. For a brief time all roads led to Athens, as, at a later date, they all led to Rome. The waterways which alone bound the widely scattered parts of Hellas into a united whole led out from Athens and back to Athens, as the spokes of a wheel to its hub. Athens was the commercial centre, and, largely for that reason, it became the centre of culture and intellectual influence also. The wise men from the colonies visited the metropolis, and the wise Athenians went out to the colonies. Whoever aspired to become a leader in politics, in art, in literature, or in philosophy, made his way to the capital, and so, with almost bewildering suddenness, there blossomed the civilization of the age of Pericles; the civilization which produced aeschylus, Sophocles, Euripides, Herodotus, and Thucydides; the civilization which made possible the building of the Parthenon.

ANAXAGORAS

Sometime during the early part of this golden age there came to Athens a middle-aged man from Clazomenae, who, from our present stand-point, was a more interesting personality than perhaps any other in the great galaxy of remarkable men assembled there. The name of this new-comer was Anaxagoras. It was said in after-time, we know not with what degree of truth, that he had been a pupil of Anaximenes. If so, he was a pupil who departed far from the teachings of his

master. What we know for certain is that Anaxagoras was a truly original thinker, and that he became a close friend--in a sense the teacher--of Pericles and of Euripides. Just how long he remained at Athens is not certain; but the time came when he had made himself in some way objectionable to the Athenian populace through his teachings. Filled with the spirit of the investigator, he could not accept the current conceptions as to the gods. He was a sceptic, an innovator. Such men are never welcome; they are the chief factors in the progress of thought, but they must look always to posterity for recognition of their worth; from their contemporaries they receive, not thanks, but persecution. Sometimes this persecution takes one form, sometimes another; to the credit of the Greeks be it said, that with them it usually led to nothing more severe than banishment. In the case of Anaxagoras, it is alleged that the sentence pronounced was death; but that, thanks to the influence of Pericles, this sentence was commuted to banishment. In any event, the aged philosopher was sent away from the city of his adoption. He retired to Lampsacus. "It is not I that have lost the Athenians," he said; "it is the Athenians that have lost me."

The exact position which Anaxagoras had among his contemporaries, and his exact place in the development of philosophy, have always been somewhat in dispute. It is not known, of a certainty, that he even held an open school at Athens. Ritter thinks it doubtful that he did. It was his fate to be misunderstood, or underestimated, by Aristotle; that in itself would have sufficed greatly to dim his fame--might, indeed, have led to his almost entire neglect had he not been a truly remarkable thinker. With most of the questions that have exercised the commentators we have but scant concern. Following Aristotle, most historians of philosophy have been metaphysicians; they have concerned themselves far less with what the ancient thinkers really knew than with what they thought. A chance using of a verbal quibble, an esoteric phrase, the expression of a vague mysticism--these would suffice to call forth reams of exposition. It has been the favorite pastime of historians to weave their own anachronistic theories upon the scanty woof of the half- remembered thoughts of the ancient philosophers. To make such cloth of the imagination as this is an alluring pastime, but one that must not divert us here. Our point of view reverses that of the philosophers. We are chiefly concerned, not with some vague saying of Anaxagoras, but with what he really knew regarding the phenomena of nature; with what he observed, and with the comprehensible deductions that he derived from his observations. In attempting to answer these inquiries, we are obliged, in part, to take our evidence at second-hand; but, fortunately, some fragments of writings of Anaxagoras have come down to us. We are told that he wrote only a single book. It was said even (by Diogenes) that he was the first man that ever wrote a work in prose. The latter statement would not bear too close an examination, yet it is true that no extensive prose compositions of an earlier day than this have been preserved, though numerous others are known by their fragments. Herodotus, "the father of prose," was a slightly younger contemporary of the Clazomenaean philosopher; not unlikely the two men may have met at Athens.

Notwithstanding the loss of the greater part of the writings of Anaxagoras, however, a tolerably precise account of his scientific doctrines is accessible. Diogenes Laertius expresses some of them in very clear and precise terms. We have already pointed out the uncertainty that attaches to such evidence as this, but it is as valid for Anaxagoras as for another. If we reject such evidence, we shall often have almost nothing left; in accepting it we may at least feel certain that we are viewing the thinker as his contemporaries and immediate successors viewed him. Following Diogenes, then, we shall find some remarkable scientific opinions ascribed to Anaxagoras. "He

asserted," we are told, "that the sun was a mass of burning iron, greater than Peloponnesus, and that the moon contained houses and also hills and ravines." In corroboration of this, Plato represents him as having conjectured the right explanation of the moon's light, and of the solar and lunar eclipses. He had other astronomical theories that were more fanciful; thus "he said that the stars originally moved about in irregular confusion, so that at first the pole-star, which is continually visible, always appeared in the zenith, but that afterwards it acquired a certain declination, and that the Milky Way was a reflection of the light of the sun when the stars did not appear. The comets he considered to be a concourse of planets emitting rays, and the shooting-stars he thought were sparks, as it were, leaping from the firmament."

Much of this is far enough from the truth, as we now know it, yet all of it shows an earnest endeavor to explain the observed phenomena of the heavens on rational principles. To have predicated the sun as a great molten mass of iron was indeed a wonderful anticipation of the results of the modern spectroscope. Nor can it be said that this hypothesis of Anaxagoras was a purely visionary guess. It was in all probability a scientific deduction from the observed character of meteoric stones. Reference has already been made to the alleged prediction of the fall of the famous meteor at aegespotomi by Anaxagoras. The assertion that he actually predicted this fall in any proper sense of the word would be obviously absurd. Yet the fact that his name is associated with it suggests that he had studied similar meteorites, or else that he studied this particular one, since it is not quite clear whether it was before or after this fall that he made the famous assertion that space is full of falling stones. We should stretch the probabilities were we to assert that Anaxagoras knew that shooting-stars and meteors were the same, yet there is an interesting suggestiveness in his likening the shooting-stars to sparks leaping from the firmament, taken in connection with his observation on meteorites. Be this as it may, the fact that something which falls from heaven as a blazing light turns out to be an iron-like mass may very well have suggested to the most rational of thinkers that the great blazing light called the sun has the same composition. This idea grasped, it was a not unnatural extension to conceive the other heavenly bodies as having the same composition.

This led to a truly startling thought. Since the heavenly bodies are of the same composition as the earth, and since they are observed to be whirling about the earth in space, may we not suppose that they were once a part of the earth itself, and that they have been thrown off by the force of a whirling motion? Such was the conclusion which Anaxagoras reached; such his explanation of the origin of the heavenly bodies. It was a marvellous guess. Deduct from it all that recent science has shown to be untrue; bear in mind that the stars are suns, compared with which the earth is a mere speck of dust; recall that the sun is parent, not daughter, of the earth, and despite all these deductions, the cosmogonic guess of Anaxagoras remains, as it seems to us, one of the most marvellous feats of human intelligence. It was the first explanation of the cosmic bodies that could be called, in any sense, an anticipation of what the science of our own day accepts as a true explanation of cosmic origins. Moreover, let us urge again that this was no mere accidental flight of the imagination; it was a scientific induction based on the only data available; perhaps it is not too much to say that it was the only scientific induction which these data would fairly sustain. Of course it is not for a moment to be inferred that Anaxagoras understood, in the modern sense, the character of that whirling force which we call centrifugal. About two thousand years were yet to elapse before that force was explained as elementary inertia; and even that explanation, let us not forget, merely sufficed to push back the barriers of mystery by one other stage; for even in our day inertia is a statement of fact rather than an explanation.

But however little Anaxagoras could explain the centrifugal force on mechanical principles, the practical powers of that force were sufficiently open to his observation. The mere experiment of throwing a stone from a sling would, to an observing mind, be full of suggestiveness. It would be obvious that by whirling the sling about, the stone which it held would be sustained in its circling path about the hand in seeming defiance of the earth's pull, and after the stone had left the sling, it could fly away from the earth to a distance which the most casual observation would prove to be proportionate to the speed of its flight. Extremely rapid motion, then, might project bodies from the earth's surface off into space; a sufficiently rapid whirl would keep them there. Anaxagoras conceived that this was precisely what had occurred. His imagination even carried him a step farther--to a conception of a slackening of speed, through which the heavenly bodies would lose their centrifugal force, and, responding to the perpetual pull of gravitation, would fall back to the earth, just as the great stone at aegespotomi had been observed to do.

Here we would seem to have a clear conception of the idea of universal gravitation, and Anaxagoras stands before us as the anticipator of Newton. Were it not for one scientific maxim, we might exalt the old Greek above the greatest of modern natural philosophers; but that maxim bids us pause. It is phrased thus, "He discovers who proves." Anaxagoras could not prove; his argument was at best suggestive, not demonstrative. He did not even know the laws which govern falling bodies; much less could he apply such laws, even had he known them, to sidereal bodies at whose size and distance he could only guess in the vaguest terms. Still his cosmogonic speculation remains as perhaps the most remarkable one of antiquity. How widely his speculation found currency among his immediate successors is instanced in a passage from Plato, where Socrates is represented as scornfully answering a calumniator in these terms: "He asserts that I say the sun is a stone and the moon an earth. Do you think of accusing Anaxagoras, Miletas, and have you so low an opinion of these men, and think them so unskilled in laws, as not to know that the books of Anaxagoras the Clazomenaean are full of these doctrines. And forsooth the young men are learning these matters from me which sometimes they can buy from the orchestra for a drachma, at the most, and laugh at Socrates if he pretends they are his-particularly seeing they are so strange."

The element of error contained in these cosmogonic speculations of Anaxagoras has led critics to do them something less than justice. But there is one other astronomical speculation for which the Clazomenaean philosopher has received full credit. It is generally admitted that it was he who first found out the explanation of the phases of the moon; a knowledge that that body shines only by reflected light, and that its visible forms, waxing and waning month by month from crescent to disk and from disk to crescent, merely represent our shifting view of its sun-illumined face. It is difficult to put ourselves in the place of the ancient observer and realize how little the appearances suggest the actual fact. That a body of the same structure as the earth should shine with the radiance of the moon merely because sunlight is reflected from it, is in itself a supposition seemingly contradicted by ordinary experience. It required the mind of a philosopher, sustained, perhaps, by some experimental observations, to conceive the idea that what seems so obviously bright may be in reality dark. The germ of the conception of what the philosopher speaks of as the noumena, or actualities, back of phenomena or appearances, had perhaps this crude beginning. Anaxagoras could surely point to the moon in support of his seeming paradox that snow, being really composed of water, which is dark, is in reality black and not white--a contention to which we shall refer more at length in a moment.

But there is yet another striking thought connected with this new explanation of the phases of the moon. The explanation implies not merely the reflection of light by a dark body, but by a dark body of a particular form. Granted that reflections are in question, no body but a spherical one could give an appearance which the moon presents. The moon, then, is not merely a mass of earth, it is a spherical mass of earth. Here there were no flaws in the reasoning of Anaxagoras. By scientific induction he passed from observation to explanation. A new and most important element was added to the science of astronomy.

Looking back from the latter-day stand-point, it would seem as if the mind of the philosopher must have taken one other step: the mind that had conceived sun, moon, stars, and earth to be of one substance might naturally, we should think, have reached out to the further induction that, since the moon is a sphere, the other cosmic bodies, including the earth, must be spheres also. But generalizer as he was, Anaxagoras was too rigidly scientific a thinker to make this assumption. The data at his command did not, as he analyzed them, seem to point to this conclusion. We have seen that Pythagoras probably, and Parmenides surely, out there in Italy had conceived the idea of the earth's rotundity, but the Pythagorean doctrines were not rapidly taken up in the mother- country, and Parmenides, it must be recalled, was a strict contemporary of Anaxagoras himself. It is no reproach, therefore, to the Clazomenaean philosopher that he should have held to the old idea that the earth is flat, or at most a convex disk--the latter being the Babylonian conception which probably dominated that Milesian school to which Anaxagoras harked back.

Anaxagoras may never have seen an eclipse of the moon, and even if he had he might have reflected that, from certain directions, a disk may throw precisely the same shadow as a sphere. Moreover, in reference to the shadow cast by the earth, there was, so Anaxagoras believed, an observation open to him nightly which, we may well suppose, was not without influence in suggesting to his mind the probable shape of the earth. The Milky Way, which doubtless had puzzled astronomers from the beginnings of history and which was to continue to puzzle them for many centuries after the day of Anaxagoras, was explained by the Clazomenaean philosopher on a theory obviously suggested by the theory of the moon's phases. Since the earth- like moon shines by reflected light at night, and since the stars seem obviously brighter on dark nights, Anaxagoras was but following up a perfectly logical induction when he propounded the theory that the stars in the Milky Way seem more numerous and brighter than those of any other part of the heavens, merely because the Milky Way marks the shadow of the earth. Of course the inference was wrong, so far as the shadow of the earth is concerned; yet it contained a part truth, the force of which was never fully recognized until the time of Galileo. This consists in the assertion that the brightness of the Milky Way is merely due to the glow of many stars. The shadow- theory of Anaxagoras would naturally cease to have validity so soon as the sphericity of the earth was proved, and with it, seemingly, fell for the time the companion theory that the Milky Way is made up of a multitude of stars.

It has been said by a modern critic[1] that the shadow-theory was childish in that it failed to note that the Milky Way does not follow the course of the ecliptic. But this criticism only holds good so long as we reflect on the true character of the earth as a symmetrical body poised in space. It is quite possible to conceive a body occupying the position of the earth with reference to the sun which would cast a shadow having such a tenuous form as the Milky Way presents. Such a body obviously would not be a globe, but a long-drawn-out, attenuated figure. There is, to be sure, no

direct evidence preserved to show that Anaxagoras conceived the world to present such a figure as this, but what we know of that philosopher's close-reasoning, logical mind gives some warrant to the assumption--gratuitous though in a sense it be-- that the author of the theory of the moon's phases had not failed to ask himself what must be the form of that terrestrial body which could cast the tenuous shadow of the Milky Way. Moreover, we must recall that the habitable earth, as known to the Greeks of that day, was a relatively narrow band of territory, stretching far to the east and to the west.

Anaxagoras as Meteorologist

The man who had studied the meteorite of aegospotami, and been put by it on the track of such remarkable inductions, was, naturally, not oblivious to the other phenomena of the atmosphere. Indeed, such a mind as that of Anaxagoras was sure to investigate all manner of natural phenomena, and almost equally sure to throw new light on any subject that it investigated. Hence it is not surprising to find Anaxagoras credited with explaining the winds as due to the rarefactions of the atmosphere produced by the sun. This explanation gives Anaxagoras full right to be called "the father of meteorology," a title which, it may be, no one has thought of applying to him, chiefly because the science of meteorology did not make its real beginnings until some twenty-four hundred years after the death of its first great votary. Not content with explaining the winds, this prototype of Franklin turned his attention even to the tipper atmosphere. "Thunder," he is reputed to have said, "was produced by the collision of the clouds, and lightning by the rubbing together of the clouds." We dare not go so far as to suggest that this implies an association in the mind of Anaxagoras between the friction of the clouds and the observed electrical effects generated by the friction of such a substance as amber. To make such a suggestion doubtless would be to fall victim to the old familiar propensity to read into Homer things that Homer never knew. Yet the significant fact remains that Anaxagoras ascribed to thunder and to lightning their true position as strictly natural phenomena. For him it was no god that menaced humanity with thundering voice and the flash of his divine fires from the clouds. Little wonder that the thinker whose science carried him to such scepticism as this should have felt the wrath of the superstitious Athenians.

Biological Speculations

Passing from the phenomena of the air to those of the earth itself, we learn that Anaxagoras explained an earthquake as being produced by the returning of air into the earth. We cannot be sure as to the exact meaning here, though the idea that gases are imprisoned in the substance of the earth seems not far afield. But a far more remarkable insight than this would imply was shown by Anaxagoras when he asserted that a certain amount of air is contained in water, and that fishes breathe this air. The passage of Aristotle in which this opinion is ascribed to Anaxagoras is of sufficient interest to be quoted at length:

"Democritus, of Abdera," says Aristotle, "and some others, that have spoken concerning respiration, have determined nothing concerning other animals, but seem to have supposed that all animals respire. But Anaxagoras and Diogenes (Apolloniates), who say that all animals respire, have also endeavored to explain how fishes, and all those animals that have a hard, rough shell, such as oysters, mussels, etc., respire. And Anaxagoras, indeed, says that fishes, when they emit water through their gills, attract air from the mouth to the vacuum in the viscera from the water which surrounds the mouth; as if air was inherent in the water."[2]

It should be recalled that of the three philosophers thus mentioned as contending that all animals respire, Anaxagoras was the elder; he, therefore, was presumably the originator of the idea. It will be observed, too, that Anaxagoras alone is held responsible for the idea that fishes respire air through their gills, "attracting" it from the water. This certainly was one of the shrewdest physiological guesses of any age, if it be regarded as a mere guess. With greater justice we might refer to it as a profound deduction from the principle of the uniformity of nature.

In making such a deduction, Anaxagoras was far in advance of his time as illustrated by the fact that Aristotle makes the citation we have just quoted merely to add that "such things are impossible," and to refute these "impossible" ideas by means of metaphysical reasonings that seemed demonstrative not merely to himself, but to many generations of his followers.

We are told that Anaxagoras alleged that all animals were originally generated out of moisture, heat, and earth particles. Just what opinion he held concerning man's development we are not informed. Yet there is one of his phrases which suggests--without, perhaps, quite proving--that he was an evolutionist. This phrase asserts, with insight that is fairly startling, that man is the most intelligent of animals because he has hands. The man who could make that assertion must, it would seem, have had in mind the idea of the development of intelligence through the use of hands-- an idea the full force of which was not evident to subsequent generations of thinkers until the time of Darwin.

Physical Speculations

Anaxagoras is cited by Aristotle as believing that "plants are animals and feel pleasure and pain, inferring this because they shed their leaves and let them grow again." The idea is fanciful, yet it suggests again a truly philosophical conception of the unity of nature. The man who could conceive that idea was but little hampered by traditional conceptions. He was exercising a rare combination of the rigidly scientific spirit with the poetical imagination. He who possesses these gifts is sure not to stop in his questionings of nature until he has found some thinkable explanation of the character of matter itself. Anaxagoras found such an explanation, and, as good luck would have it, that explanation has been preserved. Let us examine his reasoning in some detail. We have already referred to the claim alleged to have been made by Anaxagoras that snow is not really white, but black. The philosopher explained his paradox, we are told, by asserting that snow is really water, and that water is dark, when viewed under proper conditions--as at the bottom of a well. That idea contains the germ of the Clazomenaean philosopher's conception of the nature of matter. Indeed, it is not unlikely that this theory of matter grew out of his observation of the changing forms of water. He seems clearly to have grasped the idea that snow on the one hand, and vapor on the other, are of the same intimate substance as the water from which they are derived and into which they may be again transformed. The fact that steam and snow can be changed back into water, and by simple manipulation cannot be changed into any other substance, finds, as we now believe, its true explanation in the fact that the molecular structure, as we phrase it--that is to say, the ultimate particle of which water is composed, is not changed, and this is precisely the explanation which Anaxagoras gave of the same phenomena. For him the unit particle of water constituted an elementary body, uncreated, unchangeable, indestructible. This particle, in association with like particles, constitutes the substance which we call water. The same particle in association with particles unlike itself, might produce totally different substances--as, for example, when water is taken up by the roots of a plant and

becomes, seemingly, a part of the substance of the plant. But whatever the changed association, so Anaxagoras reasoned, the ultimate particle of water remains a particle of water still. And what was true of water was true also, so he conceived, of every other substance. Gold, silver, iron, earth, and the various vegetables and animal tissues--in short, each and every one of all the different substances with which experience makes us familiar, is made up of unit particles which maintain their integrity in whatever combination they may be associated. This implies, obviously, a multitude of primordial particles, each one having an individuality of its own; each one, like the particle of water already cited, uncreated, unchangeable, and indestructible.

Fortunately, we have the philosopher's own words to guide us as to his speculations here. The fragments of his writings that have come down to us (chiefly through the quotations of Simplicius) deal almost exclusively with these ultimate conceptions of his imagination. In ascribing to him, then, this conception of diverse, uncreated, primordial elements, which can never be changed, but can only be mixed together to form substances of the material world, we are not reading back post-Daltonian knowledge into the system of Anaxagoras. Here are his words: "The Greeks do not rightly use the terms 'coming into being' and 'perishing.' For nothing comes into being, nor, yet, does anything perish; but there is mixture and separation of things that are. So they would do right in calling 'coming into being' 'mixture' and 'perishing' 'separation.' For how could hair come from what is not hair? Or flesh from what is not flesh?"

Elsewhere he tells us that (at one stage of the world's development) "the dense, the moist, the cold, the dark, collected there where now is earth; the rare, the warm, the dry, the bright, departed towards the further part of the aether. The earth is condensed out of these things that are separated, for water is separated from the clouds, and earth from the water; and from the earth stones are condensed by the cold, and these are separated farther from the water." Here again the influence of heat and cold in determining physical qualities is kept pre-eminently in mind. The dense, the moist, the cold, the dark are contrasted with the rare, the warm, the dry, and bright; and the formation of stones is spoken of as a specific condensation due to the influence of cold. Here, then, we have nearly all the elements of the Daltonian theory of atoms on the one hand, and the nebular hypothesis of Laplace on the other. But this is not quite all. In addition to such diverse elementary particles as those of gold, water, and the rest, Anaxagoras conceived a species of particles differing from all the others, not merely as they differ from one another, but constituting a class by themselves; particles infinitely smaller than the others; particles that are described as infinite, self-powerful, mixed with nothing, but existing alone. That is to say (interpreting the theory in the only way that seems plausible), these most minute particles do not mix with the other primordial particles to form material substances in the same way in which these mixed with one another. But, on the other hand, these "infinite, self-powerful, and unmixed" particles commingle everywhere and in every substance whatever with the mixed particles that go to make up the substances.

There is a distinction here, it will be observed, which at once suggests the modern distinction between physical processes and chemical processes, or, putting it otherwise, between molecular processes and atomic processes; but the reader must be guarded against supposing that Anaxagoras had any such thought as this in mind. His ultimate mixable particles can be compared only with the Daltonian atom, not with the molecule of the modern physicist, and his "infinite, self- powerful, and unmixable" particles are not comparable with anything but the ether of the modern physicist, with which hypothetical substance they have many points of

resemblance. But the "infinite, self- powerful, and unmixed" particles constituting thus an ether-like plenum which permeates all material structures, have also, in the mind of Anaxagoras, a function which carries them perhaps a stage beyond the province of the modern ether. For these "infinite, self powerful, and unmixed" particles are imbued with, and, indeed, themselves constitute, what Anaxagoras terms nous, a word which the modern translator has usually paraphrased as "mind." Neither that word nor any other available one probably conveys an accurate idea of what Anaxagoras meant to imply by the word nous. For him the word meant not merely "mind" in the sense of receptive and comprehending intelligence, but directive and creative intelligence as well. Again let Anaxagoras speak for himself: "Other things include a portion of everything, but nous is infinite, and self-powerful, and mixed with nothing, but it exists alone, itself by itself. For if it were not by itself, but were mixed with anything else, it would include parts of all things, if it were mixed with anything; for a portion of everything exists in every thing, as has been said by me before, and things mingled with it would prevent it from having power over anything in the same way that it does now that it is alone by itself. For it is the most rarefied of all things and the purest, and it has all knowledge in regard to everything and the greatest power; over all that has life, both greater and less, nous rules. And nous ruled the rotation of the whole, so that it set it in rotation in the beginning. First it began the rotation from a small beginning, then more and more was included in the motion, and yet more will be included. Both the mixed and the separated and distinct, all things nous recognized. And whatever things were to be, and whatever things were, as many as are now, and whatever things shall be, all these nous arranged in order; and it arranged that rotation, according to which now rotate stars and sun and moon and air and aether, now that they are separated. Rotation itself caused the separation, and the dense is separated from the rare, the warm from the cold, the bright from the dark, the dry from the moist. And when nous began to set things in motion, there was separation from everything that was in motion, all this was made distinct. The rotation of the things that were moved and made distinct caused them to be yet more distinct."[3]

Nous, then, as Anaxagoras conceives it, is "the most rarefied of all things, and the purest, and it has knowledge in regard to everything and the greatest power; over all that has life, both greater and less, it rules." But these are postulants of omnipresence and omniscience. In other words, nous is nothing less than the omnipotent artificer of the material universe. It lacks nothing of the power of deity, save only that we are not assured that it created the primordial particles. The creation of these particles was a conception that for Anaxagoras, as for the modern Spencer, lay beyond the range of imagination. Nous is the artificer, working with "uncreated" particles. Back of nous and the particles lies, for an Anaxagoras as for a Spencer, the Unknowable. But nous itself is the equivalent of that universal energy of motion which science recognizes as operating between the particles of matter, and which the theologist personifies as Deity. It is Pantheistic deity as Anaxagoras conceives it; his may be called the first scientific conception of a non-anthropomorphic god. In elaborating this conception Anaxagoras proved himself one of the most remarkable scientific dreamers of antiquity. To have substituted for the Greek Pantheon of anthropomorphic deities the conception of a non-anthropomorphic immaterial and ethereal entity, of all things in the world "the most rarefied and the purest," is to have performed a feat which, considering the age and the environment in which it was accomplished, staggers the imagination. As a strictly scientific accomplishment the great thinker's conception of primordial elements contained a germ of the truth which was to lie dormant for 2200 years, but which then, as modified and vitalized by the genius of Dalton, was to dominate the new chemical science of the nineteenth century. If there are intimations that the primordial element of Anaxagoras and of

Dalton may turn out in the near future to be itself a compound, there will still remain the yet finer particles of the nous of Anaxagoras to baffle the most subtle analysis of which to-day's science gives us any pre-vision. All in all, then, the work of Anaxagoras must stand as that of perhaps the most far-seeing scientific imagination of pre-Socratic antiquity.

LEUCIPPUS AND DEMOCRITUS

But we must not leave this alluring field of speculation as to the nature of matter without referring to another scientific guess, which soon followed that of Anaxagoras and was destined to gain even wider fame, and which in modern times has been somewhat unjustly held to eclipse the glory of the other achievement. We mean, of course, the atomic theory of Leucippus and Democritus. This theory reduced all matter to primordial elements, called atoms <gr atoma> because they are by hypothesis incapable of further division. These atoms, making up the entire material universe, are in this theory conceived as qualitatively identical, differing from one another only in size and perhaps in shape. The union of different-sized atoms in endless combinations produces the diverse substances with which our senses make us familiar.

Before we pass to a consideration of this alluring theory, and particularly to a comparison of it with the theory of Anaxagoras, we must catch a glimpse of the personality of the men to whom the theory owes its origin. One of these, Leucippus, presents so uncertain a figure as to be almost mythical. Indeed, it was long questioned whether such a man had actually lived, or whether be were not really an invention of his alleged disciple, Democritus. Latterday scholarship, however, accepts him as a real personage, though knowing scarcely more of him than that he was the author of the famous theory with which his name was associated. It is suggested that he was a wanderer, like most philosophers of his time, and that later in life he came to Abdera, in Thrace, and through this circumstance became the teacher of Democritus. This fable answers as well as another. What we really know is that Democritus himself, through whose writings and teachings the atomic theory gained vogue, was born in Abdera, about the year 460 B.C.--that is to say, just about the time when his great precursor, Anaxagoras, was migrating to Athens. Democritus, like most others of the early Greek thinkers, lives in tradition as a picturesque figure. It is vaguely reported that he travelled for a time, perhaps in the East and in Egypt, and that then he settled down to spend the remainder of his life in Abdera. Whether or not he visited Athens in the course of his wanderings we do not know. At Abdera he was revered as a sage, but his influence upon the practical civilization of the time was not marked. He was pre-eminently a dreamer and a writer. Like his confreres of the epoch, he entered all fields of thought. He wrote voluminously, but, unfortunately, his writings have, for the most part, perished. The fables and traditions of a later day asserted that Democritus had voluntarily put out his own eyes that he might turn his thoughts inward with more concentration. Doubtless this is fiction, yet, as usual with such fictions, it contains a germ of truth; for we may well suppose that the promulgator of the atomic theory was a man whose mind was attracted by the subtleties of thought rather than by the tangibilities of observation. Yet the term "laughing philosopher," which seems to have been universally applied to Democritus, suggests a mind not altogether withdrawn from the world of practicalities.

So much for Democritus the man. Let us return now to his theory of atoms. This theory, it must be confessed, made no very great impression upon his contemporaries. It found an expositor, a little later, in the philosopher Epicurus, and later still the poet Lucretius gave it popular

expression. But it seemed scarcely more than the dream of a philosopher or the vagary of a poet until the day when modern science began to penetrate the mysteries of matter. When, finally, the researches of Dalton and his followers had placed the atomic theory on a surer footing as the foundation of modern chemistry, the ideas of the old laughing philosopher of Abdera, which all along had been half derisively remembered, were recalled with a new interest. Now it appeared that these ideas had curiously foreshadowed nineteenth-century knowledge. It appeared that away back in the fifth century B.C. a man had dreamed out a conception of the ultimate nature of matter which had waited all these centuries for corroboration. And now the historians of philosophy became more than anxious to do justice to the memory of Democritus.

It is possible that this effort at poetical restitution has carried the enthusiast too far. There is, indeed, a curious suggestiveness in the theory of Democritus; there is philosophical allurement in his reduction of all matter to a single element; it contains, it may be, not merely a germ of the science of the nineteenth-century chemistry, but perhaps the germs also of the yet undeveloped chemistry of the twentieth century. Yet we dare suggest that in their enthusiasm for the atomic theory of Democritus the historians of our generation have done something less than justice to that philosopher's precursor, Anaxagoras. And one suspects that the mere accident of a name has been instrumental in producing this result. Democritus called his primordial element an atom; Anaxagoras, too, conceived a primordial element, but he called it merely a seed or thing; he failed to christen it distinctively. Modern science adopted the word atom and gave it universal vogue. It owed a debt of gratitude to Democritus for supplying it the word, but it somewhat overpaid the debt in too closely linking the new meaning of the word with its old original one. For, let it be clearly understood, the Daltonian atom is not precisely comparable with the atom of Democritus. The atom, as Democritus conceived it, was monistic; all atoms, according to this hypothesis, are of the same substance; one atom differs from another merely in size and shape, but not at all in quality. But the Daltonian hypothesis conceived, and nearly all the experimental efforts of the nineteenth century seemed to prove, that there are numerous classes of atoms, each differing in its very essence from the others.

As the case stands to-day the chemist deals with seventy-odd substances, which he calls elements. Each one of these substances is, as he conceives it, made up of elementary atoms having a unique personality, each differing in quality from all the others. As far as experiment has thus far safely carried us, the atom of gold is a primordial element which remains an atom of gold and nothing else, no matter with what other atoms it is associated. So, too, of the atom of silver, or zinc, or sodium--in short, of each and every one of the seventy-odd elements. There are, indeed, as we shall see, experiments that suggest the dissolution of the atom--that suggest, in short, that the Daltonian atom is misnamed, being a structure that may, under certain conditions, be broken asunder. But these experiments have, as yet, the warrant rather of philosophy than of pure science, and to-day we demand that the philosophy of science shall be the handmaid of experiment.

When experiment shall have demonstrated that the Daltonian atom is a compound, and that in truth there is but a single true atom, which, combining with its fellows perhaps in varying numbers and in different special relations, produces the Daltonian atoms, then the philosophical theory of monism will have the experimental warrant which to-day it lacks; then we shall be a step nearer to the atom of Democritus in one direction, a step farther away in the other. We shall be nearer, in that the conception of Democritus was, in a sense, monistic; farther away, in that all

the atoms of Democritus, large and small alike, were considered as permanently fixed in size. Democritus postulated all his atoms as of the same substance, differing not at all in quality; yet he was obliged to conceive that the varying size of the atoms gave to them varying functions which amounted to qualitative differences. He might claim for his largest atom the same quality of substance as for his smallest, but so long as he conceived that the large atoms, when adjusted together to form a tangible substance, formed a substance different in quality from the substance which the small atoms would make up when similarly grouped, this concession amounts to the predication of difference of quality between the atoms themselves. The entire question reduces itself virtually to a quibble over the word quality, So long as one atom conceived to be primordial and indivisible is conceded to be of such a nature as necessarily to produce a different impression on our senses, when grouped with its fellows, from the impression produced by other atoms when similarly grouped, such primordial atoms do differ among themselves in precisely the same way for all practical purposes as do the primordial elements of Anaxagoras.

The monistic conception towards which twentieth- century chemistry seems to be carrying us may perhaps show that all the so-called atoms are compounded of a single element. All the true atoms making up that element may then properly be said to have the same quality, but none the less will it remain true that the combinations of that element that go to make up the different Daltonian atoms differ from one another in quality in precisely the same sense in which such tangible substances as gold, and oxygen, and mercury, and diamonds differ from one another. In the last analysis of the monistic philosophy, there is but one substance and one quality in the universe. In the widest view of that philosophy, gold and oxygen and mercury and diamonds are one substance, and, if you please, one quality. But such refinements of analysis as this are for the transcendental philosopher, and not for the scientist. Whatever the allurement of such reasoning, we must for the purpose of science let words have a specific meaning, nor must we let a mere word-jugglery blind us to the evidence of facts. That was the rock on which Greek science foundered; it is the rock which the modern helmsman sometimes finds it difficult to avoid. And if we mistake not, this case of the atom of Democritus is precisely a case in point. Because Democritus said that his atoms did not differ in quality, the modern philosopher has seen in his theory the essentials of monism; has discovered in it not merely a forecast of the chemistry of the nineteenth century, but a forecast of the hypothetical chemistry of the future. And, on the other hand, because Anaxagoras predicted a different quality for his primordial elements, the philosopher of our day has discredited the primordial element of Anaxagoras.

Yet if our analysis does not lead us astray, the theory of Democritus was not truly monistic; his indestructible atoms, differing from one another in size and shape, utterly incapable of being changed from the form which they had maintained from the beginning, were in reality as truly and primordially different as are the primordial elements of Anaxagoras. In other words, the atom of Democritus is nothing less than the primordial seed of Anaxagoras, a little more tangibly visualized and given a distinctive name. Anaxagoras explicitly conceived his elements as invisibly small, as infinite in number, and as made up of an indefinite number of kinds--one for each distinctive substance in the world. But precisely the same postulates are made of the atom of Democritus. These also are invisibly small; these also are infinite in number; these also are made up of an indefinite number of kinds, corresponding with the observed difference of substances in the world. "Primitive seeds," or "atoms," were alike conceived to be primordial, un- changeable, and indestructible. Wherein then lies the difference? We answer, chiefly in a name; almost solely in the fact that Anaxagoras did not attempt to postulate the physical

properties of the elements beyond stating that each has a distinctive personality, while Democritus did attempt to postulate these properties. He, too, admitted that each kind of element has its distinctive personality, and he attempted to visualize and describe the characteristics of the personality.

Thus while Anaxagoras tells us nothing of his elements except that they differ from one another, Democritus postulates a difference in size, imagines some elements as heavier and some as lighter, and conceives even that the elements may be provided with projecting hooks, with the aid of which they link themselves one with another. No one to-day takes these crude visualizings seriously as to their details. The sole element of truth which these dreamings contain, as distinguishing them from the dreamings of Anaxagoras, is in the conception that the various atoms differ in size and weight. Here, indeed, is a vague fore-shadowing of that chemistry of form which began to come into prominence towards the close of the nineteenth century. To have forecast even dimly this newest phase of chemical knowledge, across the abyss of centuries, is indeed a feat to put Democritus in the front rank of thinkers. But this estimate should not blind us to the fact that the pre-vision of Democritus was but a slight elaboration of a theory which had its origin with another thinker. The association between Anaxagoras and Democritus cannot be directly traced, but it is an association which the historian of ideas should never for a moment forget. If we are not to be misled by mere word-jugglery, we shall recognize the founder of the atomic theory of matter in Anaxagoras; its expositors along slightly different lines in Leucippus and Democritus; its re-discoverer of the nineteenth century in Dalton. All in all, then, just as Anaxagoras preceded Democritus in time, so must he take precedence over him also as an inductive thinker, who carried the use of the scientific imagination to its farthest reach.

An analysis of the theories of the two men leads to somewhat the same conclusion that might be reached from a comparison of their lives. Anaxagoras was a sceptical, experimental scientist, gifted also with the prophetic imagination. He reasoned always from the particular to the general, after the manner of true induction, and he scarcely took a step beyond the confines of secure induction. True scientist that he was, he could content himself with postulating different qualities for his elements, without pretending to know how these qualities could be defined. His elements were by hypothesis invisible, hence he would not attempt to visualize them. Democritus, on the other hand, refused to recognize this barrier. Where he could not know, he still did not hesitate to guess. Just as he conceived his atom of a definite form with a definite structure, even so he conceived that the atmosphere about him was full of invisible spirits; he accepted the current superstitions of his time. Like the average Greeks of his day, he even believed in such omens as those furnished by inspecting the entrails of a fowl. These chance bits of biography are weather-vanes of the mind of Democritus. They tend to substantiate our conviction that Democritus must rank below Anaxagoras as a devotee of pure science. But, after all, such comparisons and estimates as this are utterly futile. The essential fact for us is that here, in the fifth century before our era, we find put forward the most penetrating guess as to the constitution of matter that the history of ancient thought has to present to us. In one direction, the avenue of progress is barred; there will be no farther step that way till we come down the centuries to the time of Dalton.

HIPPOCRATES AND GREEK MEDICINE

These studies of the constitution of matter have carried us to the limits of the field of scientific imagination in antiquity; let us now turn sharply and consider a department of science in which

theory joins hands with practicality. Let us witness the beginnings of scientific therapeutics.

Medicine among the early Greeks, before the time of Hippocrates, was a crude mixture of religion, necromancy, and mysticism. Temples were erected to the god of medicine, aesculapius, and sick persons made their way, or were carried, to these temples, where they sought to gain the favor of the god by suitable offerings, and learn the way to regain their health through remedies or methods revealed to them in dreams by the god. When the patient had been thus cured, he placed a tablet in the temple describing his sickness, and telling by what method the god had cured him. He again made suitable offerings at the temple, which were sometimes in the form of gold or silver representations of the diseased organ--a gold or silver model of a heart, hand, foot, etc.

Nevertheless, despite this belief in the supernatural, many drugs and healing lotions were employed, and the Greek physicians possessed considerable skill in dressing wounds and bandaging. But they did not depend upon these surgical dressings alone, using with them certain appropriate prayers and incantations, recited over the injured member at the time of applying the dressings.

Even the very early Greeks had learned something of anatomy. The daily contact with wounds and broken bones must of necessity lead to a crude understanding of anatomy in general. The first Greek anatomist, however, who is recognized as such, is said to have been Alcmaeon. He is said to have made extensive dissections of the lower animals, and to have described many hitherto unknown structures, such as the optic nerve and the Eustachian canal--the small tube leading into the throat from the ear. He is credited with many unique explanations of natural phenomena, such as, for example, the explanation that "hearing is produced by the hollow bone behind the ear; for all hollow things are sonorous." He was a rationalist, and he taught that the brain is the organ of mind. The sources of our information about his work, however, are unreliable.

Democedes, who lived in the sixth century B.C., is the first physician of whom we have any trustworthy history. We learn from Herodotus that he came from Croton to aegina, where, in recognition of his skill, he was appointed medical officer of the city. From aegina he was called to Athens at an increased salary, and later was in charge of medical affairs in several other Greek cities. He was finally called to Samos by the tyrant Polycrates, who reigned there from about 536 to 522 B.C. But on the death of Polycrates, who was murdered by the Persians, Democedes became a slave. His fame as a physician, however, had reached the ears of the Persian monarch, and shortly after his capture he was permitted to show his skill upon King Darius himself. The Persian monarch was suffering from a sprained ankle, which his Egyptian surgeons had been unable to cure. Democedes not only cured the injured member but used his influence in saving the lives of his Egyptian rivals, who had been condemned to death by the king.

At another time he showed his skill by curing the queen, who was suffering from a chronic abscess of long standing. This so pleased the monarch that he offered him as a reward anything he might desire, except his liberty. But the costly gifts of Darius did not satisfy him so long as he remained a slave; and determined to secure his freedom at any cost, he volunteered to lead some Persian spies into his native country, promising to use his influence in converting some of the leading men of his nation to the Persian cause. Laden with the wealth that had been heaped upon him by Darius, he set forth upon his mission, but upon reaching his native city of Croton he

threw off his mask, renounced his Persian mission, and became once more a free Greek.

While the story of Democedes throws little light upon the medical practices of the time, it shows that paid city medical officers existed in Greece as early as the fifth and sixth centuries B.C. Even then there were different "schools" of medicine, whose disciples disagreed radically in their methods of treating diseases; and there were also specialists in certain diseases, quacks, and charlatans. Some physicians depended entirely upon external lotions for healing all disorders; others were "hydrotherapeutists" or "bath- physicians"; while there were a host of physicians who administered a great variety of herbs and drugs. There were also magicians who pretended to heal by sorcery, and great numbers of bone-setters, oculists, and dentists.

Many of the wealthy physicians had hospitals, or clinics, where patients were operated upon and treated. They were not hospitals in our modern understanding of the term, but were more like dispensaries, where patients were treated temporarily, but were not allowed to remain for any length of time. Certain communities established and supported these dispensaries for the care of the poor.

But anything approaching a rational system of medicine was not established, until Hippocrates of Cos, the "father of medicine," came upon the scene. In an age that produced Phidias, Lysias, Herodotus, Sophocles, and Pericles, it seems but natural that the medical art should find an exponent who would rise above superstitious dogmas and lay the foundation for a medical science. His rejection of the supernatural alone stamps the greatness of his genius. But, besides this, he introduced more detailed observation of diseases, and demonstrated the importance that attaches to prognosis.

Hippocrates was born at Cos, about 460 B.C., but spent most of his life at Larissa, in Thessaly. He was educated as a physician by his father, and travelled extensively as an itinerant practitioner for several years. His travels in different climates and among many different people undoubtedly tended to sharpen his keen sense of observation. He was a practical physician as well as a theorist, and, withal, a clear and concise writer. "Life is short," he says, "opportunity fleeting, judgment difficult, treatment easy, but treatment after thought is proper and profitable."

His knowledge of anatomy was necessarily very imperfect, and was gained largely from his predecessors, to whom he gave full credit. Dissections of the human body were forbidden him, and he was obliged to confine his experimental researches to operations on the lower animals. His knowledge of the structure and arrangement of the bones, however, was fairly accurate, but the anatomy of the softer tissues, as he conceived it, was a queer jumbling together of blood-vessels, muscles, and tendons. He does refer to "nerves," to be sure, but apparently the structures referred to are the tendons and ligaments, rather than the nerves themselves. He was better acquainted with the principal organs in the cavities of the body, and knew, for example, that the heart is divided into four cavities, two of which he supposed to contain blood, and the other two air.

His most revolutionary step was his divorcing of the supernatural from the natural, and establishing the fact that disease is due to natural causes and should be treated accordingly. The effect of such an attitude can hardly be over-estimated. The establishment of such a theory was naturally followed by a close observation as to the course of diseases and the effects of treatment. To facilitate this, he introduced the custom of writing down his observations as he

made them--the "clinical history" of the case. Such clinical records are in use all over the world to-day, and their importance is so obvious that it is almost incomprehensible that they should have fallen into disuse shortly after the time of Hippocrates, and not brought into general use again until almost two thousand years later.

But scarcely less important than his recognition of disease as a natural phenomenon was the importance he attributed to prognosis. Prognosis, in the sense of prophecy, was common before the time of Hippocrates. But prognosis, as he practised it and as we understand it to-day, is prophecy based on careful observation of the course of diseases--something more than superstitious conjecture.

Although Hippocratic medicine rested on the belief in natural causes, nevertheless, dogma and theory held an important place. The humoral theory of disease was an all-important one, and so fully was this theory accepted that it influenced the science of medicine all through succeeding centuries. According to this celebrated theory there are four humors in the body-- blood, phlegm, yellow bile, and black bile. When these humors are mixed in exact proportions they constitute health; but any deviations from these proportions produce disease. In treating diseases the aim of the physician was to discover which of these humors were out of proportion and to restore them to their natural equilibrium. It was in the methods employed in this restitution, rather than a disagreement about the humors themselves, that resulted in the various "schools" of medicine.

In many ways the surgery of Hippocrates showed a better understanding of the structure of the organs than of their functions. Some of the surgical procedures as described by him are followed, with slight modifications, to-day. Many of his methods were entirely lost sight of until modern times, and one, the treatment of dislocation of the outer end of the collar-bone, was not revived until some time in the eighteenth century.

Hippocrates, it seems, like modern physicians, sometimes suffered from the ingratitude of his patients. "The physician visits a patient suffering from fever or a wound, and prescribes for him," he says; "on the next day, if the patient feels worse the blame is laid upon the physician; if, on the other hand, he feels better, nature is extolled, and the physician reaps no praise." The essence of this has been repeated in rhyme and prose by writers in every age and country, but the "father of medicine" cautions physicians against allowing it to influence their attitude towards their profession.

VIII. POST-SOCRATIC SCIENCE AT ATHENS--PLATO, ARISTOTLE, AND THEOPHRASTUS

Doubtless it has been noticed that our earlier scientists were as far removed as possible from the limitations of specialism. In point of fact, in this early day, knowledge had not been classified as it came to be later on. The philosopher was, as his name implied, a lover of knowledge, and he did not find it beyond the reach of his capacity to apply himself to all departments of the field of human investigation. It is nothing strange to discover that Anaximander and the Pythagoreans and Anaxagoras have propounded theories regarding the structure of the cosmos, the origin and

development of animals and man, and the nature of matter itself. Nowadays, so enormously involved has become the mass of mere facts regarding each of these departments of knowledge that no one man has the temerity to attempt to master them all. But it was different in those days of beginnings. Then the methods of observation were still crude, and it was quite the custom for a thinker of forceful personality to find an eager following among disciples who never thought of putting his theories to the test of experiment. The great lesson that true science in the last resort depends upon observation and measurement, upon compass and balance, had not yet been learned, though here and there a thinker like Anaxagoras had gained an inkling of it.

For the moment, indeed, there in Attica, which was now, thanks to that outburst of Periclean culture, the centre of the world's civilization, the trend of thought was to take quite another direction. The very year which saw the birth of Democritus at Abdera, and of Hippocrates, marked also the birth, at Athens, of another remarkable man, whose influence it would scarcely be possible to over-estimate. This man was Socrates. The main facts of his history are familiar to every one. It will be recalled that Socrates spent his entire life in Athens, mingling everywhere with the populace; haranguing, so the tradition goes, every one who would listen; inculcating moral lessons, and finally incurring the disapprobation of at least a voting majority of his fellow-citizens. He gathered about him a company of remarkable men with Plato at their head, but this could not save him from the disapprobation of the multitudes, at whose hands he suffered death, legally administered after a public trial. The facts at command as to certain customs of the Greeks at this period make it possible to raise a question as to whether the alleged "corruption of youth," with which Socrates was charged, may not have had a different implication from what posterity has preferred to ascribe to it. But this thought, almost shocking to the modern mind and seeming altogether sacrilegious to most students of Greek philosophy, need not here detain us; neither have we much concern in the present connection with any part of the teaching of the martyred philosopher. For the historian of metaphysics, Socrates marks an epoch, but for the historian of science he is a much less consequential figure.

Similarly regarding Plato, the aristocratic Athenian who sat at the feet of Socrates, and through whose writings the teachings of the master found widest currency. Some students of philosophy find in Plato "the greatest thinker and writer of all time."[1] The student of science must recognize in him a thinker whose point of view was essentially non-scientific; one who tended always to reason from the general to the particular rather than from the particular to the general. Plato's writings covered almost the entire field of thought, and his ideas were presented with such literary charm that successive generations of readers turned to them with unflagging interest, and gave them wide currency through copies that finally preserved them to our own time. Thus we are not obliged in his case, as we are in the case of every other Greek philosopher, to estimate his teachings largely from hearsay evidence. Plato himself speaks to us directly. It is true, the literary form which he always adopted, namely, the dialogue, does not give quite the same certainty as to when he is expressing his own opinions that a more direct narrative would have given; yet, in the main, there is little doubt as to the tenor of his own opinions--except, indeed, such doubt as always attaches to the philosophical reasoning of the abstract thinker.

What is chiefly significant from our present standpoint is that the great ethical teacher had no significant message to give the world regarding the physical sciences. He apparently had no sharply defined opinions as to the mechanism of the universe; no clear conception as to the origin or development of organic beings; no tangible ideas as to the problems of physics; no

favorite dreams as to the nature of matter. Virtually his back was turned on this entire field of thought. He was under the sway of those innate ideas which, as we have urged, were among the earliest inductions of science. But he never for a moment suspected such an origin for these ideas. He supposed his conceptions of being, his standards of ethics, to lie back of all experience; for him they were the most fundamental and most dependable of facts. He criticised Anaxagoras for having tended to deduce general laws from observation. As we moderns see it, such criticism is the highest possible praise. It is a criticism that marks the distinction between the scientist who is also a philosopher and the philosopher who has but a vague notion of physical science. Plato seemed, indeed, to realize the value of scientific investigation; he referred to the astronomical studies of the Egyptians and Chaldeans, and spoke hopefully of the results that might accrue were such studies to be taken up by that Greek mind which, as he justly conceived, had the power to vitalize and enrich all that it touched. But he told here of what he would have others do, not of what he himself thought of doing. His voice was prophetic, but it stimulated no worker of his own time.

Plato himself had travelled widely. It is a familiar legend that he lived for years in Egypt, endeavoring there to penetrate the mysteries of Egyptian science. It is said even that the rudiments of geometry which he acquired there influenced all his later teachings. But be that as it may, the historian of science must recognize in the founder of the Academy a moral teacher and metaphysical dreamer and sociologist, but not, in the modern acceptance of the term, a scientist. Those wider phases of biological science which find their expression in metaphysics, in ethics, in political economy, lie without our present scope; and for the development of those subjects with which we are more directly concerned, Plato, like his master, has a negative significance.

ARISTOTLE (384-322 B.C.)

When we pass to that third great Athenian teacher, Aristotle, the case is far different. Here was a man whose name was to be received as almost a synonym for Greek science for more than a thousand years after his death. All through the Middle Ages his writings were to be accepted as virtually the last word regarding the problems of nature. We shall see that his followers actually preferred his mandate to the testimony of their own senses. We shall see, further, that modern science progressed somewhat in proportion as it overthrew the Aristotelian dogmas. But the traditions of seventeen or eighteen centuries are not easily set aside, and it is perhaps not too much to say that the name of Aristotle stands, even in our own time, as vaguely representative in the popular mind of all that was highest and best in the science of antiquity. Yet, perhaps, it would not be going too far to assert that something like a reversal of this judgment would be nearer the truth. Aristotle did, indeed, bring together a great mass of facts regarding animals in his work on natural history, which, being preserved, has been deemed to entitle its author to be called the "father of zoology." But there is no reason to suppose that any considerable portion of this work contained matter that was novel, or recorded observations that were original with Aristotle; and the classifications there outlined are at best but a vague foreshadowing of the elaboration of the science. Such as it is, however, the natural history stands to the credit of the Stagirite. He must be credited, too, with a clear enunciation of one most important scientific doctrine--namely, the doctrine of the spherical figure of the earth. We have already seen that this theory originated with the Pythagorean philosophers out in Italy. We have seen, too, that the doctrine had not made its way in Attica in the time of Anaxagoras. But in the intervening century it had gained wide currency, else so essentially conservative a thinker as Aristotle would scarcely

have accepted it. He did accept it, however, and gave the doctrine clearest and most precise expression. Here are his words:[2]

"As to the figure of the earth it must necessarily be spherical.... If it were not so, the eclipses of the moon would not have such sections as they have. For in the configurations in the course of a month the deficient part takes all different shapes; it is straight, and concave, and convex; but in eclipses it always has the line of divisions convex; wherefore, since the moon is eclipsed in consequence of the interposition of the earth, the periphery of the earth must be the cause of this by having a spherical form. And again, from the appearance of the stars it is clear, not only that the earth is round, but that its size is not very large; for when we make a small removal to the south or the north, the circle of the horizon becomes palpably different, so that the stars overhead undergo a great change, and are not the same to those that travel in the north and to the south. For some stars are seen in Egypt or at Cyprus, but are not seen in the countries to the north of these; and the stars that in the north are visible while they make a complete circuit, there undergo a setting. So that from this it is manifest, not only that the form of the earth is round, but also that it is a part of a not very large sphere; for otherwise the difference would not be so obvious to persons making so small a change of place. Wherefore we may judge that those persons who connect the region in the neighborhood of the pillars of Hercules with that towards India, and who assert that in this way the sea is one, do not assert things very improbable. They confirm this conjecture moreover by the elephants, which are said to be of the same species towards each extreme; as if this circumstance was a consequence of the conjunction of the extremes. The mathematicians who try to calculate the measure of the circumference, make it amount to four hundred thousand stadia; whence we collect that the earth is not only spherical, but is not large compared with the magnitude of the other stars."

But in giving full meed of praise to Aristotle for the promulgation of this doctrine of the sphericity of the earth, it must unfortunately be added that the conservative philosopher paused without taking one other important step. He could not accept, but, on the contrary, he expressly repudiated, the doctrine of the earth's motion. We have seen that this idea also was a part of the Pythagorean doctrine, and we shall have occasion to dwell more at length on this point in a succeeding chapter. It has even been contended by some critics that it was the adverse conviction of the Peripatetic philosopher which, more than any other single influence, tended to retard the progress of the true doctrine regarding the mechanism of the heavens. Aristotle accepted the sphericity of the earth, and that doctrine became a commonplace of scientific knowledge, and so continued throughout classical antiquity. But Aristotle rejected the doctrine of the earth's motion, and that doctrine, though promulgated actively by a few contemporaries and immediate successors of the Stagirite, was then doomed to sink out of view for more than a thousand years. If it be a correct assumption that the influence of Aristotle was, in a large measure, responsible for this result, then we shall perhaps not be far astray in assuming that the great founder of the Peripatetic school was, on the whole, more instrumental in retarding the progress of astronomical science that any other one man that ever lived.

The field of science in which Aristotle was pre-eminently a pathfinder is zoology. His writings on natural history have largely been preserved, and they constitute by far the most important contribution to the subject that has come down to us from antiquity. They show us that Aristotle had gained possession of the widest range of facts regarding the animal kingdom, and, what is far more important, had attempted to classify these facts. In so doing he became the founder of

systematic zoology. Aristotle's classification of the animal kingdom was known and studied throughout the Middle Ages, and, in fact, remained in vogue until superseded by that of Cuvier in the nineteenth century. It is not to be supposed that all the terms of Aristotle's classification originated with him. Some of the divisions are too patent to have escaped the observation of his predecessors. Thus, for example, the distinction between birds and fishes as separate classes of animals is so obvious that it must appeal to a child or to a savage. But the efforts of Aristotle extended, as we shall see, to less patent generalizations. At the very outset, his grand division of the animal kingdom into blood-bearing and bloodless animals implies a very broad and philosophical conception of the entire animal kingdom. The modern physiologist does not accept the classification, inasmuch as it is now known that colorless fluids perform the functions of blood for all the lower organisms. But the fact remains that Aristotle's grand divisions correspond to the grand divisions of the Lamarckian system--vertebrates and invertebrates--which every one now accepts. Aristotle, as we have said, based his classification upon observation of the blood; Lamarck was guided by a study of the skeleton. The fact that such diverse points of view could direct the observer towards the same result gives, inferentially, a suggestive lesson in what the modern physiologist calls the homologies of parts of the organism.

Aristotle divides his so-called blood-bearing animals into five classes: (1) Four-footed animals that bring forth their young alive; (2) birds; (3) egg-laying four- footed animals (including what modern naturalists call reptiles and amphibians); (4) whales and their allies; (5) fishes. This classification, as will be observed, is not so very far afield from the modern divisions into mammals, birds, reptiles, amphibians, and fishes. That Aristotle should have recognized the fundamental distinction between fishes and the fish- like whales, dolphins, and porpoises proves the far from superficial character of his studies. Aristotle knew that these animals breathe by means of lungs and that they produce living young. He recognized, therefore, their affinity with his first class of animals, even if he did not, like the modern naturalist, consider these affinities close enough to justify bringing the two types together into a single class.

The bloodless animals were also divided by Aristotle into five classes--namely: (1) Cephalopoda (the octopus, cuttle-fish, etc.); (2) weak-shelled animals (crabs, etc.); (3) insects and their allies (including various forms, such as spiders and centipedes, which the modern classifier prefers to place by themselves); (4) hard-shelled animals (clams, oysters, snails, etc.); (5) a conglomerate group of marine forms, including star-fish, sea-urchins, and various anomalous forms that were regarded as linking the animal to the vegetable worlds. This classification of the lower forms of animal life continued in vogue until Cuvier substituted for it his famous grouping into articulates, mollusks, and radiates; which grouping in turn was in part superseded later in the nineteenth century.

What Aristotle did for the animal kingdom his pupil, Theophrastus, did in some measure for the vegetable kingdom. Theophrastus, however, was much less a classifier than his master, and his work on botany, called The Natural History of Development, pays comparatively slight attention to theoretical questions. It deals largely with such practicalities as the making of charcoal, of pitch, and of resin, and the effects of various plants on the animal organism when taken as foods or as medicines. In this regard the work of Theophrastus, is more nearly akin to the natural history of the famous Roman compiler, Pliny. It remained, however, throughout antiquity as the most important work on its subject, and it entitles Theophrastus to be called the "father of botany." Theophrastus deals also with the mineral kingdom after much the same fashion, and

here again his work is the most notable that was produced in antiquity.

IX. GREEK SCIENCE OF THE ALEXANDRIAN OR HELLENISTIC PERIOD

We are entering now upon the most important scientific epoch of antiquity. When Aristotle and Theophrastus passed from the scene, Athens ceased to be in any sense the scientific centre of the world. That city still retained its reminiscent glory, and cannot be ignored in the history of culture, but no great scientific leader was ever again to be born or to take up his permanent abode within the confines of Greece proper. With almost cataclysmic suddenness, a new intellectual centre appeared on the south shore of the Mediterranean. This was the city of Alexandria, a city which Alexander the Great had founded during his brief visit to Egypt, and which became the capital of Ptolemy Soter when he chose Egypt as his portion of the dismembered empire of the great Macedonian. Ptolemy had been with his master in the East, and was with him in Babylonia when he died. He had therefore come personally in contact with Babylonian civilization, and we cannot doubt that this had a most important influence upon his life, and through him upon the new civilization of the West. In point of culture, Alexandria must be regarded as the successor of Babylon, scarcely less directly than of Greece. Following the Babylonian model, Ptolemy erected a great museum and began collecting a library. Before his death it was said that he had collected no fewer than two hundred thousand manuscripts. He had gathered also a company of great teachers and founded a school of science which, as has just been said, made Alexandria the culture-centre of the world.

Athens in the day of her prime had known nothing quite like this. Such private citizens as Aristotle are known to have had libraries, but there were no great public collections of books in Athens, or in any other part of the Greek domain, until Ptolemy founded his famous library. As is well known, such libraries had existed in Babylonia for thousands of years. The character which the Ptolemaic epoch took on was no doubt due to Babylonian influence, but quite as much to the personal experience of Ptolemy himself as an explorer in the Far East. The marvellous conquering journey of Alexander had enormously widened the horizon of the Greek geographer, and stimulated the imagination of all ranks of the people, It was but natural, then, that geography and its parent science astronomy should occupy the attention of the best minds in this succeeding epoch. In point of fact, such a company of star-gazers and earth-measurers came upon the scene in this third century B.C. as had never before existed anywhere in the world. The whole trend of the time was towards mechanics. It was as if the greatest thinkers had squarely faced about from the attitude of the mystical philosophers of the preceding century, and had set themselves the task of solving all the mechanical riddles of the universe, They no longer troubled themselves about problems of "being" and "becoming"; they gave but little heed to metaphysical subtleties; they demanded that their thoughts should be gauged by objective realities. Hence there arose a succession of great geometers, and their conceptions were applied to the construction of new mechanical contrivances on the one hand, and to the elaboration of theories of sidereal mechanics on the other.

The wonderful company of men who performed the feats that are about to be recorded did not all find their home in Alexandria, to be sure; but they all came more or less under the Alexandrian

influence. We shall see that there are two other important centres; one out in Sicily, almost at the confines of the Greek territory in the west; the other in Asia Minor, notably on the island of Samos--the island which, it will be recalled, was at an earlier day the birthplace of Pythagoras. But whereas in the previous century colonists from the confines of the civilized world came to Athens, now all eyes turned towards Alexandria, and so improved were the facilities for communication that no doubt the discoveries of one coterie of workers were known to all the others much more quickly than had ever been possible before. We learn, for example, that the studies of Aristarchus of Samos were definitely known to Archimedes of Syracuse, out in Sicily. Indeed, as we shall see, it is through a chance reference preserved in one of the writings of Archimedes that one of the most important speculations of Aristarchus is made known to us. This illustrates sufficiently the intercommunication through which the thought of the Alexandrian epoch was brought into a single channel. We no longer, as in the day of the earlier schools of Greek philosophy, have isolated groups of thinkers. The scientific drama is now played out upon a single stage; and if we pass, as we shall in the present chapter, from Alexandria to Syracuse and from Syracuse to Samos, the shift of scenes does no violence to the dramatic unities.

Notwithstanding the number of great workers who were not properly Alexandrians, none the less the epoch is with propriety termed Alexandrian. Not merely in the third century B.C., but throughout the lapse of at least four succeeding centuries, the city of Alexander and the Ptolemies continued to hold its place as the undisputed culture-centre of the world. During that period Rome rose to its pinnacle of glory and began to decline, without ever challenging the intellectual supremacy of the Egyptian city. We shall see, in a later chapter, that the Alexandrian influences were passed on to the Mohammedan conquerors, and every one is aware that when Alexandria was finally overthrown its place was taken by another Greek city, Byzantium or Constantinople. But that transfer did not occur until Alexandria had enjoyed a longer period of supremacy as an intellectual centre than had perhaps ever before been granted to any city, with the possible exception of Babylon.

EUCLID (ABOUT 300 B.C.)

Our present concern is with that first wonderful development of scientific activity which began under the first Ptolemy, and which presents, in the course of the first century of Alexandrian influence, the most remarkable coterie of scientific workers and thinkers that antiquity produced. The earliest group of these new leaders in science had at its head a man whose name has been a household word ever since. This was Euclid, the father of systematic geometry. Tradition has preserved to us but little of the personality of this remarkable teacher; but, on the other hand, his most important work has come down to us in its entirety. The Elements of Geometry, with which the name of Euclid is associated in the mind of every school-boy, presented the chief propositions of its subject in so simple and logical a form that the work remained a textbook everywhere for more than two thousand years. Indeed it is only now beginning to be superseded. It is not twenty years since English mathematicians could deplore the fact that, despite certain rather obvious defects of the work of Euclid, no better textbook than this was available. Euclid's work, of course, gives expression to much knowledge that did not originate with him. We have already seen that several important propositions of geometry had been developed by Thales, and one by Pythagoras, and that the rudiments of the subject were at least as old as Egyptian civilization. Precisely how much Euclid added through his own investigations cannot be

ascertained. It seems probable that he was a diffuser of knowledge rather than an originator, but as a great teacher his fame is secure. He is credited with an epigram which in itself might insure him perpetuity of fame: "There is no royal road to geometry," was his answer to Ptolemy when that ruler had questioned whether the Elements might not be simplified. Doubtless this, like most similar good sayings, is apocryphal; but whoever invented it has made the world his debtor.

HEROPHILUS AND ERASISTRATUS

The catholicity of Ptolemy's tastes led him, naturally enough, to cultivate the biological no less than the physical sciences. In particular his influence permitted an epochal advance in the field of medicine. Two anatomists became famous through the investigations they were permitted to make under the patronage of the enlightened ruler. These earliest of really scientific investigators of the mechanism of the human body were named Herophilus and Erasistratus. These two anatomists gained their knowledge by the dissection of human bodies (theirs are the first records that we have of such practices), and King Ptolemy himself is said to have been present at some of these dissections. They were the first to discover that the nerve- trunks have their origin in the brain and spinal cord, and they are credited also with the discovery that these nerve-trunks are of two different kinds--one to convey motor, and the other sensory impulses. They discovered, described, and named the coverings of the brain. The name of Herophilus is still applied by anatomists, in honor of the discoverer, to one of the sinuses or large canals that convey the venous blood from the head. Herophilus also noticed and described four cavities or ventricles in the brain, and reached the conclusion that one of these ventricles was the seat of the soul--a belief shared until comparatively recent times by many physiologists. He made also a careful and fairly accurate study of the anatomy of the eye, a greatly improved the old operation for cataract.

With the increased knowledge of anatomy came also corresponding advances in surgery, and many experimental operations are said to have been performed upon condemned criminals who were handed over to the surgeons by the Ptolemies. While many modern writers have attempted to discredit these assertions, it is not improbable that such operations were performed. In an age when human life was held so cheap, and among a people accustomed to torturing condemned prisoners for comparatively slight offences, it is not unlikely that the surgeons were allowed to inflict perhaps less painful tortures in the cause of science. Furthermore, we know that condemned criminals were sometimes handed over to the medical profession to be "operated upon and killed in whatever way they thought best" even as late as the sixteenth century. Tertullian[1] probably exaggerates, however, when he puts the number of such victims in Alexandria at six hundred.

Had Herophilus and Erasistratus been as happy in their deductions as to the functions of the organs as they were in their knowledge of anatomy, the science of medicine would have been placed upon a very high plane even in their time. Unfortunately, however, they not only drew erroneous inferences as to the functions of the organs, but also disagreed radically as to what functions certain organs performed, and how diseases should be treated, even when agreeing perfectly on the subject of anatomy itself. Their contribution to the knowledge of the scientific treatment of diseases holds no such place, therefore, as their anatomical investigations.

Half a century after the time of Herophilus there appeared a Greek physician, Heraclides, whose reputation in the use of drugs far surpasses that of the anatomists of the Alexandrian school. His reputation has been handed down through the centuries as that of a physician, rather than a

surgeon, although in his own time he was considered one of the great surgeons of the period. Heraclides belonged to the "Empiric" school, which rejected anatomy as useless, depending entirely on the use of drugs. He is thought to have been the first physician to point out the value of opium in certain painful diseases. His prescription of this drug for certain cases of "sleeplessness, spasm, cholera, and colic," shows that his use of it was not unlike that of the modern physician in certain cases; and his treatment of fevers, by keeping the patient's head cool and facilitating the secretions of the body, is still recognized as "good practice." He advocated a free use of liquids in quenching the fever patient's thirst--a recognized therapeutic measure to-day, but one that was widely condemned a century ago.

ARCHIMEDES OF SYRACUSE AND THE FOUNDATION OF MECHANICS

We do not know just when Euclid died, but as he was at the height of his fame in the time of Ptolemy I., whose reign ended in the year 285 B.C., it is hardly probable that he was still living when a young man named Archimedes came to Alexandria to study. Archimedes was born in the Greek colony of Syracuse, on the island of Sicily, in the year 287 B.C. When he visited Alexandria he probably found Apollonius of Perga, the pupil of Euclid, at the head of the mathematical school there. Just how long Archimedes remained at Alexandria is not known. When he had satisfied his curiosity or completed his studies, he returned to Syracuse and spent his life there, chiefly under the patronage of King Hiero, who seems fully to have appreciated his abilities.

Archimedes was primarily a mathematician. Left to his own devices, he would probably have devoted his entire time to the study of geometrical problems. But King Hiero had discovered that his protege had wonderful mechanical ingenuity, and he made good use of this discovery. Under stress of the king's urgings, the philosopher was led to invent a great variety of mechanical contrivances, some of them most curious ones. Antiquity credited him with the invention of more than forty machines, and it is these, rather than his purely mathematical discoveries, that gave his name popular vogue both among his contemporaries and with posterity. Every one has heard of the screw of Archimedes, through which the paradoxical effect was produced of making water seem to flow up hill. The best idea of this curious mechanism is obtained if one will take in hand an ordinary corkscrew, and imagine this instrument to be changed into a hollow tube, retaining precisely the same shape but increased to some feet in length and to a proportionate diameter. If one will hold the corkscrew in a slanting direction and turn it slowly to the right, supposing that the point dips up a portion of water each time it revolves, one can in imagination follow the flow of that portion of water from spiral to spiral, the water always running downward, of course, yet paradoxically being lifted higher and higher towards the base of the corkscrew, until finally it pours out (in the actual Archimedes' tube) at the top. There is another form of the screw in which a revolving spiral blade operates within a cylinder, but the principle is precisely the same. With either form water may be lifted, by the mere turning of the screw, to any desired height. The ingenious mechanism excited the wonder of the contemporaries of Archimedes, as well it might. More efficient devices have superseded it in modern times, but it still excites the admiration of all who examine it, and its effects seem as paradoxical as ever.

Some other of the mechanisms of Archimedes have been made known to successive generations of readers through the pages of Polybius and Plutarch. These are the devices through which Archimedes aided King Hiero to ward off the attacks of the Roman general Marcellus, who in

the course of the second Punic war laid siege to Syracuse.

Plutarch, in his life of Marcellus, describes the Roman's attack and Archimedes' defence in much detail. Incidentally he tells us also how Archimedes came to make the devices that rendered the siege so famous:

"Marcellus himself, with threescore galleys of five rowers at every bank, well armed and full of all sorts of artillery and fireworks, did assault by sea, and rowed hard to the wall, having made a great engine and device of battery, upon eight galleys chained together, to batter the wall: trusting in the great multitude of his engines of battery, and to all such other necessary provision as he had for wars, as also in his own reputation. But Archimedes made light account of all his devices, as indeed they were nothing comparable to the engines himself had invented. This inventive art to frame instruments and engines (which are called mechanical, or organical, so highly commended and esteemed of all sorts of people) was first set forth by Architas, and by Eudoxus: partly to beautify a little the science of geometry by this fineness, and partly to prove and confirm by material examples and sensible instruments, certain geometrical conclusions, where of a man cannot find out the conceivable demonstrations by enforced reasons and proofs. As that conclusion which instructeth one to search out two lines mean proportional, which cannot be proved by reason demonstrative, and yet notwithstanding is a principle and an accepted ground for many things which are contained in the art of portraiture. Both of them have fashioned it to the workmanship of certain instruments, called mesolabes or mesographs, which serve to find these mean lines proportional, by drawing certain curve lines, and overthwart and oblique sections. But after that Plato was offended with them, and maintained against them, that they did utterly corrupt and disgrace, the worthiness and excellence of geometry, making it to descend from things not comprehensible and without body, unto things sensible and material, and to bring it to a palpable substance, where the vile and base handiwork of man is to be employed: since that time, I say, handicraft, or the art of engines, came to be separated from geometry, and being long time despised by the philosophers, it came to be one of the warlike arts.

"But Archimedes having told King Hiero, his kinsman and friend, that it was possible to remove as great a weight as he would, with as little strength as he listed to put to it: and boasting himself thus (as they report of him) and trusting to the force of his reasons, wherewith he proved this conclusion, that if there were another globe of earth, he was able to remove this of ours, and pass it over to the other: King Hiero wondering to hear him, required him to put his device in execution, and to make him see by experience, some great or heavy weight removed, by little force. So Archimedes caught hold with a book of one of the greatest carects, or hulks of the king (that to draw it to the shore out of the water required a marvellous number of people to go about it, and was hardly to be done so) and put a great number of men more into her, than her ordinary burden: and he himself sitting alone at his ease far off, without any straining at all, drawing the end of an engine with many wheels and pulleys, fair and softly with his hand, made it come as gently and smoothly to him, as it had floated in the sea. The king wondering to see the sight, and knowing by proof the greatness of his art; be prayed him to make him some engines, both to assault and defend, in all manner of sieges and assaults. So Archimedes made him many engines, but King Hiero never occupied any of them, because he reigned the most part of his time in peace without any wars. But this provision and munition of engines, served the Syracusan's turn marvellously at that time: and not only the provision of the engines ready made, but also the

engineer and work-master himself, that had invented them.

"Now the Syracusans, seeing themselves assaulted by the Romans, both by sea and by land, were marvellously perplexed, and could not tell what to say, they were so afraid: imagining it was impossible for them to withstand so great an army. But when Archimedes fell to handling his engines, and to set them at liberty, there flew in the air infinite kinds of shot, and marvellous great stones, with an incredible noise and force on the sudden, upon the footmen that came to assault the city by land, bearing down, and tearing in pieces all those which came against them, or in what place soever they lighted, no earthly body being able to resist the violence of so heavy a weight: so that all their ranks were marvellously disordered. And as for the galleys that gave assault by sea, some were sunk with long pieces of timber like unto the yards of ships, whereto they fasten their sails, which were suddenly blown over the walls with force of their engines into their galleys, and so sunk them by their over great weight."

Polybius describes what was perhaps the most important of these contrivances, which was, he tells us, "a band of iron, hanging by a chain from the beak of a machine, which was used in the following manner. The person who, like a pilot, guided the beak, having let fall the hand, and catched hold of the prow of any vessel, drew down the opposite end of the machine that was on the inside of the walls. And when the vessel was thus raised erect upon its stem, the machine itself was held immovable; but, the chain being suddenly loosened from the beak by the means of pulleys, some of the vessels were thrown upon their sides, others turned with the bottom upwards; and the greatest part, as the prows were plunged from a considerable height into the sea, were filled with water, and all that were on board thrown into tumult and disorder.

"Marcellus was in no small degree embarrassed," Polybius continues, "when he found himself encountered in every attempt by such resistance. He perceived that all his efforts were defeated with loss; and were even derided by the enemy. But, amidst all the anxiety that he suffered, he could not help jesting upon the inventions of Archimedes. This man, said he, employs our ships as buckets to draw water: and boxing about our sackbuts, as if they were unworthy to be associated with him, drives them from his company with disgrace. Such was the success of the siege on the side of the sea."

Subsequently, however, Marcellus took the city by strategy, and Archimedes was killed, contrary, it is said, to the express orders of Marcellus. "Syracuse being taken," says Plutarch, "nothing grieved Marcellus more than the loss of Archimedes. Who, being in his study when the city was taken, busily seeking out by himself the demonstration of some geometrical proposition which he had drawn in figure, and so earnestly occupied therein, as he neither saw nor heard any noise of enemies that ran up and down the city, and much less knew it was taken: he wondered when he saw a soldier by him, that bade him go with him to Marcellus. Notwithstanding, he spake to the soldier, and bade him tarry until he had done his conclusion, and brought it to demonstration: but the soldier being angry with his answer, drew out his sword and killed him. Others say, that the Roman soldier when he came, offered the sword's point to him, to kill him: and that Archimedes when he saw him, prayed him to hold his hand a little, that he might not leave the matter he looked for imperfect, without demonstration. But the soldier making no reckoning of his speculation, killed him presently. It is reported a third way also, saying that certain soldiers met him in the streets going to Marcellus, carrying certain mathematical instruments in a little pretty coffer, as dials for the sun, spheres, and angles, wherewith they

measure the greatness of the body of the sun by view: and they supposing he had carried some gold or silver, or other precious jewels in that little coffer, slew him for it. But it is most certain that Marcellus was marvellously sorry for his death, and ever after hated the villain that slew him, as a cursed and execrable person: and how he had made also marvellous much afterwards of Archimedes' kinsmen for his sake."

We are further indebted to Plutarch for a summary of the character and influence of Archimedes, and for an interesting suggestion as to the estimate which the great philosopher put upon the relative importance of his own discoveries. "Notwithstanding Archimedes had such a great mind, and was so profoundly learned, having hidden in him the only treasure and secrets of geometrical inventions: as be would never set forth any book how to make all these warlike engines, which won him at that time the fame and glory, not of man's knowledge, but rather of divine wisdom. But he esteeming all kind of handicraft and invention to make engines, and generally all manner of sciences bringing common commodity by the use of them, to be but vile, beggarly, and mercenary dross: employed his wit and study only to write things, the beauty and subtlety whereof were not mingled anything at all with necessity. For all that he hath written, are geometrical propositions, which are without comparison of any other writings whatsoever: because the subject where of they treat, doth appear by demonstration, the maker gives them the grace and the greatness, and the demonstration proving it so exquisitely, with wonderful reason and facility, as it is not repugnable. For in all geometry are not to be found more profound and difficult matters written, in more plain and simple terms, and by more easy principles, than those which he hath invented. Now some do impute this, to the sharpness of his wit and understanding, which was a natural gift in him: others do refer it to the extreme pains he took, which made these things come so easily from him, that they seemed as if they had been no trouble to him at all. For no man living of himself can devise the demonstration of his propositions, what pains soever he take to seek it: and yet straight so soon as he cometh to declare and open it, every man then imagineth with himself he could have found it out well enough, he can then so plainly make demonstration of the thing he meaneth to show. And therefore that methinks is likely to be true, which they write of him: that he was so ravished and drunk with the sweet enticements of this siren, which as it were lay continually with him, as he forgot his meat and drink, and was careless otherwise of himself, that oftentimes his servants got him against his will to the baths to wash and anoint him: and yet being there, he would ever be drawing out of the geometrical figures, even in the very imbers of the chimney. And while they were anointing of him with oils and sweet savours, with his finger he did draw lines upon his naked body: so far was he taken from himself, and brought into an ecstasy or trance, with the delight he had in the study of geometry, and truly ravished with the love of the Muses. But amongst many notable things he devised, it appeareth, that he most esteemed the demonstration of the proportion between the cylinder (to wit, the round column) and the sphere or globe contained in the same: for he prayed his kinsmen and friends, that after his death they would put a cylinder upon his tomb, containing a massy sphere, with an inscription of the proportion, whereof the continent exceedeth the thing contained."[2]

It should be observed that neither Polybius nor Plutarch mentions the use of burning-glasses in connection with the siege of Syracuse, nor indeed are these referred to by any other ancient writer of authority. Nevertheless, a story gained credence down to a late day to the effect that Archimedes had set fire to the fleet of the enemy with the aid of concave mirrors. An experiment was made by Sir Isaac Newton to show the possibility of a phenomenon so well in accord with

the genius of Archimedes, but the silence of all the early authorities makes it more than doubtful whether any such expedient was really adopted.

It will be observed that the chief principle involved in all these mechanisms was a capacity to transmit great power through levers and pulleys, and this brings us to the most important field of the Syracusan philosopher's activity. It was as a student of the lever and the pulley that Archimedes was led to some of his greatest mechanical discoveries. He is even credited with being the discoverer of the compound pulley. More likely he was its developer only, since the principle of the pulley was known to the old Babylonians, as their sculptures testify. But there is no reason to doubt the general outlines of the story that Archimedes astounded King Hiero by proving that, with the aid of multiple pulleys, the strength of one man could suffice to drag the largest ship from its moorings.

The property of the lever, from its fundamental principle, was studied by him, beginning with the self- evident fact that "equal bodies at the ends of the equal arms of a rod, supported on its middle point, will balance each other"; or, what amounts to the same thing stated in another way, a regular cylinder of uniform matter will balance at its middle point. From this starting-point he elaborated the subject on such clear and satisfactory principles that they stand to-day practically unchanged and with few additions. From all his studies and experiments he finally formulated the principle that "bodies will be in equilibrio when their distance from the fulcrum or point of support is inversely as their weight." He is credited with having summed up his estimate of the capabilities of the lever with the well-known expression, "Give me a fulcrum on which to rest or a place on which to stand, and I will move the earth."

But perhaps the feat of all others that most appealed to the imagination of his contemporaries, and possibly also the one that had the greatest bearing upon the position of Archimedes as a scientific discoverer, was the one made familiar through the tale of the crown of Hiero. This crown, so the story goes, was supposed to be made of solid gold, but King Hiero for some reason suspected the honesty of the jeweller, and desired to know if Archimedes could devise a way of testing the question without injuring the crown. Greek imagination seldom spoiled a story in the telling, and in this case the tale was allowed to take on the most picturesque of phases. The philosopher, we are assured, pondered the problem for a long time without succeeding, but one day as he stepped into a bath, his attention was attracted by the overflow of water. A new train of ideas was started in his ever-receptive brain. Wild with enthusiasm he sprang from the bath, and, forgetting his robe, dashed along the streets of Syracuse, shouting: "Eureka! Eureka!" (I have found it!) The thought that had come into his mind was this: That any heavy substance must have a bulk proportionate to its weight; that gold and silver differ in weight, bulk for bulk, and that the way to test the bulk of such an irregular object as a crown was to immerse it in water. The experiment was made. A lump of pure gold of the weight of the crown was immersed in a certain receptacle filled with water, and the overflow noted. Then a lump of pure silver of the same weight was similarly immersed; lastly the crown itself was immersed, and of course--for the story must not lack its dramatic sequel--was found bulkier than its weight of pure gold. Thus the genius that could balk warriors and armies could also foil the wiles of the silversmith.

Whatever the truth of this picturesque narrative, the fact remains that some, such experiments as these must have paved the way for perhaps the greatest of all the studies of Archimedes--those that relate to the buoyancy of water. Leaving the field of fable, we must now examine these with

some precision. Fortunately, the writings of Archimedes himself are still extant, in which the results of his remarkable experiments are related, so we may present the results in the words of the discoverer.

Here they are: "First: The surface of every coherent liquid in a state of rest is spherical, and the centre of the sphere coincides with the centre of the earth. Second: A solid body which, bulk for bulk, is of the same weight as a liquid, if immersed in the liquid will sink so that the surface of the body is even with the surface of the liquid, but will not sink deeper. Third: Any solid body which is lighter, bulk for bulk, than a liquid, if placed in the liquid will sink so deep as to displace the mass of liquid equal in weight to another body. Fourth: If a body which is lighter than a liquid is forcibly immersed in the liquid, it will be pressed upward with a force corresponding to the weight of a like volume of water, less the weight of the body itself. Fifth: Solid bodies which, bulk for bulk, are heavier than a liquid, when immersed in the liquid sink to the bottom, but become in the liquid as much lighter as the weight of the displaced water itself differs from the weight of the solid." These propositions are not difficult to demonstrate, once they are conceived, but their discovery, combined with the discovery of the laws of statics already referred to, may justly be considered as proving Archimedes the most inventive experimenter of antiquity.

Curiously enough, the discovery which Archimedes himself is said to have considered the most important of all his innovations is one that seems much less striking. It is the answer to the question, What is the relation in bulk between a sphere and its circumscribing cylinder? Archimedes finds that the ratio is simply two to three. We are not informed as to how he reached his conclusion, but an obvious method would be to immerse a ball in a cylindrical cup. The experiment is one which any one can make for himself, with approximate accuracy, with the aid of a tumbler and a solid rubber ball or a billiard-ball of just the right size. Another geometrical problem which Archimedes solved was the problem as to the size of a triangle which has equal area with a circle; the answer being, a triangle having for its base the circumference of the circle and for its altitude the radius. Archimedes solved also the problem of the relation of the diameter of the circle to its circumference; his answer being a close approximation to the familiar 3.1416, which every tyro in geometry will recall as the equivalent of pi.

Numerous other of the studies of Archimedes having reference to conic sections, properties of curves and spirals, and the like, are too technical to be detailed here. The extent of his mathematical knowledge, however, is suggested by the fact that he computed in great detail the number of grains of sand that would be required to cover the sphere of the sun's orbit, making certain hypothetical assumptions as to the size of the earth and the distance of the sun for the purposes of argument. Mathematicians find his computation peculiarly interesting because it evidences a crude conception of the idea of logarithms. From our present stand-point, the paper in which this calculation is contained has considerable interest because of its assumptions as to celestial mechanics. Thus Archimedes starts out with the preliminary assumption that the circumference of the earth is less than three million stadia. It must be understood that this assumption is purely for the sake of argument. Archimedes expressly states that he takes this number because it is "ten times as large as the earth has been supposed to be by certain investigators." Here, perhaps, the reference is to Eratosthenes, whose measurement of the earth we shall have occasion to revert to in a moment. Continuing, Archimedes asserts that the sun is larger than the earth, and the earth larger than the moon. In this assumption, he says, he is

following the opinion of the majority of astronomers. In the third place, Archimedes assumes that the diameter of the sun is not more than thirty times greater than that of the moon. Here he is probably basing his argument upon another set of measurements of Aristarchus, to which, also, we shall presently refer more at length. In reality, his assumption is very far from the truth, since the actual diameter of the sun, as we now know, is something like four hundred times that of the moon. Fourth, the circumference of the sun is greater than one side of the thousand- faced figure inscribed in its orbit. The measurement, it is expressly stated, is based on the measurements of Aristarchus, who makes the diameter of the sun 1/170 of its orbit. Archimedes adds, however, that he himself has measured the angle and that it appears to him to be less than 1/164, and greater than 1/200 part of the orbit. That is to say, reduced to modern terminology, he places the limit of the sun's apparent size between thirty-three minutes and twenty-seven minutes of arc. As the real diameter is thirty-two minutes, this calculation is surprisingly exact, considering the implements then at command. But the honor of first making it must be given to Aristarchus and not to Archimedes.

We need not follow Archimedes to the limits of his incomprehensible numbers of sand-grains. The calculation is chiefly remarkable because it was made before the introduction of the so-called Arabic numerals had simplified mathematical calculations. It will be recalled that the Greeks used letters for numerals, and, having no cipher, they soon found themselves in difficulties when large numbers were involved. The Roman system of numerals simplified the matter somewhat, but the beautiful simplicity of the decimal system did not come into vogue until the Middle Ages, as we shall see. Notwithstanding the difficulties, however, Archimedes followed out his calculations to the piling up of bewildering numbers, which the modern mathematician finds to be the consistent outcome of the problem he had set himself.

But it remains to notice the most interesting feature of this document in which the calculation of the sand- grains is contained. "It was known to me," says Archimedes, "that most astronomers understand by the expression 'world' (universe) a ball of which the centre is the middle point of the earth, and of which the radius is a straight line between the centre of the earth and the sun." Archimedes himself appears to accept this opinion of the majority,--it at least serves as well as the contrary hypothesis for the purpose of his calculation,--but he goes on to say: "Aristarchus of Samos, in his writing against the astronomers, seeks to establish the fact that the world is really very different from this. He holds the opinion that the fixed stars and the sun are immovable and that the earth revolves in a circular line about the sun, the sun being at the centre of this circle." This remarkable bit of testimony establishes beyond question the position of Aristarchus of Samos as the Copernicus of antiquity. We must make further inquiry as to the teachings of the man who had gained such a remarkable insight into the true system of the heavens.

ARISTARCHUS OF SAMOS, THE COPERNICUS OF ANTIQUITY

It appears that Aristarchus was a contemporary of Archimedes, but the exact dates of his life are not known. He was actively engaged in making astronomical observations in Samos somewhat before the middle of the third century B.C.; in other words, just at the time when the activities of the Alexandrian school were at their height. Hipparchus, at a later day, was enabled to compare his own observations with those made by Aristarchus, and, as we have just seen, his work was well known to so distant a contemporary as Archimedes. Yet the facts of his life are almost a blank for us, and of his writings only a single one has been preserved. That one, however, is a

most important and interesting paper on the measurements of the sun and the moon. Unfortunately, this paper gives us no direct clew as to the opinions of Aristarchus concerning the relative positions of the earth and sun. But the testimony of Archimedes as to this is unequivocal, and this testimony is supported by other rumors in themselves less authoritative.

In contemplating this astronomer of Samos, then, we are in the presence of a man who had solved in its essentials the problem of the mechanism of the solar system. It appears from the words of Archimedes that Aristarchus; had propounded his theory in explicit writings. Unquestionably, then, he held to it as a positive doctrine, not as a mere vague guess. We shall show, in a moment, on what grounds he based his opinion. Had his teaching found vogue, the story of science would be very different from what it is. We should then have no tale to tell of a Copernicus coming upon the scene fully seventeen hundred years later with the revolutionary doctrine that our world is not the centre of the universe. We should not have to tell of the persecution of a Bruno or of a Galileo for teaching this doctrine in the seventeenth century of an era which did not begin till two hundred years after the death of Aristarchus. But, as we know, the teaching of the astronomer of Samos did not win its way. The old conservative geocentric doctrine, seemingly so much more in accordance with the every-day observations of mankind, supported by the majority of astronomers with the Peripatetic philosophers at their head, held its place. It found fresh supporters presently among the later Alexandrians, and so fully eclipsed the heliocentric view that we should scarcely know that view had even found an advocate were it not for here and there such a chance record as the phrases we have just quoted from Archimedes. Yet, as we now see, the heliocentric doctrine, which we know to be true, had been thought out and advocated as the correct theory of celestial mechanics by at least one worker of the third century B.C. Such an idea, we may be sure, did not spring into the mind of its originator except as the culmination of a long series of observations and inferences. The precise character of the evolution we perhaps cannot trace, but its broader outlines are open to our observation, and we may not leave so important a topic without at least briefly noting them.

Fully to understand the theory of Aristarchus, we must go back a century or two and recall that as long ago as the time of that other great native of Samos, Pythagoras, the conception had been reached that the earth is in motion. We saw, in dealing with Pythagoras, that we could not be sure as to precisely what he himself taught, but there is no question that the idea of the world's motion became from an early day a so-called Pythagorean doctrine. While all the other philosophers, so far as we know, still believed that the world was flat, the Pythagoreans out in Italy taught that the world is a sphere and that the apparent motions of the heavenly bodies are really due to the actual motion of the earth itself. They did not, however, vault to the conclusion that this true motion of the earth takes place in the form of a circuit about the sun. Instead of that, they conceived the central body of the universe to be a great fire, invisible from the earth, because the inhabited side of the terrestrial ball was turned away from it. The sun, it was held, is but a great mirror, which reflects the light from the central fire. Sun and earth alike revolve about this great fire, each in its own orbit. Between the earth and the central fire there was, curiously enough, supposed to be an invisible earthlike body which was given the name of Anticthon, or counter-earth. This body, itself revolving about the central fire, was supposed to shut off the central light now and again from the sun or from the moon, and thus to account for certain eclipses for which the shadow of the earth did not seem responsible. It was, perhaps, largely to account for such eclipses that the counter-earth was invented. But it is supposed that there was another reason. The Pythagoreans held that there is a peculiar sacredness in the number ten. Just

as the Babylonians of the early day and the Hegelian philosophers of a more recent epoch saw a sacred connection between the number seven and the number of planetary bodies, so the Pythagoreans thought that the universe must be arranged in accordance with the number ten. Their count of the heavenly bodies, including the sphere of the fixed stars, seemed to show nine, and the counter-earth supplied the missing body.

The precise genesis and development of this idea cannot now be followed, but that it was prevalent about the fifth century B.C. as a Pythagorean doctrine cannot be questioned. Anaxagoras also is said to have taken account of the hypothetical counter-earth in his explanation of eclipses; though, as we have seen, he probably did not accept that part of the doctrine which held the earth to be a sphere. The names of Philolaus and Heraclides have been linked with certain of these Pythagorean doctrines. Eudoxus, too, who, like the others, lived in Asia Minor in the fourth century B.C., was held to have made special studies of the heavenly spheres and perhaps to have taught that the earth moves. So, too, Nicetas must be named among those whom rumor credited with having taught that the world is in motion. In a word, the evidence, so far as we can garner it from the remaining fragments, tends to show that all along, from the time of the early Pythagoreans, there had been an undercurrent of opinion in the philosophical world which questioned the fixity of the earth; and it would seem that the school of thinkers who tended to accept the revolutionary view centred in Asia Minor, not far from the early home of the founder of the Pythagorean doctrines. It was not strange, then, that the man who was finally to carry these new opinions to their logical conclusion should hail from Samos.

But what was the support which observation could give to this new, strange conception that the heavenly bodies do not in reality move as they seem to move, but that their apparent motion is due to the actual revolution of the earth? It is extremely difficult for any one nowadays to put himself in a mental position to answer this question. We are so accustomed to conceive the solar system as we know it to be, that we are wont to forget how very different it is from what it seems. Yet one needs but to glance up at the sky, and then to glance about one at the solid earth, to grant, on a moment's reflection, that the geocentric idea is of all others the most natural; and that to conceive the sun as the actual Centre of the solar system is an idea which must look for support to some other evidence than that which ordinary observation can give. Such was the view of most of the ancient philosophers, and such continued to be the opinion of the majority of mankind long after the time of Copernicus. We must not forget that even so great an observing astronomer as Tycho Brahe, so late as the seventeenth century, declined to accept the heliocentric theory, though admitting that all the planets except the earth revolve about the sun. We shall see that before the Alexandrian school lost its influence a geocentric scheme had been evolved which fully explained all the apparent motions of the heavenly bodies. All this, then, makes us but wonder the more that the genius of an Aristarchus could give precedence to scientific induction as against the seemingly clear evidence of the senses.

What, then, was the line of scientific induction that led Aristarchus to this wonderful goal? Fortunately, we are able to answer that query, at least in part. Aristarchus gained his evidence through some wonderful measurements. First, he measured the disks of the sun and the moon. This, of course, could in itself give him no clew to the distance of these bodies, and therefore no clew as to their relative size; but in attempting to obtain such a clew he hit upon a wonderful yet altogether simple experiment. It occurred to him that when the moon is precisely dichotomized--that is to say, precisely at the half-the line of vision from the earth to the moon must be precisely

at right angles with the line of light passing from the sun to the moon. At this moment, then, the imaginary lines joining the sun, the moon, and the earth, make a right angle triangle. But the properties of the right-angle triangle had long been studied and were well under stood. One acute angle of such a triangle determines the figure of the triangle itself. We have already seen that Thales, the very earliest of the Greek philosophers, measured the distance of a ship at sea by the application of this principle. Now Aristarchus sights the sun in place of Thales' ship, and, sighting the moon at the same time, measures the angle and establishes the shape of his right-angle triangle. This does not tell him the distance of the sun, to be sure, for he does not know the length of his base-line--that is to say, of the line between the moon and the earth. But it does establish the relation of that base-line to the other lines of the triangle; in other words, it tells him the distance of the sun in terms of the moon's distance. As Aristarchus strikes the angle, it shows that the sun is eighteen times as distant as the moon. Now, by comparing the apparent size of the sun with the apparent size of the moon--which, as we have seen, Aristarchus has already measured--he is able to tell us that, the sun is "more than 5832 times, and less than 8000" times larger than the moon; though his measurements, taken by themselves, give no clew to the actual bulk of either body. These conclusions, be it understood, are absolutely valid inferences--nay, demonstrations--from the measurements involved, provided only that these measurements have been correct. Unfortunately, the angle of the triangle we have just seen measured is exceedingly difficult to determine with accuracy, while at the same time, as a moment's reflection will show, it is so large an angle that a very slight deviation from the truth will greatly affect the distance at which its line joins the other side of the triangle. Then again, it is virtually impossible to tell the precise moment when the moon is at half, as the line it gives is not so sharp that we can fix it with absolute accuracy. There is, moreover, another element of error due to the refraction of light by the earth's atmosphere. The experiment was probably made when the sun was near the horizon, at which time, as we now know, but as Aristarchus probably did not suspect, the apparent displacement of the sun's position is considerable; and this displacement, it will be observed, is in the direction to lessen the angle in question.

In point of fact, Aristarchus estimated the angle at eighty-seven degrees. Had his instrument been more precise, and had he been able to take account of all the elements of error, he would have found it eighty-seven degrees and fifty-two minutes. The difference of measurement seems slight; but it sufficed to make the computations differ absurdly from the truth. The sun is really not merely eighteen times but more than two hundred times the distance of the moon, as Wendelein discovered on repeating the experiment of Aristarchus about two thousand years later. Yet this discrepancy does not in the least take away from the validity of the method which Aristarchus employed. Moreover, his conclusion, stated in general terms, was perfectly correct: the sun is many times more distant than the moon and vastly larger than that body. Granted, then, that the moon is, as Aristarchus correctly believed, considerably less in size than the earth, the sun must be enormously larger than the earth; and this is the vital inference which, more than any other, must have seemed to Aristarchus to confirm the suspicion that the sun and not the earth is the centre of the planetary system. It seemed to him inherently improbable that an enormously large body like the sun should revolve about a small one such as the earth. And again, it seemed inconceivable that a body so distant as the sun should whirl through space so rapidly as to make the circuit of its orbit in twenty- four hours. But, on the other hand, that a small body like the earth should revolve about the gigantic sun seemed inherently probable. This proposition granted, the rotation of the earth on its axis follows as a necessary consequence in explanation of the seeming motion of the stars. Here, then, was the heliocentric doctrine reduced to a virtual

demonstration by Aristarchus of Samos, somewhere about the middle of the third century B.C.

It must be understood that in following out the, steps of reasoning by which we suppose Aristarchus to have reached so remarkable a conclusion, we have to some extent guessed at the processes of thought- development; for no line of explication written by the astronomer himself on this particular point has come down to us. There does exist, however, as we have already stated, a very remarkable treatise by Aristarchus on the Size and Distance of the Sun and the Moon, which so clearly suggests the methods of reasoning of the great astronomer, and so explicitly cites the results of his measurements, that we cannot well pass it by without quoting from it at some length. It is certainly one of the most remarkable scientific documents of antiquity. As already noted, the heliocentric doctrine is not expressly stated here. It seems to be tacitly implied throughout, but it is not a necessary consequence of any of the propositions expressly stated. These propositions have to do with certain observations and measurements and what Aristarchus believes to be inevitable deductions from them, and he perhaps did not wish to have these deductions challenged through associating them with a theory which his contemporaries did not accept. In a word, the paper of Aristarchus is a rigidly scientific document unvitiated by association with any theorizings that are not directly germane to its central theme. The treatise opens with certain hypotheses as follows:

"First. The moon receives its light from the sun.

"Second. The earth may be considered as a point and as the centre of the orbit of the moon.

"Third. When the moon appears to us dichotomized it offers to our view a great circle [or actual meridian] of its circumference which divides the illuminated part from the dark part.

"Fourth. When the moon appears dichotomized its distance from the sun is less than a quarter of the circumference [of its orbit] by a thirtieth part of that quarter."

That is to say, in modern terminology, the moon at this time lacks three degrees (one thirtieth of ninety degrees) of being at right angles with the line of the sun as viewed from the earth; or, stated otherwise, the angular distance of the moon from the sun as viewed from the earth is at this time eighty-seven degrees--this being, as we have already observed, the fundamental measurement upon which so much depends. We may fairly suppose that some previous paper of Aristarchus's has detailed the measurement which here is taken for granted, yet which of course could depend solely on observation.

"Fifth. The diameter of the shadow [cast by the earth at the point where the moon's orbit cuts that shadow when the moon is eclipsed] is double the diameter of the moon."

Here again a knowledge of previously established measurements is taken for granted; but, indeed, this is the case throughout the treatise.

"Sixth. The arc subtended in the sky by the moon is a fifteenth part of a sign" of the zodiac; that is to say, since there are twenty-four, signs in the zodiac, one-fifteenth of one twenty-fourth, or in modern terminology, one degree of arc. This is Aristarchus's measurement of the moon to which we have already referred when speaking of the measurements of Archimedes.

"If we admit these six hypotheses," Aristarchus continues, "it follows that the sun is more than

eighteen times more distant from the earth than is the moon, and that it is less than twenty times more distant, and that the diameter of the sun bears a corresponding relation to the diameter of the moon; which is proved by the position of the moon when dichotomized. But the ratio of the diameter of the sun to that of the earth is greater than nineteen to three and less than forty-three to six. This is demonstrated by the relation of the distances, by the position [of the moon] in relation to the earth's shadow, and by the fact that the arc subtended by the moon is a fifteenth part of a sign."

Aristarchus follows with nineteen propositions intended to elucidate his hypotheses and to demonstrate his various contentions. These show a singularly clear grasp of geometrical problems and an altogether correct conception of the general relations as to size and position of the earth, the moon, and the sun. His reasoning has to do largely with the shadow cast by the earth and by the moon, and it presupposes a considerable knowledge of the phenomena of eclipses. His first proposition is that "two equal spheres may always be circumscribed in a cylinder; two unequal spheres in a cone of which the apex is found on the side of the smaller sphere; and a straight line joining the centres of these spheres is perpendicular to each of the two circles made by the contact of the surface of the cylinder or of the cone with the spheres."

It will be observed that Aristarchus has in mind here the moon, the earth, and the sun as spheres to be circumscribed within a cone, which cone is made tangible and measurable by the shadows cast by the non-luminous bodies; since, continuing, he clearly states in proposition nine, that "when the sun is totally eclipsed, an observer on the earth's surface is at an apex of a cone comprising the moon and the sun." Various propositions deal with other relations of the shadows which need not detain us since they are not fundamentally important, and we may pass to the final conclusions of Aristarchus, as reached in his propositions ten to nineteen.

Now, since (proposition ten) "the diameter of the sun is more than eighteen times and less than twenty times greater than that of the moon," it follows (proposition eleven) "that the bulk of the sun is to that of the moon in ratio, greater than 5832 to 1, and less than 8000 to 1."

"Proposition sixteen. The diameter of the sun is to the diameter of the earth in greater proportion than nineteen to three, and less than forty-three to six.

"Proposition seventeen. The bulk of the sun is to that of the earth in greater proportion than 6859 to 27, and less than 79,507 to 216.

"Proposition eighteen. The diameter of the earth is to the diameter of the moon in greater proportion than 108 to 43 and less than 60 to 19.

"Proposition nineteen. The bulk of the earth is to that of the moon in greater proportion than 1,259,712 to 79,507 and less than 20,000 to 6859."

Such then are the more important conclusions of this very remarkable paper--a paper which seems to have interest to the successors of Aristarchus generation after generation, since this alone of all the writings of the great astronomer has been preserved. How widely the exact results of the measurements of Aristarchus, differ from the truth, we have pointed out as we progressed. But let it be repeated that this detracts little from the credit of the astronomer who had such clear and correct conceptions of the relations of the heavenly bodies and who invented

such correct methods of measurement. Let it be particularly observed, however, that all the conclusions of Aristarchus are stated in relative terms. He nowhere attempts to estimate the precise size of the earth, of the moon, or of the sun, or the actual distance of one of these bodies from another. The obvious reason for this is that no data were at hand from which to make such precise measurements. Had Aristarchus known the size of any one of the bodies in question, he might readily, of course, have determined the size of the others by the mere application of his relative scale; but he had no means of determining the size of the earth, and to this extent his system of measurements remained imperfect. Where Aristarchus halted, however, another worker of the same period took the task in hand and by an altogether wonderful measurement determined the size of the earth, and thus brought the scientific theories of cosmology to their climax. This worthy supplementor of the work of Aristarchus was Eratosthenes of Alexandria.

ERATOSTHENES, "THE SURVEYOR OF THE WORLD"

An altogether remarkable man was this native of Cyrene, who came to Alexandria from Athens to be the chief librarian of Ptolemy Euergetes. He was not merely an astronomer and a geographer, but a poet and grammarian as well. His contemporaries jestingly called him Beta the Second, because he was said through the universality of his attainments to be "a second Plato" in philosophy, "a second Thales" in astronomy, and so on throughout the list. He was also called the "surveyor of the world," in recognition of his services to geography. Hipparchus said of him, perhaps half jestingly, that he had studied astronomy as a geographer and geography as an astronomer. It is not quite clear whether the epigram was meant as compliment or as criticism. Similar phrases have been turned against men of versatile talent in every age. Be that as it may, Eratosthenes passed into history as the father of scientific geography and of scientific chronology; as the astronomer who first measured the obliquity of the ecliptic; and as the inventive genius who performed the astounding feat of measuring the size of the globe on which we live at a time when only a relatively small portion of that globe's surface was known to civilized man. It is no discredit to approach astronomy as a geographer and geography as an astronomer if the results are such as these. What Eratosthenes really did was to approach both astronomy and geography from two seemingly divergent points of attack--namely, from the stand-point of the geometer and also from that of the poet. Perhaps no man in any age has brought a better combination of observing and imaginative faculties to the aid of science.

Nearly all the discoveries of Eratosthenes are associated with observations of the shadows cast by the sun. We have seen that, in the study of the heavenly bodies, much depends on the measurement of angles. Now the easiest way in which angles can be measured, when solar angles are in question, is to pay attention, not to the sun itself, but to the shadow that it casts. We saw that Thales made some remarkable measurements with the aid of shadows, and we have more than once referred to the gnomon, which is the most primitive, but which long remained the most important, of astronomical instruments. It is believed that Eratosthenes invented an important modification of the gnomon which was elaborated afterwards by Hipparchus and called an armillary sphere. This consists essentially of a small gnomon, or perpendicular post, attached to a plane representing the earth's equator and a hemisphere in imitation of the earth's surface. With the aid of this, the shadow cast by the sun could be very accurately measured. It involves no new principle. Every perpendicular post or object of any kind placed in the sunlight casts a shadow from which the angles now in question could be roughly measured. The province of the armillary sphere was to make these measurements extremely accurate.

With the aid of this implement, Eratosthenes carefully noted the longest and the shortest shadows cast by the gnomon--that is to say, the shadows cast on the days of the solstices. He found that the distance between the tropics thus measured represented 47 degrees 42' 39" of arc. One-half of this, or 23 degrees 5,' 19.5", represented the obliquity of the ecliptic--that is to say, the angle by which the earth's axis dipped from the perpendicular with reference to its orbit. This was a most important observation, and because of its accuracy it has served modern astronomers well for comparison in measuring the trifling change due to our earth's slow, swinging wobble. For the earth, be it understood, like a great top spinning through space, holds its position with relative but not quite absolute fixity. It must not be supposed, however, that the experiment in question was quite new with Eratosthenes. His merit consists rather in the accuracy with which he made his observation than in the novelty of the conception; for it is recorded that Eudoxus, a full century earlier, had remarked the obliquity of the ecliptic. That observer had said that the obliquity corresponded to the side of a pentadecagon, or fifteen-sided figure, which is equivalent in modern phraseology to twenty- four degrees of arc. But so little is known regarding the way in which Eudoxus reached his estimate that the measurement of Eratosthenes is usually spoken of as if it were the first effort of the kind.

Much more striking, at least in its appeal to the popular imagination, was that other great feat which Eratosthenes performed with the aid of his perfected gnomon--the measurement of the earth itself. When we reflect that at this period the portion of the earth open to observation extended only from the Straits of Gibraltar on the west to India on the east, and from the North Sea to Upper Egypt, it certainly seems enigmatical--at first thought almost miraculous--that an observer should have been able to measure the entire globe. That he should have accomplished this through observation of nothing more than a tiny bit of Egyptian territory and a glimpse of the sun's shadow makes it seem but the more wonderful. Yet the method of Eratosthenes, like many another enigma, seems simple enough once it is explained. It required but the application of a very elementary knowledge of the geometry of circles, combined with the use of a fact or two from local geography--which detracts nothing from the genius of the man who could reason from such simple premises to so wonderful a conclusion.

Stated in a few words, the experiment of Eratosthenes was this. His geographical studies had taught him that the town of Syene lay directly south of Alexandria, or, as we should say, on the same meridian of latitude. He had learned, further, that Syene lay directly under the tropic, since it was reported that at noon on the day of the summer solstice the gnomon there cast no shadow, while a deep well was illumined to the bottom by the sun. A third item of knowledge, supplied by the surveyors of Ptolemy, made the distance between Syene and Alexandria five thousand stadia. These, then, were the preliminary data required by Eratosthenes. Their significance consists in the fact that here is a measured bit of the earth's arc five thousand stadia in length. If we could find out what angle that bit of arc subtends, a mere matter of multiplication would give us the size of the earth. But how determine this all-important number? The answer came through reflection on the relations of concentric circles. If you draw any number of circles, of whatever size, about a given centre, a pair of radii drawn from that centre will cut arcs of the same relative size from all the circles. One circle may be so small that the actual arc subtended by the radii in a given case may be but an inch in length, while another circle is so large that its corresponding are is measured in millions of miles; but in each case the same number of so-called degrees will represent the relation of each arc to its circumference. Now, Eratosthenes knew, as just stated, that the sun, when on the meridian on the day of the summer solstice, was directly over the town

of Syene. This meant that at that moment a radius of the earth projected from Syene would point directly towards the sun. Meanwhile, of course, the zenith would represent the projection of the radius of the earth passing through Alexandria. All that was required, then, was to measure, at Alexandria, the angular distance of the sun from the zenith at noon on the day of the solstice to secure an approximate measurement of the arc of the sun's circumference, corresponding to the arc of the earth's surface represented by the measured distance between Alexandria and Syene.

The reader will observe that the measurement could not be absolutely accurate, because it is made from the surface of the earth, and not from the earth's centre, but the size of the earth is so insignificant in comparison with the distance of the sun that this slight discrepancy could be disregarded.

The way in which Eratosthenes measured this angle was very simple. He merely measured the angle of the shadow which his perpendicular gnomon at Alexandria cast at mid-day on the day of the solstice, when, as already noted, the sun was directly perpendicular at Syene. Now a glance at the diagram will make it clear that the measurement of this angle of the shadow is merely a convenient means of determining the precisely equal opposite angle subtending an arc of an imaginary circle passing through the sun; the are which, as already explained, corresponds with the arc of the earth's surface represented by the distance between Alexandria and Syene. He found this angle to represent 7 degrees 12', or one-fiftieth of the circle. Five thousand stadia, then, represent one-fiftieth of the earth's circumference; the entire circumference being, therefore, 250,000 stadia. Unfortunately, we do not know which one of the various measurements used in antiquity is represented by the stadia of Eratosthenes. According to the researches of Lepsius, however, the stadium in question represented 180 meters, and this would make the earth, according to the measurement of Eratosthenes, about twenty-eight thousand miles in circumference, an answer sufficiently exact to justify the wonder which the experiment excited in antiquity, and the admiration with which it has ever since been regarded.

{illustration caption = DIAGRAM TO ILLUSTRATE ERATOSTHENES' MEASUREMENT OF THE GLOBE

FIG. 1. AF is a gnomon at Alexandria; SB a gnomon at Svene; IS and JK represent the sun's rays. The angle actually measured by Eratosthenes is KFA, as determined by the shadow cast by the gnomon AF. This angle is equal to the opposite angle JFL, which measures the sun's distance from the zenith; and which is also equal to the angle AES--to determine the Size of which is the real object of the entire measurement.

FIG. 2 shows the form of the gnomon actually employed in antiquity. The hemisphere KA being marked with a scale, it is obvious that in actual practice Eratosthenes required only to set his gnomon in the sunlight at the proper moment, and read off the answer to his problem at a glance. The simplicity of the method makes the result seem all the more wonderful.}

Of course it is the method, and not its details or its exact results, that excites our interest. And beyond question the method was an admirable one. Its result, however, could not have been absolutely accurate, because, while correct in principle, its data were defective. In point of fact Syene did not lie precisely on the same meridian as Alexandria, neither did it lie exactly on the tropic. Here, then, are two elements of inaccuracy. Moreover, it is doubtful whether Eratosthenes made allowance, as he should have done, for the semi-diameter of the sun in measuring the angle

of the shadow. But these are mere details, scarcely worthy of mention from our present standpoint. What perhaps is deserving of more attention is the fact that this epoch-making measurement of Eratosthenes may not have been the first one to be made. A passage of Aristotle records that the size of the earth was said to be 400,000 stadia. Some commentators have thought that Aristotle merely referred to the area of the inhabited portion of the earth and not to the circumference of the earth itself, but his words seem doubtfully susceptible of this interpretation; and if he meant, as his words seem to imply, that philosophers of his day had a tolerably precise idea of the globe, we must assume that this idea was based upon some sort of measurement. The recorded size, 400,000 stadia, is a sufficient approximation to the truth to suggest something more than a mere unsupported guess. Now, since Aristotle died more than fifty years before Eratosthenes was born, his report as to the alleged size of the earth certainly has a suggestiveness that cannot be overlooked; but it arouses speculations without giving an inkling as to their solution. If Eratosthenes had a precursor as an earth-measurer, no hint or rumor has come down to us that would enable us to guess who that precursor may have been. His personality is as deeply enveloped in the mists of the past as are the personalities of the great prehistoric discoverers. For the purpose of the historian, Eratosthenes must stand as the inventor of the method with which his name is associated, and as the first man of whom we can say with certainty that he measured the size of the earth. Right worthily, then, had the Alexandrian philosopher won his proud title of "surveyor of the world."

HIPPARCHUS, "THE LOVER OF TRUTH"

Eratosthenes outlived most of his great contemporaries. He saw the turning of that first and greatest century of Alexandrian science, the third century before our era. He died in the year 196 B.C., having, it is said, starved himself to death to escape the miseries of blindness;--to the measurer of shadows, life without light seemed not worth the living. Eratosthenes left no immediate successor. A generation later, however, another great figure appeared in the astronomical world in the person of Hipparchus, a man who, as a technical observer, had perhaps no peer in the ancient world: one who set so high a value upon accuracy of observation as to earn the title of "the lover of truth." Hipparchus was born at Nicaea, in Bithynia, in the year 160 B.C. His life, all too short for the interests of science, ended in the year 125 B.C. The observations of the great astronomer were made chiefly, perhaps entirely, at Rhodes. A misinterpretation of Ptolemy's writings led to the idea that Hipparchus, performed his chief labors in Alexandria, but it is now admitted that there is no evidence for this. Delambre doubted, and most subsequent writers follow him here, whether Hipparchus ever so much as visited Alexandria. In any event there seems to be no question that Rhodes may claim the honor of being the chief site of his activities.

It was Hipparchus whose somewhat equivocal comment on the work of Eratosthenes we have already noted. No counter-charge in kind could be made against the critic himself; he was an astronomer pure and simple. His gift was the gift of accurate observation rather than the gift of imagination. No scientific progress is possible without scientific guessing, but Hipparchus belonged to that class of observers with whom hypothesis is held rigidly subservient to fact. It was not to be expected that his mind would be attracted by the heliocentric theory of Aristarchus. He used the facts and observations gathered by his great predecessor of Samos, but he declined to accept his theories. For him the world was central; his problem was to explain, if he could, the irregularities of motion which sun, moon, and planets showed in their seeming circuits about the

earth. Hipparchus had the gnomon of Eratosthenes--doubtless in a perfected form--to aid him, and he soon proved himself a master in its use. For him, as we have said, accuracy was everything; this was the one element that led to all his great successes.

Perhaps his greatest feat was to demonstrate the eccentricity of the sun's seeming orbit. We of to-day, thanks to Keppler and his followers, know that the earth and the other planetary bodies in their circuit about the sun describe an ellipse and not a circle. But in the day of Hipparchus, though the ellipse was recognized as a geometrical figure (it had been described and named along with the parabola and hyperbola by Apollonius of Perga, the pupil of Euclid), yet it would have been the rankest heresy to suggest an elliptical course for any heavenly body. A metaphysical theory, as propounded perhaps by the Pythagoreans but ardently supported by Aristotle, declared that the circle is the perfect figure, and pronounced it inconceivable that the motions of the spheres should be other than circular. This thought dominated the mind of Hipparchus, and so when his careful measurements led him to the discovery that the northward and southward journeyings of the sun did not divide the year into four equal parts, there was nothing open to him but to either assume that the earth does not lie precisely at the centre of the sun's circular orbit or to find some alternative hypothesis.

In point of fact, the sun (reversing the point of view in accordance with modern discoveries) does lie at one focus of the earth's elliptical orbit, and therefore away from the physical centre of that orbit; in other words, the observations of Hipparchus were absolutely accurate. He was quite correct in finding that the sun spends more time on one side of the equator than on the other. When, therefore, he estimated the relative distance of the earth from the geometrical centre of the sun's supposed circular orbit, and spoke of this as the measure of the sun's eccentricity, he propounded a theory in which true data of observation were curiously mingled with a positively inverted theory. That the theory of Hipparchus was absolutely consistent with all the facts of this particular observation is the best evidence that could be given of the difficulties that stood in the way of a true explanation of the mechanism of the heavens.

But it is not merely the sun which was observed to vary in the speed of its orbital progress; the moon and the planets also show curious accelerations and retardations of motion. The moon in particular received most careful attention from Hipparchus. Dominated by his conception of the perfect spheres, he could find but one explanation of the anomalous motions which he observed, and this was to assume that the various heavenly bodies do not fly on in an unvarying arc in their circuit about the earth, but describe minor circles as they go which can be likened to nothing so tangibly as to a light attached to the rim of a wagon-wheel in motion. If such an invisible wheel be imagined as carrying the sun, for example, on its rim, while its invisible hub follows unswervingly the circle of the sun's mean orbit (this wheel, be it understood, lying in the plane of the orbit, not at right- angles to it), then it must be obvious that while the hub remains always at the same distance from the earth, the circling rim will carry the sun nearer the earth, then farther away, and that while it is traversing that portion of the are which brings it towards the earth, the actual forward progress of the sun will be retarded notwithstanding the uniform motion of the hub, just as it will be accelerated in the opposite arc. Now, if we suppose our sun-bearing wheel to turn so slowly that the sun revolves but once about its imaginary hub while the wheel itself is making the entire circuit of the orbit, we shall have accounted for the observed fact that the sun passes more quickly through one-half of the orbit than through the other. Moreover, if we can visualize the process and imagine the sun to have left a visible line of fire behind him throughout

the course, we shall see that in reality the two circular motions involved have really resulted in producing an elliptical orbit.

The idea is perhaps made clearer if we picture the actual progress of the lantern attached to the rim of an ordinary cart-wheel. When the cart is drawn forward the lantern is made to revolve in a circle as regards the hub of the wheel, but since that hub is constantly going forward, the actual path described by the lantern is not a circle at all but a waving line. It is precisely the same with the imagined course of the sun in its orbit, only that we view these lines just as we should view the lantern on the wheel if we looked at it from directly above and not from the side. The proof that the sun is describing this waving line, and therefore must be considered as attached to an imaginary wheel, is furnished, as it seemed to Hipparchus, by the observed fact of the sun's varying speed.

That is one way of looking at the matter. It is an hypothesis that explains the observed facts--after a fashion, and indeed a very remarkable fashion. The idea of such an explanation did not originate with Hipparchus. The germs of the thought were as old as the Pythagorean doctrine that the earth revolves about a centre that we cannot see. Eudoxus gave the conception greater tangibility, and may be considered as the father of this doctrine of wheels--epicycles, as they came to be called. Two centuries before the time of Hipparchus he conceived a doctrine of spheres which Aristotle found most interesting, and which served to explain, along the lines we have just followed, the observed motions of the heavenly bodies. Calippus, the reformer of the calendar, is said to have carried an account of this theory to Aristotle. As new irregularities of motion of the sun, moon, and planetary bodies were pointed out, new epicycles were invented. There is no limit to the number of imaginary circles that may be inscribed about an imaginary centre, and if we conceive each one of these circles to have a proper motion of its own, and each one to carry the sun in the line of that motion, except as it is diverted by the other motions--if we can visualize this complex mingling of wheels--we shall certainly be able to imagine the heavenly body which lies at the juncture of all the rims, as being carried forward in as erratic and wobbly a manner as could be desired. In other words, the theory of epicycles will account for all the facts of the observed motions of all the heavenly bodies, but in so doing it fills the universe with a most bewildering network of intersecting circles. Even in the time of Calippus fifty-five of these spheres were computed.

We may well believe that the clear-seeing Aristarchus would look askance at such a complex system of imaginary machinery. But Hipparchus, pre-eminently an observer rather than a theorizer, seems to have been content to accept the theory of epicycles as he found it, though his studies added to its complexities; and Hipparchus was the dominant scientific personality of his century. What he believed became as a law to his immediate successors. His tenets were accepted as final by their great popularizer, Ptolemy, three centuries later; and so the heliocentric theory of Aristarchus passed under a cloud almost at the hour of its dawning, there to remain obscured and forgotten for the long lapse of centuries. A thousand pities that the greatest observing astronomer of antiquity could not, like one of his great precursors, have approached astronomy from the stand-point of geography and poetry. Had he done so, perhaps he might have reflected, like Aristarchus before him, that it seems absurd for our earth to hold the giant sun in thraldom; then perhaps his imagination would have reached out to the heliocentric doctrine, and the cobweb hypothesis of epicycles, with that yet more intangible figment of the perfect circle, might have been wiped away.

But it was not to be. With Aristarchus the scientific imagination had reached its highest flight; but with Hipparchus it was beginning to settle back into regions of foggier atmosphere and narrower horizons. For what, after all, does it matter that Hipparchus should go on to measure the precise length of the year and the apparent size of the moon's disk; that he should make a chart of the heavens showing the place of 1080 stars; even that he should discover the precession of the equinox;--what, after all, is the significance of these details as against the all-essential fact that the greatest scientific authority of his century--the one truly heroic scientific figure of his epoch--should have lent all the forces of his commanding influence to the old, false theory of cosmology, when the true theory had been propounded and when he, perhaps, was the only man in the world who might have substantiated and vitalized that theory? It is easy to overestimate the influence of any single man, and, contrariwise, to underestimate the power of the Zeitgeist. But when we reflect that the doctrines of Hipparchus, as promulgated by Ptolemy, became, as it were, the last word of astronomical science for both the Eastern and Western worlds, and so continued after a thousand years, it is perhaps not too much to say that Hipparchus, "the lover of truth," missed one of the greatest opportunities for the promulgation of truth ever vouchsafed to a devotee of pure science.

But all this, of course, detracts nothing from the merits of Hipparchus as an observing astronomer. A few words more must be said as to his specific discoveries in this field. According to his measurement, the tropic year consists of 365 days, 5 hours, and 49 minutes, varying thus only 12 seconds from the true year, as the modern astronomer estimates it. Yet more remarkable, because of the greater difficulties involved, was Hipparchus's attempt to measure the actual distance of the moon. Aristarchus had made a similar attempt before him. Hipparchus based his computations on studies of the moon in eclipse, and he reached the conclusion that the distance of the moon is equal to 59 radii of the earth (in reality it is 60.27 radii). Here, then, was the measure of the base-line of that famous triangle with which Aristarchus had measured the distance of the sun. Hipparchus must have known of that measurement, since he quotes the work of Aristarchus in other fields. Had he now but repeated the experiment of Aristarchus, with his perfected instruments and his perhaps greater observational skill, he was in position to compute the actual distance of the sun in terms not merely of the moon's distance but of the earth's radius. And now there was the experiment of Eratosthenes to give the length of that radius in precise terms. In other words, Hipparchus might have measured the distance of the sun in stadia. But if he had made the attempt--and, indeed, it is more than likely that he did so--the elements of error in his measurements would still have kept him wide of the true figures.

The chief studies of Hipparchus were directed, as we have seen, towards the sun and the moon, but a phenomenon that occurred in the year 134 B.C. led him for a time to give more particular attention to the fixed stars. The phenomenon in question was the sudden outburst of a new star; a phenomenon which has been repeated now and again, but which is sufficiently rare and sufficiently mysterious to have excited the unusual attention of astronomers in all generations. Modern science offers an explanation of the phenomenon, as we shall see in due course. We do not know that Hipparchus attempted to explain it, but he was led to make a chart of the heavens, probably with the idea of guiding future observers in the observation of new stars. Here again Hipparchus was not altogether an innovator, since a chart showing the brightest stars had been made by Eratosthenes; but the new charts were much elaborated.

The studies of Hipparchus led him to observe the stars chiefly with reference to the meridian

rather than with reference to their rising, as had hitherto been the custom. In making these studies of the relative position of the stars, Hipparchus was led to compare his observations with those of the Babylonians, which, it was said, Alexander had caused to be transmitted to Greece. He made use also of the observations of Aristarchus and others of his Greek precursors. The result of his comparisons proved that the sphere of the fixed stars had apparently shifted its position in reference to the plane of the sun's orbit--that is to say, the plane of the ecliptic no longer seemed to cut the sphere of the fixed stars at precisely the point where the two coincided in former centuries. The plane of the ecliptic must therefore be conceived as slowly revolving in such a way as gradually to circumnavigate the heavens. This important phenomenon is described as the precession of the equinoxes.

It is much in question whether this phenomenon was not known to the ancient Egyptian astronomers; but in any event, Hipparchus is to be credited with demonstrating the fact and making it known to the Western world. A further service was rendered theoretical astronomy by Hipparchus through his invention of the planosphere, an instrument for the representation of the mechanism of the heavens. His computations of the properties of the spheres led him also to what was virtually a discovery of the method of trigonometry, giving him, therefore, a high position in the field of mathematics. All in all, then, Hipparchus is a most heroic figure. He may well be considered the greatest star-gazer of antiquity, though he cannot, without injustice to his great precursors, be allowed the title which is sometimes given him of "father of systematic astronomy."

CTESIBIUS AND HERO: MAGICIANS OF ALEXANDRIA

Just about the time when Hipparchus was working out at Rhodes his puzzles of celestial mechanics, there was a man in Alexandria who was exercising a strangely inventive genius over mechanical problems of another sort; a man who, following the example set by Archimedes a century before, was studying the problems of matter and putting his studies to practical application through the invention of weird devices. The man's name was Ctesibius. We know scarcely more of him than that he lived in Alexandria, probably in the first half of the second century B.C. His antecedents, the place and exact time of his birth and death, are quite unknown. Neither are we quite certain as to the precise range of his studies or the exact number of his discoveries. It appears that he had a pupil named Hero, whose personality, unfortunately, is scarcely less obscure than that of his master, but who wrote a book through which the record of the master's inventions was preserved to posterity. Hero, indeed, wrote several books, though only one of them has been preserved. The ones that are lost bear the following suggestive titles: On the Construction of Slings; On the Construction of Missiles; On the Automaton; On the Method of Lifting Heavy Bodies; On the Dioptric or Spying-tube. The work that remains is called Pneumatics, and so interesting a work it is as to make us doubly regret the loss of its companion volumes. Had these other books been preserved we should doubtless have a clearer insight than is now possible into some at least of the mechanical problems that exercised the minds of the ancient philosophers. The book that remains is chiefly concerned, as its name implies, with the study of gases, or, rather, with the study of a single gas, this being, of course, the air. But it tells us also of certain studies in the dynamics of water that are most interesting, and for the historian of science most important.

Unfortunately, the pupil of Ctesibius, whatever his ingenuity, was a man with a deficient sense of

the ethics of science. He tells us in his preface that the object of his book is to record some ingenious discoveries of others, together with additional discoveries of his own, but nowhere in the book itself does he give us the, slightest clew as to where the line is drawn between the old and the new. Once, in discussing the weight of water, he mentions the law of Archimedes regarding a floating body, but this is the only case in which a scientific principle is traced to its source or in which credit is given to any one for a discovery. This is the more to be regretted because Hero has discussed at some length the theories involved in the treatment of his subject. This reticence on the part of Hero, combined with the fact that such somewhat later writers as Pliny and Vitruvius do not mention Hero's name, while they frequently mention the name of his master, Ctesibius, has led modern critics to a somewhat sceptical attitude regarding the position of Hero as an actual discoverer.

The man who would coolly appropriate some discoveries of others under cloak of a mere prefatorial reference was perhaps an expounder rather than an innovator, and had, it is shrewdly suspected, not much of his own to offer. Meanwhile, it is tolerably certain that Ctesibius was the discoverer of the principle of the siphon, of the forcing-pump, and of a pneumatic organ. An examination of Hero's book will show that these are really the chief principles involved in most of the various interesting mechanisms which he describes. We are constrained, then, to believe that the inventive genius who was really responsible for the mechanisms we are about to describe was Ctesibius, the master. Yet we owe a debt of gratitude to Hero, the pupil, for having given wider vogue to these discoveries, and in particular for the discussion of the principles of hydrostatics and pneumatics contained in the introduction to his book. This discussion furnishes us almost our only knowledge as to the progress of Greek philosophers in the field of mechanics since the time of Archimedes.

The main purpose of Hero in his preliminary thesis has to do with the nature of matter, and recalls, therefore, the studies of Anaxagoras and Democritus. Hero, however, approaches his subject from a purely material or practical stand-point. He is an explicit champion of what we nowadays call the molecular theory of matter. "Every body," he tells us, "is composed of minute particles, between which are empty spaces less than these particles of the body. It is, therefore, erroneous to say that there is no vacuum except by the application of force, and that every space is full either of air or water or some other substance. But in proportion as any one of these particles recedes, some other follows it and fills the vacant space; therefore there is no continuous vacuum, except by the application of some force [like suction]--that is to say, an absolute vacuum is never found, except as it is produced artificially." Hero brings forward some thoroughly convincing proofs of the thesis he is maintaining. "If there were no void places between the particles of water," he says, "the rays of light could not penetrate the water; moreover, another liquid, such as wine, could not spread itself through the water, as it is observed to do, were the particles of water absolutely continuous." The latter illustration is one the validity of which appeals as forcibly to the physicists of to-day as it did to Hero. The same is true of the argument drawn from the compressibility of gases. Hero has evidently made a careful study of this subject. He knows that an inverted tube full of air may be immersed in water without becoming wet on the inside, proving that air is a physical substance; but he knows also that this same air may be caused to expand to a much greater bulk by the application of heat, or may, on the other hand, be condensed by pressure, in which case, as he is well aware, the air exerts force in the attempt to regain its normal bulk. But, he argues, surely we are not to believe that the particles of air expand to fill all the space when the bulk of air as a whole expands under

the influence of heat; nor can we conceive that the particles of normal air are in actual contact, else we should not be able to compress the air. Hence his conclusion, which, as we have seen, he makes general in its application to all matter, that there are spaces, or, as he calls them, vacua, between the particles that go to make up all substances, whether liquid, solid, or gaseous.

Here, clearly enough, was the idea of the "atomic" nature of matter accepted as a fundamental notion. The argumentative attitude assumed by Hero shows that the doctrine could not be expected to go unchallenged. But, on the other hand, there is nothing in his phrasing to suggest an intention to claim originality for any phase of the doctrine. We may infer that in the three hundred years that had elapsed since the time of Anaxagoras, that philosopher's idea of the molecular nature of matter had gained fairly wide currency. As to the expansive power of gas, which Hero describes at some length without giving us a clew to his authorities, we may assume that Ctesibius was an original worker, yet the general facts involved were doubtless much older than his day. Hero, for example, tells us of the cupping-glass used by physicians, which he says is made into a vacuum by burning up the air in it; but this apparatus had probably been long in use, and Hero mentions it not in order to describe the ordinary cupping-glass which is referred to, but a modification of it. He refers to the old form as if it were something familiar to all.

Again, we know that Empedocles studied the pressure of the air in the fifth century B.C., and discovered that it would support a column of water in a closed tube, so this phase of the subject is not new. But there is no hint anywhere before this work of Hero of a clear understanding that the expansive properties of the air when compressed, or when heated, may be made available as a motor power. Hero, however, has the clearest notions on the subject and puts them to the practical test of experiment. Thus he constructs numerous mechanisms in which the expansive power of air under pressure is made to do work, and others in which the same end is accomplished through the expansive power of heated air. For example, the doors of a temple are made to swing open automatically when a fire is lighted on a distant altar, closing again when the fire dies out--effects which must have filled the minds of the pious observers with bewilderment and wonder, serving a most useful purpose for the priests, who alone, we may assume, were in the secret. There were two methods by which this apparatus was worked. In one the heated air pressed on the water in a close retort connected with the altar, forcing water out of the retort into a bucket, which by its weight applied a force through pulleys and ropes that turned the standards on which the temple doors revolved. When the fire died down the air contracted, the water was siphoned back from the bucket, which, being thus lightened, let the doors close again through the action of an ordinary weight. The other method was a slight modification, in which the retort of water was dispensed with and a leather sack like a large football substitued. The ropes and pulleys were connected with this sack, which exerted a pull when the hot air expanded, and which collapsed and thus relaxed its strain when the air cooled. A glance at the illustrations taken from Hero's book will make the details clear.

Other mechanisms utilized a somewhat different combination of weights, pulleys, and siphons, operated by the expansive power of air, unheated but under pressure, such pressure being applied with a force- pump, or by the weight of water running into a closed receptacle. One such mechanism gives us a constant jet of water or perpetual fountain. Another curious application of the principle furnishes us with an elaborate toy, consisting of a group of birds which alternately whistle or are silent, while an owl seated on a neighboring perch turns towards the birds when their song begins and away from them when it ends. The "singing" of the birds, it must be

explained, is produced by the expulsion of air through tiny tubes passing up through their throats from a tank below. The owl is made to turn by a mechanism similar to that which manipulates the temple doors. The pressure is supplied merely by a stream of running water, and the periodical silence of the birds is due to the fact that this pressure is relieved through the automatic siphoning off of the water when it reaches a certain height. The action of the siphon, it may be added, is correctly explained by Hero as due to the greater weight of the water in the longer arm of the bent tube. As before mentioned, the siphon is repeatedly used in these mechanisms of Hero. The diagram will make clear the exact application of it in the present most ingenious mechanism. We may add that the principle of the whistle was a favorite one of Hero. By the aid of a similar mechanism he brought about the blowing of trumpets when the temple doors were opened, a phenomenon which must greatly have enhanced the mystification. It is possible that this principle was utilized also in connection with statues to produce seemingly supernatural effects. This may be the explanation of the tradition of the speaking statue in the temple of Ammon at Thebes.

{illustration caption = DEVICE FOR CAUSING THE DOORS OF THE TEMPLE TO OPEN WHEN THE FIRE ON THE ALTAR IS LIGHTED (Air heated in the altar F drives water from the closed receptacle H through the tube KL into the bucket M, which descends through gravity, thus opening the doors. When the altar cools, the air contracts, the water is sucked from the bucket, and the weight and pulley close the doors.)}

{illustration caption = THE STEAM-ENGINE OF HERO (The steam generated in the receptacle AB passes through the tube EF into the globe, and escapes through the bent tubes H and K, causing the globe to rotate on the axis LG.)}

The utilization of the properties of compressed air was not confined, however, exclusively to mere toys, or to produce miraculous effects. The same principle was applied to a practical fire-engine, worked by levers and force-pumps; an apparatus, in short, altogether similar to that still in use in rural districts. A slightly different application of the motive power of expanding air is furnished in a very curious toy called "the dancing figures." In this, air heated in a retort like a miniature altar is allowed to escape through the sides of two pairs of revolving arms precisely like those of the ordinary revolving fountain with which we are accustomed to water our lawns, the revolving arms being attached to a plane on which several pairs of statuettes representing dancers are placed, An even more interesting application of this principle of setting a wheel in motion is furnished in a mechanism which must be considered the earliest of steam-engines. Here, as the name implies, the gas supplying the motive power is actually steam. The apparatus made to revolve is a globe connected with the steam-retort by a tube which serves as one of its axes, the steam escaping from the globe through two bent tubes placed at either end of an equatorial diameter. It does not appear that Hero had any thought of making practical use of this steam- engine. It was merely a curious toy--nothing more. Yet had not the age that succeeded that of Hero been one in which inventive genius was dormant, some one must soon have hit upon the idea that this steam- engine might be improved and made to serve a useful purpose. As the case stands, however, there was no advance made upon the steam motor of Hero for almost two thousand years. And, indeed, when the practical application of steam was made, towards the close of the eighteenth century, it was made probably quite without reference to the experiment of Hero, though knowledge of his toy may perhaps have given a clew to Watt or his predecessors.

{illustration caption = THE SLOT-MACHINE OF HERO (The coin introduced at A falls on the lever R, and by its weight opens the valve S, permitting the liquid to escape through the invisible tube LM. As the lever tips, the coin slides off and the valve closes. The liquid in tank must of course be kept above F.)}

In recent times there has been a tendency to give to this steam-engine of Hero something more than full meed of appreciation. To be sure, it marked a most important principle in the conception that steam might be used as a motive power, but, except in the demonstration of this principle, the mechanism of Hero was much too primitive to be of any importance. But there is one mechanism described by Hero which was a most explicit anticipation of a device, which presumably soon went out of use, and which was not reinvented until towards the close of the nineteenth century. This was a device which has become familiar in recent times as the penny-in-the-slot machine. When towards the close of the nineteenth century some inventive craftsman hit upon the idea of an automatic machine to supply candy, a box of cigarettes, or a whiff of perfumery, he may or may not have borrowed his idea from the slot-machine of Hero; but in any event, instead of being an innovator he was really two thousand years behind the times, for the slot-machine of Hero is the precise prototype of these modern ones.

The particular function which the mechanism of Hero was destined to fulfil was the distribution of a jet of water, presumably used for sacramental purposes, which was given out automatically when a five- drachma coin was dropped into the slot at the top of the machine. The internal mechanism of the machine was simple enough, consisting merely of a lever operating a valve which was opened by the weight of the coin dropping on the little shelf at the end of the lever, and which closed again when the coin slid off the shelf. The illustration will show how simple this mechanism was. Yet to the worshippers, who probably had entered the temple through doors miraculously opened, and who now witnessed this seemingly intelligent response of a machine, the result must have seemed mystifying enough; and, indeed, for us also, when we consider how relatively crude was the mechanical knowledge of the time, this must seem nothing less than marvellous. As in imagination we walk up to the sacred tank, drop our drachma in the slot, and hold our hand for the spurt of holy-water, can we realize that this is the land of the Pharaohs, not England or America; that the kingdom of the Ptolemies is still at its height; that the republic of Rome is mistress of the world; that all Europe north of the Alps is inhabited solely by barbarians; that Cleopatra and Julius Caesar are yet unborn; that the Christian era has not yet begun? Truly, it seems as if there could be no new thing under the sun.

X. SCIENCE OF THE ROMAN PERIOD

We have seen that the third century B.C. was a time when Alexandrian science was at its height, but that the second century produced also in Hipparchus at least one investigator of the very first rank; though, to be sure, Hipparchus can be called an Alexandrian only by courtesy. In the ensuing generations the Greek capital at the mouth of the Nile continued to hold its place as the centre of scientific and philosophical thought. The kingdom of the Ptolemies still flourished with at least the outward appearances of its old-time glory, and a company of grammarians and commentators of no small merit could always be found in the service of the famous museum and

library; but the whole aspect of world-history was rapidly changing. Greece, after her brief day of political supremacy, was sinking rapidly into desuetude, and the hard-headed Roman in the West was making himself master everywhere. While Hipparchus of Rhodes was in his prime, Corinth, the last stronghold of the main-land of Greece, had fallen before the prowess of the Roman, and the kingdom of the Ptolemies, though still nominally free, had begun to come within the sphere of Roman influence.

Just what share these political changes had in changing the aspect of Greek thought is a question regarding which difference of opinion might easily prevail; but there can be no question that, for one reason or another, the Alexandrian school as a creative centre went into a rapid decline at about the time of the Roman rise to world-power. There are some distinguished names, but, as a general rule, the spirit of the times is reminiscent rather than creative; the workers tend to collate the researches of their predecessors rather than to make new and original researches for themselves. Eratosthenes, the inventive world-measurer, was succeeded by Strabo, the industrious collator of facts; Aristarchus and Hipparchus, the originators of new astronomical methods, were succeeded by Ptolemy, the perfecter of their methods and the systematizer of their knowledge. Meanwhile, in the West, Rome never became a true culture-centre. The great genius of the Roman was political; the Augustan Age produced a few great historians and poets, but not a single great philosopher or creative devotee of science. Cicero, Lucian, Seneca, Marcus Aurelius, give us at best a reflection of Greek philosophy. Pliny, the one world-famous name in the scientific annals of Rome, can lay claim to no higher credit than that of a marvellously industrious collector of facts--the compiler of an encyclopaedia which contains not one creative touch.

All in all, then, this epoch of Roman domination is one that need detain the historian of science but a brief moment. With the culmination of Greek effort in the so-called Hellenistic period we have seen ancient science at its climax. The Roman period is but a time of transition, marking, as it were, a plateau on the slope between those earlier heights and the deep, dark valleys of the Middle Ages. Yet we cannot quite disregard the efforts of such workers as those we have just named. Let us take a more specific glance at their accomplishments.

STRABO THE GEOGRAPHER

The earliest of these workers in point of time is Strabo. This most famous of ancient geographers was born in Amasia, Pontus, about 63 B.C., and lived to the year 24 A.D., living, therefore, in the age of Caesar and Augustus, during which the final transformation in the political position of the kingdom of Egypt was effected. The name of Strabo in a modified form has become popularized through a curious circumstance. The geographer, it appears, was afflicted with a peculiar squint of the eyes, hence the name strabismus, which the modern oculist applies to that particular infirmity.

Fortunately, the great geographer has not been forced to depend upon hearsay evidence for recognition. His comprehensive work on geography has been preserved in its entirety, being one of the few expansive classical writings of which this is true. The other writings of Strabo, however, including certain histories of which reports have come down to us, are entirely lost. The geography is in many ways a remarkable book. It is not, however, a work in which any important new principles are involved. Rather is it typical of its age in that it is an elaborate compilation and a critical review of the labors of Strabo's predecessors. Doubtless it contains a

vast deal of new information as to the details of geography--precise areas and distance, questions of geographical locations as to latitude and zones, and the like. But however important these details may have been from a contemporary stand-point, they, of course, can have nothing more than historical interest to posterity. The value of the work from our present stand-point is chiefly due to the criticisms which Strabo passes upon his forerunners, and to the incidental historical and scientific references with which his work abounds. Being written in this closing period of ancient progress, and summarizing, as it does, in full detail the geographical knowledge of the time, it serves as an important guide-mark for the student of the progress of scientific thought. We cannot do better than briefly to follow Strabo in his estimates and criticisms of the work of his predecessors, taking note thus of the point of view from which he himself looked out upon the world. We shall thus gain a clear idea as to the state of scientific geography towards the close of the classical epoch.

"If the scientific investigation of any subject be the proper avocation of the philosopher," says Strabo, "geography, the science of which we propose to treat, is certainly entitled to a high place; and this is evident from many considerations. They who first undertook to handle the matter were distinguished men. Homer, Anaximander the Milesian, and Hecaeus (his fellow-citizen according to Eratosthenes), Democritus, Eudoxus, Dicaearchus, and Ephorus, with many others, and after these, Eratosthenes, Polybius, and Posidonius, all of them philosophers. Nor is the great learning through which alone this subject can be approached possessed by any but a person acquainted with both human and divine things, and these attainments constitute what is called philosophy. In addition to its vast importance in regard to social life and the art of government, geography unfolds to us a celestial phenomena, acquaints us with the occupants of the land and ocean, and the vegetation, fruits, and peculiarities of the various quarters of the earth, a knowledge of which marks him who cultivates it as a man earnest in the great problem of life and happiness."

Strabo goes on to say that in common with other critics, including Hipparchus, he regards Homer as the first great geographer. He has much to say on the geographical knowledge of the bard, but this need not detain us. We are chiefly concerned with his comment upon his more recent predecessors, beginning with Eratosthenes. The constant reference to this worker shows the important position which he held. Strabo appears neither as detractor nor as partisan, but as one who earnestly desires the truth. Sometimes he seems captious in his criticisms regarding some detail, nor is he always correct in his emendations of the labors of others; but, on the whole, his work is marked by an evident attempt at fairness. In reading his book, however, one is forced to the conclusion that Strabo is an investigator of details, not an original thinker. He seems more concerned with precise measurements than with questionings as to the open problems of his science. Whatever he accepts, then, may be taken as virtually the stock doctrine of the period.

"As the size of the earth," he says, "has been demonstrated by other writers, we shall here take for granted and receive as accurate what they have advanced. We shall also assume that the earth is spheroidal, that its surface is likewise spheroidal and, above all, that bodies have a tendency towards its centre, which latter point is clear to the perception of the most average understanding. However, we may show summarily that the earth is spheroidal, from the consideration that all things, however distant, tend to its centre, and that every body is attracted towards its centre by gravity. This is more distinctly proved from observations of the sea and sky, for here the evidence of the senses and common observation is alone requisite. The convexity of the sea is a

further proof of this to those who have sailed, for they cannot perceive lights at a distance when placed at the same level as their eyes, and if raised on high they at once become perceptible to vision though at the same time farther removed. So when the eye is raised it sees what before was utterly imperceptible. Homer speaks of this when he says:

" 'Lifted up on the vast wave he quickly beheld afar.'

Sailors as they approach their destination behold the shore continually raising itself to their view, and objects which had at first seemed low begin to lift themselves. Our gnomons, also, are, among other things, evidence of the revolution of the heavenly bodies, and common-sense at once shows us that if the depth of the earth were infinite such a revolution could not take place."[1]

Elsewhere Strabo criticises Eratosthenes for having entered into a long discussion as to the form of the earth. This matter, Strabo thinks, "should have been disposed of in the compass of a few words." Obviously this doctrine of the globe's sphericity had, in the course of 600 years, become so firmly established among the Greek thinkers as to seem almost axiomatic. We shall see later on how the Western world made a curious recession from this seemingly secure position under stimulus of an Oriental misconception. As to the size of the globe, Strabo is disposed to accept without particular comment the measurements of Eratosthenes. He speaks, however, of "more recent measurements," referring in particular to that adopted by Posidonius, according to which the circumference is only about one hundred and eighty thousand stadia. Posidonius, we may note in passing, was a contemporary and friend of Cicero, and hence lived shortly before the time of Strabo. His measurement of the earth was based on observations of a star which barely rose above the southern horizon at Rhodes as compared with the height of the same star when observed at Alexandria. This measurement of Posidonius, together with the even more famous measurement of Eratosthenes, appears to have been practically the sole guide as to the size of the earth throughout the later periods of antiquity, and, indeed, until the later Middle Ages.

As becomes a writer who is primarily geographer and historian rather than astronomer, Strabo shows a much keener interest in the habitable portions of the globe than in the globe as a whole. He assures us that this habitable portion of the earth is a great island, "since wherever men have approached the termination of the land, the sea, which we designate ocean, has been met with, and reason assures us of the similarity of this place which our senses have not been tempted to survey." He points out that whereas sailors have not circumnavigated the globe, that they had not been prevented from doing so by any continent, and it seems to him altogether unlikely that the Atlantic Ocean is divided into two seas by narrow isthmuses so placed as to prevent circumnavigation. "How much more probable that it is confluent and uninterrupted. This theory," he adds, "goes better with the ebb and flow of the ocean. Moreover (and here his reasoning becomes more fanciful), the greater the amount of moisture surrounding the earth, the easier would the heavenly bodies be supplied with vapor from thence." Yet he is disposed to believe, following Plato, that the tradition "concerning the island of Atlantos might be received as something more than idle fiction, it having been related by Solon, on the authority of the Egyptian priests, that this island, almost as large as a continent, was formerly in existence although now it had disappeared."[2]

In a word, then, Strabo entertains no doubt whatever that it would be possible to sail around the globe from Spain to India. Indeed, so matter-of-fact an inference was this that the feat of

Columbus would have seemed less surprising in the first century of our era than it did when actually performed in the fifteenth century. The terrors of the great ocean held the mariner back, rather than any doubt as to where he would arrive at the end of the voyage.

Coupled with the idea that the habitable portion of the earth is an island, there was linked a tolerably definite notion as to the shape of this island. This shape Strabo likens to a military cloak. The comparison does not seem peculiarly apt when we are told presently that the length of the habitable earth is more than twice its breadth. This idea, Strabo assures us, accords with the most accurate observations "both ancient and modern." These observations seemed to show that it is not possible to live in the region close to the equator, and that, on the other hand, the cold temperature sharply limits the habitability of the globe towards the north. All the civilization of antiquity clustered about the Mediterranean, or extended off towards the east at about the same latitude. Hence geographers came to think of the habitable globe as having the somewhat lenticular shape which a crude map of these regions suggests. We have already had occasion to see that at an earlier day Anaxagoras was perhaps influenced in his conception of the shape of the earth by this idea, and the constant references of Strabo impress upon us the thought that this long, relatively narrow area of the earth's surface is the only one which can be conceived of as habitable.

Strabo had much to tell us concerning zones, which, following Posidonius, he believes to have been first described by Parmenides. We may note, however, that other traditions assert that both Thales and Pythagoras had divided the earth into zones. The number of zones accepted by Strabo is five, and he criticises Polybius for making the number six. The five zones accepted by Strabo are as follows: the uninhabitable torrid zone lying in the region of the equator; a zone on either side of this extending to the tropic; and then the temperate zones extending in either direction from the tropic to the arctic regions. There seems to have been a good deal of dispute among the scholars of the time as to the exact arrangement of these zones, but the general idea that the north-temperate zone is the part of the earth with which the geographer deals seemed clearly established. That the south-temperate zone would also present a habitable area is an idea that is sometimes suggested, though seldom or never distinctly expressed. It is probable that different opinions were held as to this, and no direct evidence being available, a cautiously scientific geographer like Strabo would naturally avoid the expression of an opinion regarding it. Indeed, his own words leave us somewhat in doubt as to the precise character of his notion regarding the zones. Perhaps we shall do best to quote them:

"Let the earth be supposed to consist of five zones. (1) The equatorial circle described around it. (2) Another parallel to this, and defining the frigid zone of the northern hemisphere. (3) A circle passing through the poles and cutting the two preceding circles at right- angles. The northern hemisphere contains two quarters of the earth, which are bounded by the equator and circle passing through the poles. Each of these quarters should be supposed to contain a four-sided district, its northern side being of one-half of the parallel next the pole, its southern by the half of the equator, and its remaining sides by two segments of the circle drawn through the poles, opposite to each other, and equal in length. In one of these (which of them is of no consequence) the earth which we inhabit is situated, surrounded by a sea and similar to an island. This, as we said before, is evident both to our senses and to our reason. But let any one doubt this, it makes no difference so far as geography is concerned whether you believe the portion of the earth which we inhabit to be an island or only admit what we know from experience --namely, that

whether you start from the east or the west you may sail all around it. Certain intermediate spaces may have been left (unexplored), but these are as likely to be occupied by sea as uninhabited land. The object of the geographer is to describe known countries. Those which are unknown he passes over equally with those beyond the limits of the inhabited earth. It will, therefore, be sufficient for describing the contour of the island we have been speaking of, if we join by a right line the outmost points which, up to this time, have been explored by voyagers along the coast on either side."[3]

We may pass over the specific criticisms of Strabo upon various explorations that seem to have been of great interest to his contemporaries, including an alleged trip of one Eudoxus out into the Atlantic, and the journeyings of Pytheas in the far north. It is Pytheas, we may add, who was cited by Hipparchus as having made the mistaken observation that the length of the shadow of the gnomon is the same at Marseilles and Byzantium, hence that these two places are on the same parallel. Modern commentators have defended Pytheas as regards this observation, claiming that it was Hipparchus and not Pytheas who made the second observation from which the faulty induction was drawn. The point is of no great significance, however, except as showing that a correct method of determining the problems of latitude had thus early been suggested. That faulty observations and faulty application of the correct principle should have been made is not surprising. Neither need we concern ourselves with the details as to the geographical distances, which Strabo found so worthy of criticism and controversy. But in leaving the great geographer we may emphasize his point of view and that of his contemporaries by quoting three fundamental principles which he reiterates as being among the "facts established by natural philosophers." He tells us that "(1) The earth and heavens are spheroidal. (2) The tendency of all bodies having weight is towards a centre. (3) Further, the earth being spheroidal and having the same centre as the heavens, is motionless, as well as the axis that passes through both it and the heavens. The heavens turn round both the earth and its axis, from east to west. The fixed stars turn round with it at the same rate as the whole. These fixed stars follow in their course parallel circles, the principal of which are the equator, two tropics, and the arctic circles; while the planets, the sun, and the moon describe certain circles comprehended within the zodiac."[4]

Here, then, is a curious mingling of truth and error. The Pythagorean doctrine that the earth is round had become a commonplace, but it would appear that the theory of Aristarchus, according to which the earth is in motion, has been almost absolutely forgotten. Strabo does not so much as refer to it; neither, as we shall see, is it treated with greater respect by the other writers of the period.

TWO FAMOUS EXPOSITORS--PLINY AND PTOLEMY

While Strabo was pursuing his geographical studies at Alexandria, a young man came to Rome who was destined to make his name more widely known in scientific annals than that of any other Latin writer of antiquity. This man was Plinius Secundus, who, to distinguish him from his nephew, a famous writer in another field, is usually spoken of as Pliny the Elder. There is a famous story to the effect that the great Roman historian Livy on one occasion addressed a casual associate in the amphitheatre at Rome, and on learning that the stranger hailed from the outlying Spanish province of the empire, remarked to him, "Yet you have doubtless heard of my writings even there." "Then," replied the stranger, "you must be either Livy or Pliny."

The anecdote illustrates the wide fame which the Roman naturalist achieved in his own day. And the records of the Middle Ages show that this popularity did not abate in succeeding times. Indeed, the Natural History of Pliny is one of the comparatively few bulky writings of antiquity that the efforts of copyists have preserved to us almost entire. It is, indeed, a remarkable work and eminently typical of its time; but its author was an industrious compiler, not a creative genius. As a monument of industry it has seldom been equalled, and in this regard it seems the more remarkable inasmuch as Pliny was a practical man of affairs who occupied most of his life as a soldier fighting the battles of the empire. He compiled his book in the leisure hours stolen from sleep, often writing by the light of the camp-fire. Yet he cites or quotes from about four thousand works, most of which are known to us only by his references. Doubtless Pliny added much through his own observations. We know how keen was his desire to investigate, since he lost his life through attempting to approach the crater of Vesuvius on the occasion of that memorable eruption which buried the cities of Herculaneum and Pompeii.

Doubtless the wandering life of the soldier had given Pliny abundant opportunity for personal observation in his favorite fields of botany and zoology. But the records of his own observations are so intermingled with knowledge drawn from books that it is difficult to distinguish the one from the other. Nor does this greatly matter, for whether as closet-student or field-naturalist, Pliny's trait of mind is essentially that of the compiler. He was no philosophical thinker, no generalizer, no path-maker in science. He lived at the close of a great progressive epoch of thought; in one of those static periods when numberless observers piled up an immense mass of details which might advantageously be sorted into a kind of encyclopaedia. Such an encyclopaedia is the so-called Natural History of Pliny. It is a vast jumble of more or less uncritical statements regarding almost every field of contemporary knowledge. The descriptions of animals and plants predominate, but the work as a whole would have been immensely improved had the compiler shown a more critical spirit. As it is, he seems rather disposed to quote any interesting citation that he comes across in his omnivorous readings, shielding himself behind an equivocal "it is said," or "so and so alleges." A single illustration will suffice to show what manner of thing is thought worthy of repetition.

"It is asserted," he says, "that if the fish called a sea-star is smeared with the fox's blood and then nailed to the upper lintel of the door, or to the door itself, with a copper nail, no noxious spell will be able to obtain admittance, or, at all events, be productive of any ill effects."

It is easily comprehensible that a work fortified with such practical details as this should have gained wide popularity. Doubtless the natural histories of our own day would find readier sale were they to pander to various superstitions not altogether different from that here suggested. The man, for example, who believes that to have a black cat cross his path is a lucky omen would naturally find himself attracted by a book which took account of this and similar important details of natural history. Perhaps, therefore, it was its inclusion of absurdities, quite as much as its legitimate value, that gave vogue to the celebrated work of Pliny. But be that as it may, the most famous scientist of Rome must be remembered as a popular writer rather than as an experimental worker. In the history of the promulgation of scientific knowledge his work is important; in the history of scientific principles it may virtually be disregarded.

PTOLEMY, THE LAST GREAT ASTRONOMER OF ANTIQUITY

Almost the same thing may be said of Ptolemy, an even more celebrated writer, who was born

not very long after the death of Pliny. The exact dates of Ptolemy's life are not known, but his recorded observations extend to the year 151 A.D. He was a working astronomer, and he made at least one original discovery of some significance--namely, the observation of a hitherto unrecorded irregularity of the moon's motion, which came to be spoken of as the moon's evection. This consists of periodical aberrations from the moon's regular motion in its orbit, which, as we now know, are due to the gravitation pull of the sun, but which remained unexplained until the time of Newton. Ptolemy also made original observations as to the motions of the planets. He is, therefore, entitled to a respectable place as an observing astronomer; but his chief fame rests on his writings.

His great works have to do with geography and astronomy. In the former field he makes an advance upon Strabo, citing the latitude of no fewer than five thousand places. In the field of astronomy, his great service was to have made known to the world the labors of Hipparchus. Ptolemy has been accused of taking the star-chart of his great predecessor without due credit, and indeed it seems difficult to clear him of this charge. Yet it is at least open to doubt whether be intended any impropriety, inasmuch as be all along is sedulous in his references to his predecessor. Indeed, his work might almost be called an exposition of the astronomical doctrines of Hipparchus. No one pretends that Ptolemy is to be compared with the Rhodesian observer as an original investigator, but as a popular expounder his superiority is evidenced in the fact that the writings of Ptolemy became practically the sole astronomical text-book of the Middle Ages both in the East and in the West, while the writings of Hipparchus were allowed to perish.

The most noted of all the writings of Ptolemy is the work which became famous under the Arabic name of Almagest. This word is curiously derived from the Greek title <gr h megisth suntazis>, "the greatest construction," a name given the book to distinguish it from a work on astrology in four books by the same author. For convenience of reference it came to be spoken of merely as <gr h megisth>, from which the Arabs form the title Tabair al Magisthi, under which title the book was published in the year 827. From this it derived the word Almagest, by which Ptolemy's work continued to be known among the Arabs, and subsequently among Europeans when the book again became known in the West. Ptolemy's book, as has been said, is virtually an elaboration of the doctrines of Hipparchus. It assumes that the earth is the fixed centre of the solar system, and that the stars and planets revolve about it in twenty-four hours, the earth being, of course, spherical. It was not to be expected that Ptolemy should have adopted the heliocentric idea of Aristarchus. Yet it is much to be regretted that he failed to do so, since the deference which was accorded his authority throughout the Middle Ages would doubtless have been extended in some measure at least to this theory as well, had he championed it. Contrariwise, his unqualified acceptance of the geocentric doctrine sufficed to place that doctrine beyond the range of challenge.

The Almagest treats of all manner of astronomical problems, but the feature of it which gained it widest celebrity was perhaps that which has to do with eccentrics and epicycles. This theory was, of course, but an elaboration of the ideas of Hipparchus; but, owing to the celebrity of the expositor, it has come to be spoken of as the theory of Ptolemy. We have sufficiently detailed the theory in speaking of Hipparchus. It should be explained, however, that, with both Hipparchus and Ptolemy, the theory of epicycles would appear to have been held rather as a working hypothesis than as a certainty, so far as the actuality of the minor spheres or epicycles is concerned. That is to say, these astronomers probably did not conceive either the epicycles or the

greater spheres as constituting actual solid substances. Subsequent generations, however, put this interpretation upon the theory, conceiving the various spheres as actual crystalline bodies. It is difficult to imagine just how the various epicycles were supposed to revolve without interfering with the major spheres, but perhaps this is no greater difficulty than is presented by the alleged properties of the ether, which physicists of to-day accept as at least a working hypothesis. We shall see later on how firmly the conception of concentric crystalline spheres was held to, and that no real challenge was ever given that theory until the discovery was made that comets have an orbit that must necessarily intersect the spheres of the various planets.

Ptolemy's system of geography in eight books, founded on that of Marinus of Tyre, was scarcely less celebrated throughout the Middle Ages than the Almagest. It contained little, however, that need concern us here, being rather an elaboration of the doctrines to which we have already sufficiently referred. None of Ptolemy's original manuscripts has come down to us, but there is an alleged fifth-century manuscript attributed to Agathadamon of Alexandria which has peculiar interest because it contains a series of twenty-seven elaborately colored maps that are supposed to be derived from maps drawn up by Ptolemy himself. In these maps the sea is colored green, the mountains red or dark yellow, and the land white. Ptolemy assumed that a degree at the equator was 500 stadia instead of 604 stadia in length. We are not informed as to the grounds on which this assumption was made, but it has been suggested that the error was at least partially instrumental in leading to one very curious result. "Taking the parallel of Rhodes," says Donaldson,[5] "he calculated the longitudes from the Fortunate Islands to Cattigara or the west coast of Borneo at 180 degrees, conceiving this to be one-half the circumference of the globe. The real distance is only 125 degrees or 127 degrees, so that his measurement is wrong by one third of the whole, one-sixth for the error in the measurement of a degree and one-sixth for the errors in measuring the distance geometrically. These errors, owing to the authority attributed to the geography of Ptolemy in the Middle Ages, produced a consequence of the greatest importance. They really led to the discovery of America. For the design of Columbus to sail from the west of Europe to the east of Asia was founded on the supposition that the distance was less by one third than it really was." This view is perhaps a trifle fanciful, since there is nothing to suggest that the courage of Columbus would have balked at the greater distance, and since the protests of the sailors, which nearly thwarted his efforts, were made long before the distance as estimated by Ptolemy had been covered; nevertheless it is interesting to recall that the great geographical doctrines, upon which Columbus must chiefly have based his arguments, had been before the world in an authoritative form practically unheeded for more than twelve hundred years, awaiting a champion with courage enough to put them to the test.

GALEN--THE LAST GREAT ALEXANDRIAN

There is one other field of scientific investigation to which we must give brief attention before leaving the antique world. This is the field of physiology and medicine. In considering it we shall have to do with the very last great scientist of the Alexandrian school. This was Claudius Galenus, commonly known as Galen, a man whose fame was destined to eclipse that of all other physicians of antiquity except Hippocrates, and whose doctrines were to have the same force in their field throughout the Middle Ages that the doctrines of Aristotle had for physical science. But before we take up Galen's specific labors, it will be well to inquire briefly as to the state of medical art and science in the Roman world at the time when the last great physician of antiquity came upon the scene.

The Romans, it would appear, had done little in the way of scientific discoveries in the field of medicine, but, nevertheless, with their practicality of mind, they had turned to better account many more of the scientific discoveries of the Greeks than did the discoverers themselves. The practising physicians in early Rome were mostly men of Greek origin, who came to the capital after the overthrow of the Greeks by the Romans. Many of them were slaves, as earning money by either bodily or mental labor was considered beneath the dignity of a Roman citizen. The wealthy Romans, who owned large estates and numerous slaves, were in the habit of purchasing some of these slave doctors, and thus saving medical fees by having them attend to the health of their families.

By the beginning of the Christian era medicine as a profession had sadly degenerated, and in place of a class of physicians who practised medicine along rational or legitimate lines, in the footsteps of the great Hippocrates, there appeared great numbers of "specialists," most of them charlatans, who pretended to possess supernatural insight in the methods of treating certain forms of disease. These physicians rightly earned the contempt of the better class of Romans, and were made the object of many attacks by the satirists of the time. Such specialists travelled about from place to place in much the same manner as the itinerant "Indian doctors" and "lightning tooth-extractors" do to-day. Eye-doctors seem to have been particularly numerous, and these were divided into two classes, eye-surgeons and eye-doctors proper. The eye-surgeon performed such operations as cauterizing for ingrowing eyelashes and operating upon growths about the eyes; while the eye-doctors depended entirely upon salves and lotions. These eye-salves were frequently stamped with the seal of the physician who compounded them, something like two hundred of these seals being still in existence. There were besides these quacks, however, reputable eye-doctors who must have possessed considerable skill in the treatment of certain ophthalmias. Among some Roman surgical instruments discovered at Rheims were found also some drugs employed by ophthalmic surgeons, and an analysis of these show that they contained, among other ingredients, some that are still employed in the treatment of certain affections of the eye.

One of the first steps taken in recognition of the services of physicians was by Julius Caesar, who granted citizenship to all physicians practising in Rome. This was about fifty years before the Christian era, and from that time on there was a gradual improvement in the attitude of the Romans towards the members of the medical profession. As the Romans degenerated from a race of sturdy warriors and became more and more depraved physically, the necessity for physicians made itself more evident. Court physicians, and physicians-in-ordinary, were created by the emperors, as were also city and district physicians. In the year 133 A.D. Hadrian granted immunity from taxes and military service to physicians in recognition of their public services.

The city and district physicians, known as the archiatri populaires, treated and cared for the poor without remuneration, having a position and salary fixed by law and paid them semi-annually. These were honorable positions, and the archiatri were obliged to give instruction in medicine, without pay, to the poor students. They were allowed to receive fees and donations from their patients, but not, however, until the danger from the malady was past. Special laws were enacted to protect them, and any person subjecting them to an insult was liable to a fine "not exceeding one thousand pounds."

An example of Roman practicality is shown in the method of treating hemorrhage, as described

by Aulus Cornelius Celsus (53 B.C. to 7 A.D.). Hippocrates and Hippocratic writers treated hemorrhage by application of cold, pressure, styptics, and sometimes by actual cauterizing; but they knew nothing of the simple method of stopping a hemorrhage by a ligature tied around the bleeding vessel. Celsus not only recommended tying the end of the injured vessel, but describes the method of applying two ligatures before the artery is divided by the surgeon--a common practice among surgeons at the present time. The cut is made between these two, and thus hemorrhage is avoided from either end of the divided vessel.

Another Roman surgeon, Heliodorus, not only describes the use of the ligature in stopping hemorrhage, but also the practice of torsion--twisting smaller vessels, which causes their lining membrane to contract in a manner that produces coagulation and stops hemorrhage. It is remarkable that so simple and practical a method as the use of the ligature in stopping hemorrhage could have gone out of use, once it had been discovered; but during the Middle Ages it was almost entirely lost sight of, and was not reintroduced until the time of Ambroise Pare, in the sixteenth century.

Even at a very early period the Romans recognized the advantage of surgical methods on the field of battle. Each soldier was supplied with bandages, and was probably instructed in applying them, something in the same manner as is done now in all modern armies. The Romans also made use of military hospitals and had established a rude but very practical field-ambulance service. "In every troop or bandon of two or four hundred men, eight or ten stout fellows were deputed to ride immediately behind the fighting-line to pick up and rescue the wounded, for which purpose their saddles had two stirrups on the left side, while they themselves were provided with water-flasks, and perhaps applied temporary bandages. They were encouraged by a reward of a piece of gold for each man they rescued. 'Noscomi' were male nurses attached to the military hospitals, but not inscribed 'on strength' of the legions, and were probably for the most part of the servile class."[6]

From the time of the early Alexandrians, Herophilus and Erasistratus, whose work we have already examined, there had been various anatomists of some importance in the Alexandrian school, though none quite equal to these earlier workers. The best-known names are those of Celsus (of whom we have already spoken), who continued the work of anatomical investigation, and Marinus, who lived during the reign of Nero, and Rufus of Ephesus. Probably all of these would have been better remembered by succeeding generations had their efforts not been eclipsed by those of Galen. This greatest of ancient anatomists was born at Pergamus of Greek parents. His father, Nicon, was an architect and a man of considerable ability. Until his fifteenth year the youthful Galen was instructed at home, chiefly by his father; but after that time he was placed under suitable teachers for instruction in the philosophical systems in vogue at that period. Shortly after this, however, the superstitious Nicon, following the interpretations of a dream, decided that his son should take up the study of medicine, and placed him under the instruction of several learned physicians.

Galen was a tireless worker, making long tours into Asia Minor and Palestine to improve himself in pharmacology, and studying anatomy for some time at Alexandria. He appears to have been full of the superstitions of the age, however, and early in his career made an extended tour into western Asia in search of the chimerical "jet-stone"--a stone possessing the peculiar qualities of "burning with a bituminous odor and supposed to possess great potency in curing such diseases

as epilepsy, hysteria, and gout."

By the time he had reached his twenty-eighth year he had perfected his education in medicine and returned to his home in Pergamus. Even at that time he had acquired considerable fame as a surgeon, and his fellow-citizens showed their confidence in his ability by choosing him as surgeon to the wounded gladiators shortly after his return to his native city. In these duties his knowledge of anatomy aided him greatly, and he is said to have healed certain kinds of wounds that had previously baffled the surgeons.

In the time of Galen dissections of the human body were forbidden by law, and he was obliged to confine himself to dissections of the lower animals. He had the advantage, however, of the anatomical works of Herophilus and Erasistratus, and he must have depended upon them in perfecting his comparison between the anatomy of men and the lower animals. It is possible that he did make human dissections surreptitiously, but of this we have no proof.

He was familiar with the complicated structure of the bones of the cranium. He described the vertebrae clearly, divided them into groups, and named them after the manner of anatomists of to-day. He was less accurate in his description of the muscles, although a large number of these were described by him. Like all anatomists before the time of Harvey, he had a very erroneous conception of the circulation, although he understood that the heart was an organ for the propulsion of blood, and he showed that the arteries of the living animals did not contain air alone, as was taught by many anatomists. He knew, also, that the heart was made up of layers of fibres that ran in certain fixed directions--that is, longitudinal, transverse, and oblique; but he did not recognize the heart as a muscular organ. In proof of this he pointed out that all muscles require rest, and as the heart did not rest it could not be composed of muscular tissue.

Many of his physiological experiments were conducted upon scientific principles. Thus he proved that certain muscles were under the control of definite sets of nerves by cutting these nerves in living animals, and observing that the muscles supplied by them were rendered useless. He pointed out also that nerves have no power in themselves, but merely conduct impulses to and from the brain and spinal-cord. He turned this peculiar knowledge to account in the case of a celebrated sophist, Pausanias, who had been under the treatment of various physicians for a numbness in the fourth and fifth fingers of his left hand. These physicians had been treating this condition by applications of poultices to the hand itself. Galen, being called in consultation, pointed out that the injury was probably not in the hand itself, but in the ulner nerve, which controls sensation in the fourth and fifth fingers. Surmising that the nerve must have been injured in some way, he made careful inquiries of the patient, who recalled that he had been thrown from his chariot some time before, striking and injuring his back. Acting upon this information, Galen applied stimulating remedies to the source of the nerve itself--that is, to the bundle of nerve-trunks known as the brachial plexus, in the shoulder. To the surprise and confusion of his fellow-physicians, this method of treatment proved effective and the patient recovered completely in a short time.

Although the functions of the organs in the chest were not well understood by Galen, he was well acquainted with their anatomy. He knew that the lungs were covered by thin membrane, and that the heart was surrounded by a sac of very similar tissue. He made constant comparisons also between these organs in different animals, as his dissections were performed upon beasts ranging in size from a mouse to an elephant. The minuteness of his observations is shown by the fact that

he had noted and described the ring of bone found in the hearts of certain animals, such as the horse, although not found in the human heart or in most animals.

His description of the abdominal organs was in general accurate. He had noted that the abdominal cavity was lined with a peculiar saclike membrane, the peritoneum, which also surrounded most of the organs contained in the cavity, and he made special note that this membrane also enveloped the liver in a peculiar manner. The exactness of the last observation seems the more wonderful when we reflect that even to-day the medical, student finds a correct understanding of the position of the folds of the peritoneum one of the most difficult subjects in anatomy.

As a practical physician he was held in the highest esteem by the Romans. The Emperor Marcus Aurelius called him to Rome and appointed him physician-inordinary to his son Commodus, and on special occasions Marcus Aurelius himself called in Galen as his medical adviser. On one occasion, the three army surgeons in attendance upon the emperor declared that he was about to be attacked by a fever. Galen relates how "on special command I felt his pulse, and finding it quite normal, considering his age and the time of day, I declared it was no fever but a digestive disorder, due to the food he had eaten, which must be converted into phlegm before being excreted. Then the emperor repeated three times, 'That's the very thing,' and asked what was to be done. I answered that I usually gave a glass of wine with pepper sprinkled on it, but for you kings we only use the safest remedies, and it will suffice to apply wool soaked in hot nard ointment locally. The emperor ordered the wool, wine, etc., to be brought, and I left the room. His feet were warmed by rubbing with hot hands, and after drinking the peppered wine, he said to Pitholaus (his son's tutor), 'We have only one doctor, and that an honest one,' and went on to describe me as the first of physicians and the only philosopher, for he had tried many before who were not only lovers of money, but also contentious, ambitious, envious, and malignant."[7]

It will be seen from this that Galen had a full appreciation of his own abilities as a physician, but inasmuch as succeeding generations for a thousand years concurred in the alleged statement made by Marcus Aurelius as to his ability, he is perhaps excusable for his open avowal of his belief in his powers. His faith in his accuracy in diagnosis and prognosis was shown when a colleague once said to him, "I have used the prognostics of Hippocrates as well as you. Why can I not prognosticate as well as you?" To this Galen replied, "By God's help I have never been deceived in my prognosis."[8] It is probable that this statement was made in the heat of argument, and it is hardly to be supposed that he meant it literally.

His systems of treatment were far in advance of his theories regarding the functions of organs, causes of disease, etc., and some of them are still first principles with physicians. Like Hippocrates, he laid great stress on correct diet, exercise, and reliance upon nature. "Nature is the overseer by whom health is supplied to the sick," he says. "Nature lends her aid on all sides, she decides and cures diseases. No one can be saved unless nature conquers the disease, and no one dies unless nature succumbs."

From the picture thus drawn of Galen as an anatomist and physician, one might infer that he should rank very high as a scientific exponent of medicine, even in comparison with modern physicians. There is, however, another side to the picture. His knowledge of anatomy was certainly very considerable, but many of his deductions and theories as to the functions of organs, the cause of diseases, and his methods of treating them, would be recognized as absurd

by a modern school-boy of average intelligence. His greatness must be judged in comparison with ancient, not with modern, scientists. He maintained, for example, that respiration and the pulse-beat were for one and the same purpose--that of the reception of air into the arteries of the body. To him the act of breathing was for the purpose of admitting air into the lungs, whence it found its way into the heart, and from there was distributed throughout the body by means of the arteries. The skin also played an important part in supplying the body with air, the pores absorbing the air and distributing it through the arteries. But, as we know that he was aware of the fact that the arteries also contained blood, he must have believed that these vessels contained a mixture of the two.

Modern anatomists know that the heart is divided into two approximately equal parts by an impermeable septum of tough fibres. Yet, Galen, who dissected the hearts of a vast number of the lower animals according to his own account, maintained that this septum was permeable, and that the air, entering one side of the heart from the lungs, passed through it into the opposite side and was then transferred to the arteries.

He was equally at fault, although perhaps more excusably so, in his explanation of the action of the nerves. He had rightly pointed out that nerves were merely connections between the brain and spinal-cord and distant muscles and organs, and had recognized that there were two kinds of nerves, but his explanation of the action of these nerves was that "nervous spirits" were carried to the cavities of the brain by blood-vessels, and from there transmitted through the body along the nerve-trunks.

In the human skull, overlying the nasal cavity, there are two thin plates of bone perforated with numerous small apertures. These apertures allow the passage of numerous nerve-filaments which extend from a group of cells in the brain to the delicate membranes in the nasal cavity. These perforations in the bone, therefore, are simply to allow the passage of the nerves. But Galen gave a very different explanation. He believed that impure "animal spirits" were carried to the cavities of the brain by the arteries in the neck and from there were sifted out through these perforated bones, and so expelled from the body.

He had observed that the skin played an important part in cooling the body, but he seems to have believed that the heart was equally active in overheating it. The skin, therefore, absorbed air for the purpose of "cooling the heart," and this cooling process was aided by the brain, whose secretions aided also in the cooling process. The heart itself was the seat of courage; the brain the seat of the rational soul; and the liver the seat of love.

The greatness of Galen's teachings lay in his knowledge of anatomy of the organs; his weakness was in his interpretations of their functions. Unfortunately, succeeding generations of physicians for something like a thousand years rejected the former but clung to the latter, so that the advances he had made were completely overshadowed by the mistakes of his teachings.

XI. A RETROSPECTIVE GLANCE AT CLASSICAL SCIENCE

It is a favorite tenet of the modern historian that history is a continuous stream. The contention

has fullest warrant. Sharp lines of demarcation are an evidence of man's analytical propensity rather than the work of nature. Nevertheless it would be absurd to deny that the stream of history presents an ever-varying current. There are times when it seems to rush rapidly on; times when it spreads out into a broad--seemingly static--current; times when its catastrophic changes remind us of nothing but a gigantic cataract. Rapids and whirlpools, broad estuaries and tumultuous cataracts are indeed part of the same stream, but they are parts that vary one from another in their salient features in such a way as to force the mind to classify them as things apart and give them individual names.

So it is with the stream of history; however strongly we insist on its continuity we are none the less forced to recognize its periodicity. It may not be desirable to fix on specific dates as turning-points to the extent that our predecessors were wont to do. We may not, for example, be disposed to admit that the Roman Empire came to any such cataclysmic finish as the year 476 A.D., when cited in connection with the overthrow of the last Roman Empire of the West, might seem to indicate. But, on the other hand, no student of the period can fail to realize that a great change came over the aspect of the historical stream towards the close of the Roman epoch.

The span from Thales to Galen has compassed about eight hundred years--let us say thirty generations. Throughout this period there is scarcely a generation that has not produced great scientific thinkers--men who have put their mark upon the progress of civilization; but we shall see, as we look forward for a corresponding period, that the ensuing thirty generations produced scarcely a single scientific thinker of the first rank. Eight hundred years of intellectual activity -- thirty generations of greatness; then eight hundred years of stasis--thirty generations of mediocrity; such seems to be the record as viewed in perspective. Doubtless it seemed far different to the contemporary observer; it is only in reasonable perspective that any scene can be viewed fairly. But for us, looking back without prejudice across the stage of years, it seems indisputable that a great epoch came to a close at about the time when the barbarian nations of Europe began to sweep down into Greece and Italy. We are forced to feel that we have reached the limits of progress of what historians are pleased to call the ancient world. For about eight hundred years Greek thought has been dominant, but in the ensuing period it is to play a quite subordinate part, except in so far as it influences the thought of an alien race. As we leave this classical epoch, then, we may well recapitulate in brief its triumphs. A few words will suffice to summarize a story the details of which have made up our recent chapters.

In the field of cosmology, Greek genius has demonstrated that the earth is spheroidal, that the moon is earthlike in structure and much smaller than our globe, and that the sun is vastly larger and many times more distant than the moon. The actual size of the earth and the angle of its axis with the ecliptic have been measured with approximate accuracy. It has been shown that the sun and moon present inequalities of motion which may be theoretically explained by supposing that the earth is not situated precisely at the centre of their orbits. A system of eccentrics and epicycles has been elaborated which serves to explain the apparent motions of the heavenly bodies in a manner that may be called scientific even though it is based, as we now know, upon a false hypothesis. The true hypothesis, which places the sun at the centre of the planetary system and postulates the orbital and axial motions of our earth in explanation of the motions of the heavenly bodies, has been put forward and ardently championed, but, unfortunately, is not accepted by the dominant thinkers at the close of our epoch. In this regard, therefore, a vast revolutionary work remains for the thinkers of a later period. Moreover, such observations as the

precession of the equinoxes and the moon's evection are as yet unexplained, and measurements of the earth's size, and of the sun's size and distance, are so crude and imperfect as to be in one case only an approximation, and in the other an absurdly inadequate suggestion. But with all these defects, the total achievement of the Greek astronomers is stupendous. To have clearly grasped the idea that the earth is round is in itself an achievement that marks off the classical from the Oriental period as by a great gulf.

In the physical sciences we have seen at least the beginnings of great things. Dynamics and hydrostatics may now, for the first time, claim a place among the sciences. Geometry has been perfected and trigonometry has made a sure beginning. The conception that there are four elementary substances, earth, water, air, and fire, may not appear a secure foundation for chemistry, yet it marks at least an attempt in the right direction. Similarly, the conception that all matter is made up of indivisible particles and that these have adjusted themselves and are perhaps held in place by a whirling motion, while it is scarcely more than a scientific dream, is, after all, a dream of marvellous insight.

In the field of biological science progress has not been so marked, yet the elaborate garnering of facts regarding anatomy, physiology, and the zoological sciences is at least a valuable preparation for the generalizations of a later time.

If with a map before us we glance at the portion of the globe which was known to the workers of the period now in question, bearing in mind at the same time what we have learned as to the seat of labors of the various great scientific thinkers from Thales to Galen, we cannot fail to be struck with a rather startling fact, intimations of which have been given from time to time--the fact, namely, that most of the great Greek thinkers did not live in Greece itself. As our eye falls upon Asia Minor and its outlying islands, we reflect that here were born such men as Thales, Anaximander, Anaximenes, Heraclitus, Pythagoras, Anaxagoras, Socrates, Aristarchus, Hipparchus, Eudoxus, Philolaus, and Galen. From the northern shores of the aegean came Lucippus, Democritus, and Aristotle. Italy, off to the west, is the home of Pythagoras and Xenophanes in their later years, and of Parmenides and Empedocles, Zeno, and Archimedes. Northern Africa can claim, by birth or by adoption, such names as Euclid, Apollonius of Perga, Herophilus, Erasistratus, Aristippus, Eratosthenes, Ctesibius, Hero, Strabo, and Ptolemy. This is but running over the list of great men whose discoveries have claimed our attention. Were we to extend the list to include a host of workers of the second rank, we should but emphasize the same fact.

All along we are speaking of Greeks, or, as they call themselves, Hellenes, and we mean by these words the people whose home was a small jagged peninsula jutting into the Mediterranean at the southeastern extremity of Europe. We think of this peninsula as the home of Greek culture, yet of all the great thinkers we have just named, not one was born on this peninsula, and perhaps not one in five ever set foot upon it. In point of fact, one Greek thinker of the very first rank, and one only, was born in Greece proper; that one, however, was Plato, perhaps the greatest of them all. With this one brilliant exception (and even he was born of parents who came from the provinces), all the great thinkers of Greece had their origin at the circumference rather than the centre of the empire. And if we reflect that this circumference of the Greek world was in the nature of the case the widely circling region in which the Greek came in contact with other nations, we shall see at once that there could be no more striking illustration in all history than

that furnished us here of the value of racial mingling as a stimulus to intellectual progress.

But there is one other feature of the matter that must not be overlooked. Racial mingling gives vitality, but to produce the best effect the mingling must be that of races all of which are at a relatively high plane of civilization. In Asia Minor the Greek mingled with the Semite, who had the heritage of centuries of culture; and in Italy with the Umbrians, Oscans, and Etruscans, who, little as we know of their antecedents, have left us monuments to testify to their high development. The chief reason why the racial mingling of a later day did not avail at once to give new life to Roman thought was that the races which swept down from the north were barbarians. It was no more possible that they should spring to the heights of classical culture than it would, for example, be possible in two or three generations to produce a racer from a stock of draught horses. Evolution does not proceed by such vaults as this would imply. Celt, Goth, Hun, and Slav must undergo progressive development for many generations before the population of northern Europe can catch step with the classical Greek and prepare to march forward. That, perhaps, is one reason why we come to a period of stasis or retrogression when the time of classical activity is over. But, at best, it is only one reason of several.

The influence of the barbarian nations will claim further attention as we proceed. But now, for the moment, we must turn our eyes in the other direction and give attention to certain phases of Greek and of Oriental thought which were destined to play a most important part in the development of the Western mind--a more important part, indeed, in the early mediaeval period than that played by those important inductions of science which have chiefly claimed our attention in recent chapters. The subject in question is the old familiar one of false inductions or pseudoscience. In dealing with the early development of thought and with Oriental science, we had occasion to emphasize the fact that such false inductions led everywhere to the prevalence of superstition. In dealing with Greek science, we have largely ignored this subject, confining attention chiefly to the progressive phases of thought; but it must not be inferred from this that Greek science, with all its secure inductions, was entirely free from superstition. On the contrary, the most casual acquaintance with Greek literature would suffice to show the incorrectness of such a supposition. True, the great thinkers of Greece were probably freer from this thraldom. of false inductions than any of their predecessors. Even at a very early day such men as Xenophanes, Empedocles, Anaxagoras, and Plato attained to a singularly rationalistic conception of the universe.

We saw that "the father of medicine," Hippocrates, banished demonology and conceived disease as due to natural causes. At a slightly later day the sophists challenged all knowledge, and Pyrrhonism became a synonym for scepticism in recognition of the leadership of a master doubter. The entire school of Alexandrians must have been relatively free from superstition, else they could not have reasoned with such effective logicality from their observations of nature. It is almost inconceivable that men like Euclid and Archimedes, and Aristarchus and Eratosthenes, and Hipparchus and Hero, could have been the victims of such illusions regarding occult forces of nature as were constantly postulated by Oriental science. Herophilus and Erasistratus and Galen would hardly have pursued their anatomical studies with equanimity had they believed that ghostly apparitions watched over living and dead alike, and exercised at will a malign influence.

Doubtless the Egyptian of the period considered the work, of the Ptolemaic anatomists an

unspeakable profanation, and, indeed, it was nothing less than revolutionary--so revolutionary that it could not be sustained in subsequent generations. We have seen that the great Galen, at Rome, five centuries after the time of Herophilus, was prohibited from dissecting the human subject. The fact speaks volumes for the attitude of the Roman mind towards science. Vast audiences made up of every stratum of society thronged the amphitheatre, and watched exultingly while man slew his fellow-man in single or in multiple combat. Shouts of frenzied joy burst from a hundred thousand throats when the death-stroke was given to a new victim. The bodies of the slain, by scores, even by hundreds, were dragged ruthlessly from the arena and hurled into a ditch as contemptuously as if pity were yet unborn and human life the merest bauble. Yet the same eyes that witnessed these scenes with ecstatic approval would have been averted in pious horror had an anatomist dared to approach one of the mutilated bodies with the scalpel of science. It was sport to see the blade of the gladiator enter the quivering, living flesh of his fellow-gladiator; it was joy to see the warm blood spurt forth from the writhing victim while he still lived; but it were sacrilegious to approach that body with the knife of the anatomist, once it had ceased to pulsate with life. Life itself was held utterly in contempt, but about the realm of death hovered the threatening ghosts of superstition. And such, be it understood, was the attitude of the Roman populace in the early and the most brilliant epoch of the empire, before the Western world came under the influence of that Oriental philosophy which was presently to encompass it.

In this regard the Alexandrian world was, as just intimated, far more advanced than the Roman, yet even there we must suppose that the leaders of thought were widely at variance with the popular conceptions. A few illustrations, drawn from Greek literature at various ages, will suggest the popular attitude. In the first instance, consider the poems of Homer and of Hesiod. For these writers, and doubtless for the vast majority of their readers, not merely of their own but of many subsequent generations, the world is peopled with a multitude of invisible apparitions, which, under title of gods, are held to dominate the affairs of man. It is sometimes difficult to discriminate as to where the Greek imagination drew the line between fact and allegory; nor need we attempt to analyse the early poetic narratives to this end. It will better serve our present purpose to cite three or four instances which illustrate the tangibility of beliefs based upon pseudo-scientific inductions.

Let us cite, for example, the account which Herodotus gives us of the actions of the Greeks at Plataea, when their army confronted the remnant of the army of Xerxes, in the year 479 B.C. Here we see each side hesitating to attack the other, merely because the oracle had declared that whichever side struck the first blow would lose the conflict. Even after the Persian soldiers, who seemingly were a jot less superstitious or a shade more impatient than their opponents, had begun the attack, we are told that the Greeks dared not respond at first, though they were falling before the javelins of the enemy, because, forsooth, the entrails of a fowl did not present an auspicious appearance. And these were Greeks of the same generation with Empedocles and Anaxagoras and aeschylus; of the same epoch with Pericles and Sophocles and Euripides and Phidias. Such was the scientific status of the average mind--nay, of the best minds--with here and there a rare exception, in the golden age of Grecian culture.

Were we to follow down the pages of Greek history, we should but repeat the same story over and over. We should, for example, see Alexander the Great balked at the banks of the Hyphasis, and forced to turn back because of inauspicious auguries based as before upon the dissection of a

fowl. Alexander himself, to be sure, would have scorned the augury; had he been the prey of such petty superstitions he would never have conquered Asia. We know how he compelled the oracle at Delphi to yield to his wishes; how he cut the Gordian knot; how he made his dominating personality felt at the temple of Ammon in Egypt. We know, in a word, that he yielded to superstitions only in so far as they served his purpose. Left to his own devices, he would not have consulted an oracle at the banks of the Hyphasis; or, consulting, would have forced from the oracle a favorable answer. But his subordinates were mutinous and he had no choice. Suffice it for our present purpose that the oracle was consulted, and that its answer turned the conqueror back.

One or two instances from Roman history may complete the picture. Passing over all those mythical narratives which virtually constitute the early history of Rome, as preserved to us by such historians as Livy and Dionysius, we find so logical an historian as Tacitus recording a miraculous achievement of Vespasian without adverse comment. "During the months when Vespasian was waiting at Alexandria for the periodical season of the summer winds, and a safe navigation, many miracles occurred by which the favor of Heaven and a sort of bias in the powers above towards Vespasian were manifested." Tacitus then describes in detail the cure of various maladies by the emperor, and relates that the emperor on visiting a temple was met there, in the spirit, by a prominent Egyptian who was proved to be at the same time some eighty miles distant from Alexandria.

It must be admitted that Tacitus, in relating that Vespasian caused the blind to see and the lame to walk, qualifies his narrative by asserting that "persons who are present attest the truth of the transaction when there is nothing to be gained by falsehood." Nor must we overlook the fact that a similar belief in the power of royalty has persisted almost to our own day. But no such savor of scepticism attaches to a narrative which Dion Cassius gives us of an incident in the life of Marcus Aurelius--an incident that has become famous as the episode of The Thundering Legion. Xiphilinus has preserved the account of Dion, adding certain picturesque interpretations of his own. The original narrative, as cited, asserts that during one of the northern campaigns of Marcus Aurelius, the emperor and his army were surrounded by the hostile Quadi, who had every advantage of position and who presently ceased hostilities in the hope that heat and thirst would deliver their adversaries into their hands without the trouble of further fighting. "Now," says Dion, "while the Romans, unable either to combat or to retreat, and reduced to the last extremity by wounds, fatigue, heat, and thirst, were standing helplessly at their posts, clouds suddenly gathered in great number and rain descended in floods--certainly not without divine intervention, since the Egyptian Maege Arnulphis, who was with Marcus Antoninus, is said to have invoked several genii by the aerial mercury by enchantment, and thus through them had brought down rain."

Here, it will be observed, a supernatural explanation is given of a natural phenomenon. But the narrator does not stop with this. If we are to accept the account of Xiphilinus, Dion brings forward some striking proofs of divine interference. Xiphilinus gives these proofs in the following remarkable paragraph:

"Dion adds that when the rain began to fall every soldier lifted his head towards heaven to receive the water in his mouth; but afterwards others hold out their shields or their helmets to catch the water for themselves and for their horses. Being set upon by the barbarians . . . while

occupied in drinking, they would have been seriously incommoded had not heavy hail and numerous thunderbolts thrown consternation into the ranks of the enemy. Fire and water were seen to mingle as they left the heavens. The fire, however, did not reach the Romans, but if it did by chance touch one of them it was immediately extinguished, while at the same time the rain, instead of comforting the barbarians, seemed merely to excite like oil the fire with which they were being consumed. Some barbarians inflicted wounds upon themselves as though their blood had power to extinguish flames, while many rushed over to the side of the Romans, hoping that there water might save them."

We cannot better complete these illustrations of pagan credulity than by adding the comment of Xiphilinus himself. That writer was a Christian, living some generations later than Dion. He never thought of questioning the facts, but he felt that Dion's interpretation of these facts must not go unchallenged. As he interprets the matter, it was no pagan magician that wrought the miracle. He even inclines to the belief that Dion himself was aware that Christian interference, and not that of an Egyptian, saved the day. "Dion knew," he declares, "that there existed a legion called The Thundering Legion, which name was given it for no other reason than for what came to pass in this war," and that this legion was composed of soldiers from Militene who were all professed Christians. "During the battle," continues Xiphilinus, "the chief of the Pretonians , had set at Marcus Antoninus, who was in great perplexity at the turn events were taking, representing to him that there was nothing the people called Christians could not obtain by their prayers, and that among his forces was a troop composed wholly of followers of that religion. Rejoiced at this news, Marcus Antoninus demanded of these soldiers that they should pray to their god, who granted their petition on the instant, sent lightning among the enemy and consoled the Romans with rain. Struck by this wonderful success, the emperor honored the Christians in an edict and named their legion The Thundering. It is even asserted that a letter existed by Marcus Antoninus on this subject. The pagans well knew that the company was called The Thunderers, having attested the fact themselves, but they revealed nothing of the occasion on which the leader received the name."[1]

Peculiar interest attaches to this narrative as illustrating both credulousness as to matters of fact and pseudo-scientific explanation of alleged facts. The modern interpreter may suppose that a violent thunderstorm came up during the course of a battle between the Romans and the so-called barbarians, and that owing to the local character of the storm, or a chance discharge of lightning, the barbarians suffered more than their opponents. We may well question whether the philosophical emperor himself put any other interpretation than this upon the incident. But, on the other hand, we need not doubt that the major part of his soldiers would very readily accept such an explanation as that given by Dion Cassius, just as most readers of a few centuries later would accept the explanation of Xiphilinus. It is well to bear this thought in mind in considering the static period of science upon which we are entering. We shall perhaps best understand this period, and its seeming retrogressions, if we suppose that the average man of the Middle Ages was no more credulous, no more superstitious, than the average Roman of an earlier period or than the average Greek; though the precise complexion of his credulity had changed under the influence of Oriental ideas, as we have just seen illustrated by the narrative of Xiphilinus.

APPENDIX

REFERENCE LIST, NOTES, AND BIBLIOGRAPHIES

CHAPTER I. PREHISTORIC SCIENCE

Length of the Prehistoric Period.--It is of course quite impossible to reduce the prehistoric period to any definite number of years. There are, however, numerous bits of evidence that enable an anthropologist to make rough estimates as to the relative lengths of the different periods into which prehistoric time is divided. Gabriel de Mortillet, one of the most industrious students of prehistoric archaeology, ventured to give a tentative estimate as to the numbers of years involved in each period. He of course claimed for this nothing more than the value of a scientific guess. It is, however, a guess based on a very careful study of all data at present available. Mortillet divides the prehistoric period, as a whole, into four epochs. The first of these is the preglacial, which he estimates as comprising seventy-eight thousand years; the second is the glacial, covering one hundred thousand years; then follows what he terms the Solutreen, which numbers eleven thousand years; and, finally, the Magdalenien, comprising thirty-three thousand years. This gives, for the prehistoric period proper, a term of about two hundred and twenty-two thousand years. Add to this perhaps twelve thousand years ushering in the civilization of Egypt, and the six thousand years of stable, sure chronology of the historical period, and we have something like two hundred and thirty thousand or two hundred and forty thousand years as the age of man.

"These figures," says Mortillet, "are certainly not exaggerated. It is even probable that they are below the truth. Constantly new discoveries are being made that tend to remove farther back the date of man's appearance." We see, then, according to this estimate, that about a quarter of a million years have elapsed since man evolved to a state that could properly be called human. This guess is as good as another, and it may advantageously be kept in mind, as it will enable us all along to understand better than we might otherwise be able to do the tremendous force of certain prejudices and preconceptions which recent man inherited from his prehistoric ancestor. Ideas which had passed current as unquestioned truths for one hundred thousand years or so are not easily cast aside.

In going back, in imagination, to the beginning of the prehistoric period, we must of course reflect, in accordance with modern ideas on the subject, that there was no year, no millennium even, when it could be said expressly: "This being was hitherto a primate, he is now a man." The transition period must have been enormously long, and the changes from generation to generation, even from century to century, must have been very slight. In speaking of the extent of the age of man this must be borne in mind: it must be recalled that, even if the period were not vague for other reasons, the vagueness of its beginning must make it indeterminate.

Bibliographical Notes.--A great mass of literature has been produced in recent years dealing with various phases of the history of prehistoric man. No single work known to the writer deals comprehensively with the scientific attainments of early man; indeed, the subject is usually ignored, except where practical phases of the mechanical arts are in question. But of course any attempt to consider the condition of primitive man talies into account, by inference at least, his

knowledge and attainments. Therefore, most works on anthropology, ethnology, and primitive culture may be expected to throw some light on our present subject. Works dealing with the social and mental conditions of existing savages are also of importance, since it is now an accepted belief that the ancestors of civilized races evolved along similar lines and passed through corresponding stages of nascent culture. Herbert Spencer's Descriptive Sociology presents an unequalled mass of facts regarding existing primitive races, but, unfortunately, its inartistic method of arrangement makes it repellent to the general reader. E. B. Tyler's Primitive Culture and Anthropology; Lord Avebury's Prehistoric Times, The Origin of Civilization, and The Primitive Condition of Man; W. Boyd Dawkin's Cave-Hunting and Early Man in Britain; and Edward Clodd's Childhood of the World and Story of Primitive Man are deservedly popular. Paul Topinard's Elements d'Anthropologie Generale is one of the best-known and most comprehensive French works on the technical phases of anthropology; but Mortillet's Le Prehistorique has a more popular interest, owing to its chapters on primitive industries, though this work also contains much that is rather technical. Among periodicals, the Revue de l'Ecole d'Anthropologie de Paris, published by the professors, treats of all phases of anthropology, and the American Anthropologist, edited by F. W. Hodge for the American Anthropological Association, and intended as "a medium of communication between students of all branches of anthropology," contains much that is of interest from the present stand-point. The last-named journal devotes a good deal of space to Indian languages.

CHAPTER II. EGYPTIAN SCIENCE

1 (p. 34). Sir J. Norman Lockyer, The Dawn of Astronomy; a study of the temple worship and mythology of the ancient Egyptians, London, 1894.

2 (p. 43). G. Maspero, Histoire Ancie-nne des Peuples de l'Orient Classique, Paris, 1895. Translated as (1) The Dawn of Civilization, (2) The Struggle of the Nations, (3) The Passing of the Empires, 3 vols., London and New York, 1894-1900. Professor Maspero is one of the most famous of living Orientalists. His most important special studies have to do with Egyptology, but his writings cover the entire field of Oriental antiquity. He is a notable stylist, and his works are at once readable and authoritative.

3 (p. 44). Adolf Erman, Life in Ancient Egypt, London, 1894, p. 352. (Translated from the original German work entitled Aegypten und aegyptisches Leben in Alterthum, Tilbigen, 1887.) An altogether admirable work, full of interest for the general reader, though based on the most erudite studies.

4 (p. 47). Erman, op. cit., pp. 356, 357.

5 (p. 48). Erman, op. cit., p. 357. The work on Egyptian medicine here referred to is Georg Ebers' edition of an Egyptian document discovered by the explorer whose name it bears. It remains the most important source of our knowledge of Egyptian medicine. As mentioned in the text, this document dates from the eighteenth dynasty--that is to say, from about the fifteenth or sixteenth century, B.C., a relatively late period of Egyptian history.

6 (p. 49). Erman, op. cit., p. 357.

7 (p. 50). The History of Herodotus, pp. 85-90. There are numerous translations of the famous

work of the "father of history," one of the most recent and authoritative being that of G. C. Macaulay, M.A., in two volumes, Macmillan & Co., London and New York, 1890.

8 (p. 50). The Historical Library of Diodorus the Sicilian, London, 1700. This most famous of ancient world histories is difficult to obtain in an English version. The most recently published translation known to the writer is that of G. Booth, London, 1814.

9 (p. 51). Erman, op. cit., p. 357.

10 (p. 52). The Papyrus Rhind is a sort of mathematical hand-book of the ancient Egyptians; it was made in the time of the Hyksos Kings (about 2000 B.C.), but is a copy of an older book. It is now preserved in the British Museum.

The most accessible recent sources of information as to the social conditions of the ancient Egyptians are the works of Maspero and Erman, above mentioned; and the various publications of W. M. Flinders Petrie, The Pyramids and Temples of Gizeh, London, 1883; Tanis I., London, 1885; Tanis H., Nebesheh, and Defe-nnel, London, 1887; Ten Years' Diggings, London, 1892; Syria and Egypt from the Tel-el-Amar-na Letters, London, 1898, etc. The various works of Professor Petrie, recording his explorations from year to year, give the fullest available insight into Egyptian archaeology.

CHAPTER III. SCIENCE OF BABYLONIA AND ASSYRIA

1 (p. 57). The Medes. Some difference of opinion exists among historians as to the exact ethnic relations of the conquerors; the precise date of the fall of Nineveh is also in doubt.

2 (p. 57). Darius. The familiar Hebrew narrative ascribes the first Persian conquest of Babylon to Darius, but inscriptions of Cyrus and of Nabonidus, the Babylonian king, make it certain that Cyrus was the real conqueror. These inscriptions are preserved on cylinders of baked clay, of the type made familiar by the excavation of the past fifty years, and they are invaluable historical documents.

3 (p. 58). Berosus. The fragments of Berosus have been translated by L. P. Cory, and included in his Ancient Fragments of Phenician, Chaldean, Egyptian, and Other Writers, London, 1826, second edition, 1832.

4 (p. 58). Chaldean learning. Recent writers reserve the name Chaldean for the later period of Babylonian history-- the time when the Greeks came in contact with the Mesopotamians--in contradistinction to the earlier periods which are revealed to us by the archaeological records.

5 (p. 59) King Sargon of Agade. The date given for this early king must not be accepted as absolute; but it is probably approximately correct.

6 (p. 59). Nippur. See the account of the early expeditions as recorded by the director, Dr. John P. Peters, Nippur, or explorations and adventures, etc., New York and London, 1897.

7 (p. 62). Fritz Hommel, Geschichte Babyloniens und Assyriens, Berlin, 1885.

8 (p. 63). R. Campbell Thompson, Reports of the Magicians and Astrologers of Nineveh and Babylon, London, 1900, p. xix.

9 (p. 64). George Smith, The Assyrian Canon, p. 21.

10 (p. 64). Thompson, op. cit., p. xix.

11 (p. 65). Thompson, op. cit., p. 2.

12 (p. 67). Thompson, op. cit., p. xvi.

13 (p. 68). Sextus Empiricus, author of Adversus Mathematicos, lived about 200 A.D.

14 (p. 68). R. Campbell Thompson, op. cit., p. xxiv.

15 (p. 72). Records of the Past (editor, Samuel Birch), Vol. III., p. 139.

16 (p. 72). Ibid., Vol. V., p. 16.

17 (p. 72). Quoted in Records of the Past, Vol. III., p. 143, from the Translations of the Society of Biblical Archeology, vol. II., p. 58.

18 (p. 73). Records of the Past, vol. L, p. 131.

19 (p. 73). Ibid., vol. V., p. 171.

20 (p. 74). Ibid., vol. V., p. 169.

21 (p. 74). Joachim Menant, La Bibliotheque du Palais de Ninive, Paris, 188o.

22 (p. 76). Code of Khamurabi. This famous inscription is on a block of black diorite nearly eight feet in height. It was discovered at Susa by the French expedition under M. de Morgan, in December, 1902. We quote the translation given in The Historians' History of the World, edited by Henry Smith Williams, London and New York, 1904, Vol. I, p. 510.

23 (p. 77). The Historical Library of Diodorus Siculus, p. 519.

24 (p. 82). George S. Goodspeed, Ph.D., History of the Babylonians and Assyrians, New York, 1902.

25 (p. 82). George Rawlinson, Great Oriental Monarchies, (second edition, London, 1871), Vol. III., pp. 75 ff.

Of the books mentioned above, that of Hommel is particularly full in reference to culture development; Goodspeed's small volume gives an excellent condensed account; the original documents as translated in the various volumes of Records of the Past are full of interest; and Menant's little book is altogether admirable. The work of excavation is still going on in old Babylonia, and newly discovered texts add from time to time to our knowledge, but A. H. Layard's Nineveh and its Remains (London, 1849) still has importance as a record of the most important early discoveries. The general histories of Antiquity of Duncker, Lenormant, Maspero, and Meyer give full treatment of Babylonian and Assyrian development. Special histories of Babylonia and Assyria, in addition to these named above, are Tiele's Babylonisch-Assyrische Geschichte (Zwei Tiele, Gotha, 1886-1888); Winckler's Geschichte Babyloniens und Assyriens (Berlin, 1885-1888), and Rogers' History of Babylonia and Assyria, New York and London,

1900, the last of which, however, deals almost exclusively with political history. Certain phases of science, particularly with reference to chronology and cosmology, are treated by Edward Meyer (Geschichte des Alterthum, Vol. I., Stuttgart, 1884), and by P. Jensen (Die Kosmologie der Babylonier, Strassburg, 1890), but no comprehensive specific treatment of the subject in its entirety has yet been attempted.

CHAPTER IV. THE DEVELOPMENT OF THE ALPHABET

1 (p. 87). Vicomte E. de Rouge, Memoire sur l'Origine Egyptienne de l'Alphabet Phinicien, Paris, 1874.

2 (p. 88). See the various publications of Mr. Arthur Evans.

3 (p. 80). Aztec and Maya writing. These pictographs are still in the main undecipherable, and opinions differ as to the exact stage of development which they represent.

4 (p. 90). E. A. Wallace Budge's First Steps in Egyptian, London, 1895, is an excellent elementary work on the Egyptian writing. Professor Erman's Egyptian Grammar, London, 1894, is the work of perhaps the foremost living Egyptologist.

5 (P. 93). Extant examples of Babylonian and Assyrian writing give opportunity to compare earlier and later systems, so the fact of evolution from the pictorial to the phonetic system rests on something more than mere theory.

6 (p. 96). Friedrich Delitzsch, Assyrischc Lesestucke mit grammatischen Tabellen und vollstdndigem Glossar einfiihrung in die assyrische und babylonische Keilschrift-litteratur bis hinauf zu Hammurabi, Leipzig, 1900.

7 (p. 97). It does not appear that the Babylonians thcmselves ever gave up the old system of writing, so long as they retained political autonomy.

8 (p. 101). See Isaac Taylor's History of the Alphabet; an Account of the origin and Development of Letters, new edition, 2 vols., London, 1899.

For facsimiles of the various scripts, see Henry Smith Williams' History of the Art Of Writing, 4 vols, New York and London, 1902-1903.

CHAPTER V. THE BEGINNINGS OF GREEK SCIENCE

1 (p. III). Anaximander, as recorded by Plutarch, vol. VIII-. See Arthur Fairbanks'First Philosophers of Greece: an Edition and Translation of the Remaining Fragments of the Pre-Socratic Philosophers, together with a Translation of the more Important Accounts of their Opinions Contained in the Early Epitomcs of their Works, London, 1898. This highly scholarly and extremely useful book contains the Greek text as well as translations.

CHAPTER VI. THE EARLY GREEK PHILOSOPHERS IN ITALY

1 (p. 117). George Henry Lewes, A Biographical History of Philosophy from its Origin in Greece down to the Present Day, enlarged edition, New York, 1888, p. 17.

2 (p. 121). Diogenes Laertius, The Lives and Opinions of Eminent Philosophers, C. D. Yonge's translation, London, 1853, VIII., p. 153.

3 (p. 121). Alexander, Successions of Philosophers.

4 (p. 122). "All over its centre." Presumably this is intended to refer to the entire equatorial region.

5 (p. 125). Laertius, op. cit., pp. 348-351.

6 (p. 128). Arthur Fairbanks, The First Philosophers of Greece London, 1898, pp. 67-717.

7 (p. 129). Ibid., p. 838.

8 (p. 130). Ibid., p. 109.

9 (p. 130). Heinrich Ritter, The History of Ancient Philosophy, translated from the German by A. J. W. Morrison, 4 vols., London, 1838, vol, I., p. 463.

10 (p. 131). Ibid., p. 465.

11 (p. 132). George Henry Lewes, op. cit., p. 81.

12 (p. 135). Fairbanks, op. cit., p. 201.

13 (p. 136). Ibid., P. 234.

14 (p. 137). Ibid., p. 189.

15 (p. 137). Ibid., P. 220.

16 (p. 138). Ibid., p. 189.

17 (p. 138). Ibid., p. 191.

CHAPTER VII. GREEK SCIENCE IN THE EARLY ATTIC PERIOD

1 (p. 150). Theodor Gomperz, Greek Thinkers: a History of Ancient Philosophy (translated from the German by Laurie Magnes), New York, 190 1, pp. 220, 221.

2 (p. 153). Aristotle's Treatise on Respiration, ch. ii.

3 (p. 159). Fairbanks' translation of the fragments of Anaxagoras, in The First Philosophers of Greece, pp. 239-243.

CHAPTER VIII. POST-SOCRATIC SCIENCE AT ATHENS

1 (p. 180). Alfred William Bern, The Philosophy of Greece Considered in Relation to the Character and History of its People, London, 1898, p. 186.

2 (p. 183). Aristotle, quoted in William Whewell's History of the Inductive Sciences (second edition, London, 1847), Vol. II., p. 161.

CHAPTER IX. GREEK SCIENCE OF THE ALEXANDRIAN OR HELLENISTIC PERIOD

1 (p. 195). Tertullian's Apologeticus.

2 (p. 205). We quote the quaint old translation of North, printed in 1657.

CHAPTER X. SCIENCE OF THE ROMAN PERIOD

1 (p. 258). The Geography of Strabo, translated by H. C. Hamilton and W. Falconer, 3 vols., London, 1857, Vol. I, pp. 19, 20.

2 (p. 260). Ibid., p. 154.

3 (p. 263). Ibid., pp. 169, 170.

4 (p. 264) Ibid., pp. 166, 167.

5 (p. 271). K. 0. Miller and John W. Donaldson, The History of the Literature of Greece, 3 vols., London, Vol. III., p. 268.

6 (p. 276). E. T. Withington, Medical History fron., the Earliest Times, London, 1894, p. 118.

7 (p. 281). Ibid.

8 (p. 281). Johann Hermann Bass, History of Medicine, New York, 1889.

CHAPTER XI. A RETROSPECTIVE GLANCE AT CLASSICAL SCIENCE

(p. 298). Dion Cassius, as preserved by Xiphilinus. Our extract is quoted from the translation given in The Historians' History of the World (edited by Henry Smith Williams), 25 vols., London and New York, 1904, Vol. VI., p. 297 ff.

[For further bibliographical notes, the reader is referred to the Appendix of volume V.]

A HISTORY OF SCIENCE

BY

Henry Smith Williams, M.D., LL.D.

Assisted by Edward H. Williams, M.D.

IN FIVE VOLUMES

VOLUME II. THE BEGINNINGS OF MODERN SCIENCE

CONTENTS

A HISTORY OF SCIENCE- BOOK II THE BEGINNINGS OF MODERN SCIENCE

The studies of the present book cover the progress of science from the close of the Roman period in the fifth century A.D. to about the middle of the eighteenth century. In tracing the course of events through so long a period, a difficulty becomes prominent which everywhere besets the historian in less degree--a difficulty due to the conflict between the strictly chronological and the topical method of treatment. We must hold as closely as possible to the actual sequence of events, since, as already pointed out, one discovery leads on to another. But, on the other hand, progressive steps are taken contemporaneously in the various fields of science, and if we were to attempt to introduce these in strict chronological order we should lose all sense of topical continuity.

Our method has been to adopt a compromise, following the course of a single science in each great epoch to a convenient stopping-point, and then turning back to bring forward the story of another science. Thus, for example, we tell the story of Copernicus and Galileo, bringing the record of cosmical and mechanical progress down to about the middle of the seventeenth century, before turning back to take up the physiological progress of the fifteenth and sixteenth centuries. Once the latter stream is entered, however, we follow it without interruption to the time of Harvey and his contemporaries in the middle of the seventeenth century, where we leave it to return to the field of mechanics as exploited by the successors of Galileo, who were also the predecessors and contemporaries of Newton.

In general, it will aid the reader to recall that, so far as possible, we hold always to the same sequences of topical treatment of contemporary events; as a rule we treat first the cosmical, then the physical, then the biological sciences. The same order of treatment will be held to in succeeding volumes.

Several of the very greatest of scientific generalizations are developed in the period covered by the present book: for example, the Copernican theory of the solar system, the true doctrine of planetary motions, the laws of motion, the theory of the circulation of the blood, and the Newtonian theory of gravitation. The labors of the investigators of the early decades of the eighteenth century, terminating with Franklin's discovery of the nature of lightning and with the Linnaean classification of plants and animals, bring us to the close of our second great epoch; or, to put it otherwise, to the threshold of the modern period,

I. SCIENCE IN THE DARK AGE

An obvious distinction between the classical and mediaeval epochs may be found in the fact that

the former produced, whereas the latter failed to produce, a few great thinkers in each generation who were imbued with that scepticism which is the foundation of the investigating spirit; who thought for themselves and supplied more or less rational explanations of observed phenomena. Could we eliminate the work of some score or so of classical observers and thinkers, the classical epoch would seem as much a dark age as does the epoch that succeeded it.

But immediately we are met with the question: Why do no great original investigators appear during all these later centuries? We have already offered a part explanation in the fact that the borders of civilization, where racial mingling naturally took place, were peopled with semi-barbarians. But we must not forget that in the centres of civilization all along there were many men of powerful intellect. Indeed, it would violate the principle of historical continuity to suppose that there was any sudden change in the level of mentality of the Roman world at the close of the classical period. We must assume, then, that the direction in which the great minds turned was for some reason changed. Newton is said to have alleged that he made his discoveries by "intending" his mind in a certain direction continuously. It is probable that the same explanation may be given of almost every great scientific discovery. Anaxagoras could not have thought out the theory of the moon's phases; Aristarchus could not have found out the true mechanism of the solar system; Eratosthenes could not have developed his plan for measuring the earth, had not each of these investigators "intended" his mind persistently towards the problems in question.

Nor can we doubt that men lived in every generation of the dark age who were capable of creative thought in the field of science, bad they chosen similarly to "intend" their minds in the right direction. The difficulty was that they did not so choose. Their minds had a quite different bent. They were under the spell of different ideals; all their mental efforts were directed into different channels. What these different channels were cannot be in doubt--they were the channels of oriental ecclesiasticism. One all-significant fact speaks volumes here. It is the fact that, as Professor Robinson[1] points out, from the time of Boethius (died 524 or 525 A.D.) to that of Dante (1265-1321 A.D.) there was not a single writer of renown in western Europe who was not a professional churchman. All the learning of the time, then, centred in the priesthood. We know that the same condition of things pertained in Egypt, when science became static there. But, contrariwise, we have seen that in Greece and early Rome the scientific workers were largely physicians or professional teachers; there was scarcely a professional theologian among them.

Similarly, as we shall see in the Arabic world, where alone there was progress in the mediaeval epoch, the learned men were, for the most part, physicians. Now the meaning of this must be self-evident. The physician naturally "intends" his mind towards the practicalities. His professional studies tend to make him an investigator of the operations of nature. He is usually a sceptic, with a spontaneous interest in practical science. But the theologian "intends" his mind away from practicalities and towards mysticism. He is a professional believer in the supernatural; he discounts the value of merely "natural" phenomena. His whole attitude of mind is unscientific; the fundamental tenets of his faith are based on alleged occurrences which inductive science cannot admit--namely, miracles. And so the minds "intended" towards the supernatural achieved only the hazy mysticism of mediaeval thought. Instead of investigating natural laws, they paid heed (as, for example, Thomas Aquinas does in his Summa Theologia) to the "acts of angels," the "speaking of angels," the "subordination of angels," the "deeds of guardian angels,"

and the like. They disputed such important questions as, How many angels can stand upon the point of a needle? They argued pro and con as to whether Christ were coeval with God, or whether he had been merely created "in the beginning," perhaps ages before the creation of the world. How could it be expected that science should flourish when the greatest minds of the age could concern themselves with problems such as these?

Despite our preconceptions or prejudices, there can be but one answer to that question. Oriental superstition cast its blight upon the fair field of science, whatever compensation it may or may not have brought in other fields. But we must be on our guard lest we overestimate or incorrectly estimate this influence. Posterity, in glancing backward, is always prone to stamp any given age of the past with one idea, and to desire to characterize it with a single phrase; whereas in reality all ages are diversified, and any generalization regarding an epoch is sure to do that epoch something less or something more than justice. We may be sure, then, that the ideal of ecclesiasticism is not solely responsible for the scientific stasis of the dark age. Indeed, there was another influence of a totally different character that is too patent to be overlooked--the influence, namely, of the economic condition of western Europe during this period. As I have elsewhere pointed out,[2] Italy, the centre of western civilization, was at this time impoverished, and hence could not provide the monetary stimulus so essential to artistic and scientific no less than to material progress. There were no patrons of science and literature such as the Ptolemies of that elder Alexandrian day. There were no great libraries; no colleges to supply opportunities and afford stimuli to the rising generation. Worst of all, it became increasingly difficult to secure books.

This phase of the subject is often overlooked. Yet a moment's consideration will show its importance. How should we fare to-day if no new scientific books were being produced, and if the records of former generations were destroyed? That is what actually happened in Europe during the Middle Ages. At an earlier day books were made and distributed much more abundantly than is sometimes supposed. Bookmaking had, indeed, been an important profession in Rome, the actual makers of books being slaves who worked under the direction of a publisher. It was through the efforts of these workers that the classical works in Greek and Latin were multiplied and disseminated. Unfortunately the climate of Europe does not conduce to the indefinite preservation of a book; hence very few remnants of classical works have come down to us in the original from a remote period. The rare exceptions are certain papyrus fragments, found in Egypt, some of which are Greek manuscripts dating from the third century B.C. Even from these sources the output is meagre; and the only other repository of classical books is a single room in the buried city of Herculaneum, which contained several hundred manuscripts, mostly in a charred condition, a considerable number of which, however, have been unrolled and found more or less legible. This library in the buried city was chiefly made up of philosophical works, some of which were quite unknown to the modern world until discovered there.

But this find, interesting as it was from an archaeological stand-point, had no very important bearing on our knowledge of the literature of antiquity. Our chief dependence for our knowledge of that literature must still be placed in such copies of books as were made in the successive generations. Comparatively few of the extant manuscripts are older than the tenth century of our era. It requires but a momentary consideration of the conditions under which ancient books were produced to realize how slow and difficult the process was before the invention of printing. The taste of the book-buying public demanded a clearly written text, and in the Middle Ages it

became customary to produce a richly ornamented text as well. The script employed being the prototype of the modern printed text, it will be obvious that a scribe could produce but a few pages at best in a day. A large work would therefore require the labor of a scribe for many months or even for several years. We may assume, then, that it would be a very flourishing publisher who could produce a hundred volumes all told per annum; and probably there were not many publishers at any given time, even in the period of Rome's greatest glory, who had anything like this output.

As there was a large number of authors in every generation of the classical period, it follows that most of these authors must have been obliged to content themselves with editions numbering very few copies; and it goes without saying that the greater number of books were never reproduced in what might be called a second edition. Even books that retained their popularity for several generations would presently fail to arouse sufficient interest to be copied; and in due course such works would pass out of existence altogether. Doubtless many hundreds of books were thus lost before the close of the classical period, the names of their authors being quite forgotten, or preserved only through a chance reference; and of course the work of elimination went on much more rapidly during the Middle Ages, when the interest in classical literature sank to so low an ebb in the West. Such collections of references and quotations as the Greek Anthology and the famous anthologies of Stobaeus and Athanasius and Eusebius give us glimpses of a host of writers--more than seven hundred are quoted by Stobaeus--a very large proportion of whom are quite unknown except through these brief excerpts from their lost works.

Quite naturally the scientific works suffered at least as largely as any others in an age given over to ecclesiastical dreamings. Yet in some regards there is matter for surprise as to the works preserved. Thus, as we have seen, the very extensive works of Aristotle on natural history, and the equally extensive natural history of Pliny, which were preserved throughout this period, and are still extant, make up relatively bulky volumes. These works seem to have interested the monks of the Middle Ages, while many much more important scientific books were allowed to perish. A considerable bulk of scientific literature was also preserved through the curious channels of Arabic and Armenian translations. Reference has already been made to the Almagest of Ptolemy, which, as we have seen, was translated into Arabic, and which was at a later day brought by the Arabs into western Europe and (at the instance of Frederick II of Sicily) translated out of their language into mediaeval Latin.

It remains to inquire, however, through what channels the Greek works reached the Arabs themselves. To gain an answer to this question we must follow the stream of history from its Roman course eastward to the new seat of the Roman empire in Byzantium. Here civilization centred from about the fifth century A.D., and here the European came in contact with the civilization of the Syrians, the Persians, the Armenians, and finally of the Arabs. The Byzantines themselves, unlike the inhabitants of western Europe, did not ignore the literature of old Greece; the Greek language became the regular speech of the Byzantine people, and their writers made a strenuous effort to perpetuate the idiom and style of the classical period. Naturally they also made transcriptions of the classical authors, and thus a great mass of literature was preserved, while the corresponding works were quite forgotten in western Europe.

Meantime many of these works were translated into Syriac, Armenian, and Persian, and when later on the Byzantine civilization degenerated, many works that were no longer to be had in the

Greek originals continued to be widely circulated in Syriac, Persian, Armenian, and, ultimately, in Arabic translations. When the Arabs started out in their conquests, which carried them through Egypt and along the southern coast of the Mediterranean, until they finally invaded Europe from the west by way of Gibraltar, they carried with them their translations of many a Greek classical author, who was introduced anew to the western world through this strange channel.

We are told, for example, that Averrhoes, the famous commentator of Aristotle, who lived in Spain in the twelfth century, did not know a word of Greek and was obliged to gain his knowledge of the master through a Syriac translation; or, as others alleged (denying that he knew even Syriac), through an Arabic version translated from the Syriac. We know, too, that the famous chronology of Eusebius was preserved through an Armenian translation; and reference has more than once been made to the Arabic translation of Ptolemy's great work, to which we still apply its Arabic title of Almagest.

The familiar story that when the Arabs invaded Egypt they burned the Alexandrian library is now regarded as an invention of later times. It seems much more probable that the library bad been largely scattered before the coming of the Moslems. Indeed, it has even been suggested that the Christians of an earlier day removed the records of pagan thought. Be that as it may, the famous Alexandrian library had disappeared long before the revival of interest in classical learning. Meanwhile, as we have said, the Arabs, far from destroying the western literature, were its chief preservers. Partly at least because of their regard for the records of the creative work of earlier generations of alien peoples, the Arabs were enabled to outstrip their contemporaries. For it cannot be in doubt that, during that long stretch of time when the western world was ignoring science altogether or at most contenting itself with the casual reading of Aristotle and Pliny, the Arabs had the unique distinction of attempting original investigations in science. To them were due all important progressive steps which were made in any scientific field whatever for about a thousand years after the time of Ptolemy and Galen. The progress made even by the Arabs during this long period seems meagre enough, yet it has some significant features. These will now demand our attention.

II. MEDIAEVAL SCIENCE AMONG THE ARABIANS

The successors of Mohammed showed themselves curiously receptive of the ideas of the western people whom they conquered. They came in contact with the Greeks in western Asia and in Egypt, and, as has been said, became their virtual successors in carrying forward the torch of learning. It must not be inferred, however, that the Arabian scholars, as a class, were comparable to their predecessors in creative genius. On the contrary, they retained much of the conservative oriental spirit. They were under the spell of tradition, and, in the main, what they accepted from the Greeks they regarded as almost final in its teaching. There were, however, a few notable exceptions among their men of science, and to these must be ascribed several discoveries of some importance.

The chief subjects that excited the interest and exercised the ingenuity of the Arabian scholars were astronomy, mathematics, and medicine. The practical phases of all these subjects were given particular attention. Thus it is well known that our so-called Arabian numerals date from this period. The revolutionary effect of these characters, as applied to practical mathematics, can hardly be overestimated; but it is generally considered, and in fact was admitted by the Arabs themselves, that these numerals were really borrowed from the Hindoos, with whom the Arabs

came in contact on the east. Certain of the Hindoo alphabets, notably that of the Battaks of Sumatra, give us clews to the originals of the numerals. It does not seem certain, however, that the Hindoos employed these characters according to the decimal system, which is the prime element of their importance. Knowledge is not forthcoming as to just when or by whom such application was made. If this was an Arabic innovation, it was perhaps the most important one with which that nation is to be credited. Another mathematical improvement was the introduction into trigonometry of the sine--the half-chord of the double arc--instead of the chord of the arc itself which the Greek astronomers had employed. This improvement was due to the famous Albategnius, whose work in other fields we shall examine in a moment.

Another evidence of practicality was shown in the Arabian method of attempting to advance upon Eratosthenes' measurement of the earth. Instead of trusting to the measurement of angles, the Arabs decided to measure directly a degree of the earth's surface--or rather two degrees. Selecting a level plain in Mesopotamia for the experiment, one party of the surveyors progressed northward, another party southward, from a given point to the distance of one degree of arc, as determined by astronomical observations. The result found was fifty-six miles for the northern degree, and fifty-six and two-third miles for the southern. Unfortunately, we do not know the precise length of the mile in question, and therefore cannot be assured as to the accuracy of the measurement. It is interesting to note, however, that the two degrees were found of unequal lengths, suggesting that the earth is not a perfect sphere--a suggestion the validity of which was not to be put to the test of conclusive measurements until about the close of the eighteenth century. The Arab measurement was made in the time of Caliph Abdallah al-Mamun, the son of the famous Harun-al-Rashid. Both father and son were famous for their interest in science. Harun-al-Rashid was, it will be recalled, the friend of Charlemagne. It is said that he sent that ruler, as a token of friendship, a marvellous clock which let fall a metal ball to mark the hours. This mechanism, which is alleged to have excited great wonder in the West, furnishes yet another instance of Arabian practicality.

Perhaps the greatest of the Arabian astronomers was Mohammed ben Jabir Albategnius, or El-batani, who was born at Batan, in Mesopotamia, about the year 850 A.D., and died in 929. Albategnius was a student of the Ptolemaic astronomy, but he was also a practical observer. He made the important discovery of the motion of the solar apogee. That is to say, he found that the position of the sun among the stars, at the time of its greatest distance from the earth, was not what it had been in the time of Ptolemy. The Greek astronomer placed the sun in longitude 65 degrees, but Albategnius found it in longitude 82 degrees, a distance too great to be accounted for by inaccuracy of measurement. The modern inference from this observation is that the solar system is moving through space; but of course this inference could not well be drawn while the earth was regarded as the fixed centre of the universe.

In the eleventh century another Arabian discoverer, Arzachel, observing the sun to be less advanced than Albategnius had found it, inferred incorrectly that the sun had receded in the mean time. The modern explanation of this observation is that the measurement of Albategnius was somewhat in error, since we know that the sun's motion is steadily progressive. Arzachel, however, accepting the measurement of his predecessor, drew the false inference of an oscillatory motion of the stars, the idea of the motion of the solar system not being permissible. This assumed phenomenon, which really has no existence in point of fact, was named the "trepidation of the fixed stars," and was for centuries accepted as an actual phenomenon.

Arzachel explained this supposed phenomenon by assuming that the equinoctial points, or the points of intersection of the equator and the ecliptic, revolve in circles of eight degrees' radius. The first points of Aries and Libra were supposed to describe the circumference of these circles in about eight hundred years. All of which illustrates how a difficult and false explanation may take the place of a simple and correct one. The observations of later generations have shown conclusively that the sun's shift of position is regularly progressive, hence that there is no "trepidation" of the stars and no revolution of the equinoctial points.

If the Arabs were wrong as regards this supposed motion of the fixed stars, they made at least one correct observation as to the inequality of motion of the moon. Two inequalities of the motion of this body were already known. A third, called the moon's variation, was discovered by an Arabian astronomer who lived at Cairo and observed at Bagdad in 975, and who bore the formidable name of Mohammed Aboul Wefaal-Bouzdjani. The inequality of motion in question, in virtue of which the moon moves quickest when she is at new or full, and slowest at the first and third quarter, was rediscovered by Tycho Brahe six centuries later; a fact which in itself evidences the neglect of the Arabian astronomer's discovery by his immediate successors.

In the ninth and tenth centuries the Arabian city of Cordova, in Spain, was another important centre of scientific influence. There was a library of several hundred thousand volumes here, and a college where mathematics and astronomy were taught. Granada, Toledo, and Salamanca were also important centres, to which students flocked from western Europe. It was the proximity of these Arabian centres that stimulated the scientific interests of Alfonso X. of Castile, at whose instance the celebrated Alfonsine tables were constructed. A familiar story records that Alfonso, pondering the complications of the Ptolemaic cycles and epicycles, was led to remark that, had he been consulted at the time of creation, he could have suggested a much better and simpler plan for the universe. Some centuries were to elapse before Copernicus was to show that it was not the plan of the universe, but man's interpretation of it, that was at fault.

Another royal personage who came under Arabian influence was Frederick II. of Sicily--the "Wonder of the World," as he was called by his contemporaries. The Almagest of Ptolemy was translated into Latin at his instance, being introduced to the Western world through this curious channel. At this time it became quite usual for the Italian and Spanish scholars to understand Arabic although they were totally ignorant of Greek.

In the field of physical science one of the most important of the Arabian scientists was Alhazen. His work, published about the year 1100 A.D., had great celebrity throughout the mediaeval period. The original investigations of Alhazen had to do largely with optics. He made particular studies of the eye itself, and the names given by him to various parts of the eye, as the vitreous humor, the cornea, and the retina, are still retained by anatomists. It is known that Ptolemy had studied the refraction of light, and that he, in common with his immediate predecessors, was aware that atmospheric refraction affects the apparent position of stars near the horizon. Alhazen carried forward these studies, and was led through them to make the first recorded scientific estimate of the phenomena of twilight and of the height of the atmosphere. The persistence of a glow in the atmosphere after the sun has disappeared beneath the horizon is so familiar a phenomenon that the ancient philosophers seem not to have thought of it as requiring an explanation. Yet a moment's consideration makes it clear that, if light travels in straight lines and the rays of the sun were in no wise deflected, the complete darkness of night should instantly

succeed to day when the sun passes below the horizon. That this sudden change does not occur, Alhazen explained as due to the reflection of light by the earth's atmosphere.

Alhazen appears to have conceived the atmosphere as a sharply defined layer, and, assuming that twilight continues only so long as rays of the sun reflected from the outer surface of this layer can reach the spectator at any given point, he hit upon a means of measurement that seemed to solve the hitherto inscrutable problem as to the atmospheric depth. Like the measurements of Aristarchus and Eratosthenes, this calculation of Alhazen is simple enough in theory. Its defect consists largely in the difficulty of fixing its terms with precision, combined with the further fact that the rays of the sun, in taking the slanting course through the earth's atmosphere, are really deflected from a straight line in virtue of the constantly increasing density of the air near the earth's surface. Alhazen must have been aware of this latter fact, since it was known to the later Alexandrian astronomers, but he takes no account of it in the present measurement. The diagram will make the method of Alhazen clear.

His important premises are two: first, the well-recognized fact that, when light is reflected from any surface, the angle of incidence is equal to the angle of reflection; and, second, the much more doubtful observation that twilight continues until such time as the sun, according to a simple calculation, is nineteen degrees below the horizon. Referring to the diagram, let the inner circle represent the earth's surface, the outer circle the limits of the atmosphere, C being the earth's centre, and RR radii of the earth. Then the observer at the point A will continue to receive the reflected rays of the sun until that body reaches the point S, which is, according to the hypothesis, nineteen degrees below the horizon line of the observer at A. This horizon line, being represented by AH, and the sun's ray by SM, the angle HMS is an angle of nineteen degrees. The complementary angle SMA is, obviously, an angle of (180-19) one hundred and sixty-one degrees. But since M is the reflecting surface and the angle of incidence equals the angle of reflection, the angle AMC is an angle of one-half of one hundred and sixty-one degrees, or eighty degrees and thirty minutes. Now this angle AMC, being known, the right-angled triangle MAC is easily resolved, since the side AC of that triangle, being the radius of the earth, is a known dimension. Resolution of this triangle gives us the length of the hypotenuse MC, and the difference between this and the radius (AC), or CD, is obviously the height of the atmosphere (h), which was the measurement desired. According to the calculation of Alhazen, this h, or the height of the atmosphere, represents from twenty to thirty miles. The modern computation extends this to about fifty miles. But, considering the various ambiguities that necessarily attended the experiment, the result was a remarkably close approximation to the truth.

Turning from physics to chemistry, we find as perhaps the greatest Arabian name that of Geber, who taught in the College of Seville in the first half of the eighth century. The most important researches of this really remarkable experimenter had to do with the acids. The ancient world had had no knowledge of any acid more powerful than acetic. Geber, however, vastly increased the possibilities of chemical experiment by the discovery of sulphuric, nitric, and nitromuriatic acids. He made use also of the processes of sublimation and filtration, and his works describe the water bath and the chemical oven. Among the important chemicals which he first differentiated is oxide of mercury, and his studies of sulphur in its various compounds have peculiar interest. In particular is this true of his observation that, tinder certain conditions of oxidation, the weight of a metal was lessened.

From the record of these studies in the fields of astronomy, physics, and chemistry, we turn to a somewhat extended survey of the Arabian advances in the field of medicine.

ARABIAN MEDICINE

The influence of Arabian physicians rested chiefly upon their use of drugs rather than upon anatomical knowledge. Like the mediaeval Christians, they looked with horror on dissection of the human body; yet there were always among them investigators who turned constantly to nature herself for hidden truths, and were ready to uphold the superiority of actual observation to mere reading. Thus the physician Abd el-Letif, while in Egypt, made careful studies of a mound of bones containing more than twenty thousand skeletons. While examining these bones he discovered that the lower jaw consists of a single bone, not of two, as had been taught by Galen. He also discovered several other important mistakes in Galenic anatomy, and was so impressed with his discoveries that he contemplated writing a work on anatomy which should correct the great classical authority's mistakes.

It was the Arabs who invented the apothecary, and their pharmacopoeia, issued from the hospital at Gondisapor, and elaborated from time to time, formed the basis for Western pharmacopoeias. Just how many drugs originated with them, and how many were borrowed from the Hindoos, Jews, Syrians, and Persians, cannot be determined. It is certain, however, that through them various new and useful drugs, such as senna, aconite, rhubarb, camphor, and mercury, were handed down through the Middle Ages, and that they are responsible for the introduction of alcohol in the field of therapeutics.

In mediaeval Europe, Arabian science came to be regarded with superstitious awe, and the works of certain Arabian physicians were exalted to a position above all the ancient writers. In modern times, however, there has been a reaction and a tendency to depreciation of their work. By some they are held to be mere copyists or translators of Greek books, and in no sense original investigators in medicine. Yet there can be little doubt that while the Arabians did copy and translate freely, they also originated and added considerably to medical knowledge. It is certain that in the time when Christian monarchs in western Europe were paying little attention to science or education, the caliphs and vizirs were encouraging physicians and philosophers, building schools, and erecting libraries and hospitals. They made at least a creditable effort to uphold and advance upon the scientific standards of an earlier age.

The first distinguished Arabian physician was Harets ben Kaladah, who received his education in the Nestonian school at Gondisapor, about the beginning of the seventh century. Notwithstanding the fact that Harets was a Christian, he was chosen by Mohammed as his chief medical adviser, and recommended as such to his successor, the Caliph Abu Bekr. Thus, at the very outset, the science of medicine was divorced from religion among the Arabians; for if the prophet himself could employ the services of an unbeliever, surely others might follow his example. And that this example was followed is shown in the fact that many Christian physicians were raised to honorable positions by succeeding generations of Arabian monarchs. This broad-minded view of medicine taken by the Arabs undoubtedly assisted as much as any one single factor in upbuilding the science, just as the narrow and superstitious view taken by Western nations helped to destroy it.

The education of the Arabians made it natural for them to associate medicine with the natural

sciences, rather than with religion. An Arabian savant was supposed to be equally well educated in philosophy, jurisprudence, theology, mathematics, and medicine, and to practise law, theology, and medicine with equal skill upon occasion. It is easy to understand, therefore, why these religious fanatics were willing to employ unbelieving physicians, and their physicians themselves to turn to the scientific works of Hippocrates and Galen for medical instruction, rather than to religious works. Even Mohammed himself professed some knowledge of medicine, and often relied upon this knowledge in treating ailments rather than upon prayers or incantations. He is said, for example, to have recommended and applied the cautery in the case of a friend who, when suffering from angina, had sought his aid.

The list of eminent Arabian physicians is too long to be given here, but some of them are of such importance in their influence upon later medicine that they cannot be entirely ignored. One of the first of these was Honain ben Isaac (809-873 A.D.), a Christian Arab of Bagdad. He made translations of the works of Hippocrates, and practised the art along the lines indicated by his teachings and those of Galen. He is considered the greatest translator of the ninth century and one of the greatest philosophers of that period.

Another great Arabian physician, whose work was just beginning as Honain's was drawing to a close, was Rhazes (850-923 A.D.), who during his life was no less noted as a philosopher and musician than as a physician. He continued the work of Honain, and advanced therapeutics by introducing more extensive use of chemical remedies, such as mercurial ointments, sulphuric acid, and aqua vitae. He is also credited with being the first physician to describe small-pox and measles accurately.

While Rhazes was still alive another Arabian, Haly Abbas (died about 994), was writing his famous encyclopaedia of medicine, called The Royal Book. But the names of all these great physicians have been considerably obscured by the reputation of Avicenna (980-1037), the Arabian "Prince of Physicians," the greatest name in Arabic medicine, and one of the most remarkable men in history. Leclerc says that "he was perhaps never surpassed by any man in brilliancy of intellect and indefatigable activity." His career was a most varied one. He was at all times a boisterous reveller, but whether flaunting gayly among the guests of an emir or biding in some obscure apothecary cellar, his work of philosophical writing was carried on steadily. When a friendly emir was in power, he taught and wrote and caroused at court; but between times, when some unfriendly ruler was supreme, he was hiding away obscurely, still pouring out his great mass of manuscripts. In this way his entire life was spent.

By his extensive writings he revived and kept alive the best of the teachings of the Greek physicians, adding to them such observations as he had made in anatomy, physiology, and materia medica. Among his discoveries is that of the contagiousness of pulmonary tuberculosis. His works for several centuries continued to be looked upon as the highest standard by physicians, and he should undoubtedly be credited with having at least retarded the decline of mediaeval medicine.

But it was not the Eastern Arabs alone who were active in the field of medicine. Cordova, the capital of the western caliphate, became also a great centre of learning and produced several great physicians. One of these, Albucasis (died in 1013 A.D.), is credited with having published the first illustrated work on surgery, this book being remarkable in still another way, in that it was also the first book, since classical times, written from the practical experience of the

physician, and not a mere compilation of ancient authors. A century after Albucasis came the great physician Avenzoar (1113-1196), with whom he divides about equally the medical honors of the western caliphate. Among Avenzoar's discoveries was that of the cause of "itch"--a little parasite, "so small that he is hardly visible." The discovery of the cause of this common disease seems of minor importance now, but it is of interest in medical history because, had Avenzoar's discovery been remembered a hundred years ago, "itch struck in" could hardly have been considered the cause of three-fourths of all diseases, as it was by the famous Hahnemann.

The illustrious pupil of Avenzoar, Averrhoes, who died in 1198 A.D., was the last of the great Arabian physicians who, by rational conception of medicine, attempted to stem the flood of superstition that was overwhelming medicine. For a time he succeeded; but at last the Moslem theologians prevailed, and he was degraded and banished to a town inhabited only by the despised Jews.

ARABIAN HOSPITALS

To early Christians belong the credit of having established the first charitable institutions for caring for the sick; but their efforts were soon eclipsed by both Eastern and Western Mohammedans. As early as the eighth century the Arabs had begun building hospitals, but the flourishing time of hospital building seems to have begun early in the tenth century. Lady Seidel, in 918 A.D., opened a hospital at Bagdad, endowed with an amount corresponding to about three hundred pounds sterling a month. Other similar hospitals were erected in the years immediately following, and in 977 the Emir Adad-adaula established an enormous institution with a staff of twenty-four medical officers. The great physician Rhazes is said to have selected the site for one of these hospitals by hanging pieces of meat in various places about the city, selecting the site near the place at which putrefaction was slowest in making its appearance. By the middle of the twelfth century there were something like sixty medical institutions in Bagdad alone, and these institutions were free to all patients and supported by official charity.

The Emir Nureddin, about the year 1160, founded a great hospital at Damascus, as a thank-offering for his victories over the Crusaders. This great institution completely overshadowed all the earlier Moslem hospitals in size and in the completeness of its equipment. It was furnished with facilities for teaching, and was conducted for several centuries in a lavish manner, regardless of expense. But little over a century after its foundation the fame of its methods of treatment led to the establishment of a larger and still more luxurious institution--the Mansuri hospital at Cairo. It seems that a certain sultan, having been cured by medicines from the Damascene hospital, determined to build one of his own at Cairo which should eclipse even the great Damascene institution.

In a single year (1283-1284) this hospital was begun and completed. No efforts were spared in hurrying on the good work, and no one was exempt from performing labor on the building if he chanced to pass one of the adjoining streets. It was the order of the sultan that any person passing near could be impressed into the work, and this order was carried out to the letter, noblemen and beggars alike being forced to lend a hand. Very naturally, the adjacent thoroughfares became unpopular and practically deserted, but still the holy work progressed rapidly and was shortly completed.

This immense structure is said to have contained four courts, each having a fountain in the

centre; lecture-halls, wards for isolating certain diseases, and a department that corresponded to the modern hospital's "out-patient" department. The yearly endowment amounted to something like the equivalent of one hundred and twenty-five thousand dollars. A novel feature was a hall where musicians played day and night, and another where story-tellers were employed, so that persons troubled with insomnia were amused and melancholiacs cheered. Those of a religious turn of mind could listen to readings of the Koran, conducted continuously by a staff of some fifty chaplains. Each patient on leaving the hospital received some gold pieces, that he need not be obliged to attempt hard labor at once.

In considering the astonishing tales of these sumptuous Arabian institutions, it should be borne in mind that our accounts of them are, for the most part, from Mohammedan sources. Nevertheless, there can be little question that they were enormous institutions, far surpassing any similar institutions in western Europe. The so-called hospitals in the West were, at this time, branches of monasteries under supervision of the monks, and did not compare favorably with the Arabian hospitals.

But while the medical science of the Mohammedans greatly overshadowed that of the Christians during this period, it did not completely obliterate it. About the year 1000 A.D. came into prominence the Christian medical school at Salerno, situated on the Italian coast, some thirty miles southeast of Naples. Just how long this school had been in existence, or by whom it was founded, cannot be determined, but its period of greatest influence was the eleventh, twelfth, and thirteenth centuries. The members of this school gradually adopted Arabic medicine, making use of many drugs from the Arabic pharmacopoeia, and this formed one of the stepping-stones to the introduction of Arabian medicine all through western Europe.

It was not the adoption of Arabian medicines, however, that has made the school at Salerno famous both in rhyme and prose, but rather the fact that women there practised the healing art. Greatest among them was Trotula, who lived in the eleventh century, and whose learning is reputed to have equalled that of the greatest physicians of the day. She is accredited with a work on Diseases of Women, still extant, and many of her writings on general medical subjects were quoted through two succeeding centuries. If we may judge from these writings, she seemed to have had many excellent ideas as to the proper methods of treating diseases, but it is difficult to determine just which of the writings credited to her are in reality hers. Indeed, the uncertainty is even greater than this implies, for, according to some writers, "Trotula" is merely the title of a book. Such an authority as Malgaigne, however, believed that such a woman existed, and that the works accredited to her are authentic. The truth of the matter may perhaps never be fully established, but this at least is certain--the tradition in regard to Trotula could never have arisen had not women held a far different position among the Arabians of this period from that accorded them in contemporary Christendom.

III. MEDIAEVAL SCIENCE IN THE WEST

We have previously referred to the influence of the Byzantine civilization in transmitting the learning of antiquity across the abysm of the dark age. It must be admitted, however, that the importance of that civilization did not extend much beyond the task of the common carrier. There were no great creative scientists in the later Roman empire of the East any more than in the corresponding empire of the West. There was, however, one field in which the Byzantine made respectable progress and regarding which their efforts require a few words of special

comment. This was the field of medicine.

The Byzantines of this time could boast of two great medical men, Aetius of Amida (about 502-575 A.D.) and Paul of Aegina (about 620-690). The works of Aetius were of value largely because they recorded the teachings of many of his eminent predecessors, but he was not entirely lacking in originality, and was perhaps the first physician to mention diphtheria, with an allusion to some observations of the paralysis of the palate which sometimes follows this disease.

Paul of Aegina, who came from the Alexandrian school about a century later, was one of those remarkable men whose ideas are centuries ahead of their time. This was particularly true of Paul in regard to surgery, and his attitude towards the supernatural in the causation and treatment of diseases. He was essentially a surgeon, being particularly familiar with military surgery, and some of his descriptions of complicated and difficult operations have been little improved upon even in modern times. In his books he describes such operations as the removal of foreign bodies from the nose, ear, and esophagus; and he recognizes foreign growths such as polypi in the air-passages, and gives the method of their removal. Such operations as tracheotomy, tonsellotomy, bronchotomy, staphylotomy, etc., were performed by him, and he even advocated and described puncture of the abdominal cavity, giving careful directions as to the location in which such punctures should be made. He advocated amputation of the breast for the cure of cancer, and described extirpation of the uterus. Just how successful this last operation may have been as performed by him does not appear; but he would hardly have recommended it if it had not been sometimes, at least, successful. That he mentions it at all, however, is significant, as this difficult operation is considered one of the great triumphs of modern surgery.

But Paul of Aegina is a striking exception to the rule among Byzantine surgeons, and as he was their greatest, so he was also their last important surgeon. The energies of all Byzantium were so expended in religious controversies that medicine, like the other sciences, was soon relegated to a place among the other superstitions, and the influence of the Byzantine school was presently replaced by that of the conquering Arabians.

THIRTEENTH-CENTURY MEDICINE

The thirteenth century marks the beginning of a gradual change in medicine, and a tendency to leave the time-worn rut of superstitious dogmas that so long retarded the progress of science. It is thought that the great epidemics which raged during the Middle Ages acted powerfully in diverting the medical thought of the times into new and entirely different channels. It will be remembered that the teachings of Galen were handed through mediaeval times as the highest and best authority on the subject of all diseases. When, however, the great epidemics made their appearance, the medical men appealed to the works of Galen in vain for enlightenment, as these works, having been written several centuries before the time of the plagues, naturally contained no information concerning them. It was evident, therefore, that on this subject, at least, Galen was not infallible; and it would naturally follow that, one fallible point having been revealed, others would be sought for. In other words, scepticism in regard to accepted methods would be aroused, and would lead naturally, as such scepticism usually does, to progress. The devastating effects of these plagues, despite prayers and incantations, would arouse doubt in the minds of many as to the efficacy of superstitious rites and ceremonies in curing diseases. They had seen thousands and tens of thousands of their fellow-beings swept away by these awful scourges. They had seen the ravages of these epidemics continue for months or even years,

notwithstanding the fact that multitudes of God-fearing people prayed hourly that such ravages might be checked. And they must have observed also that when even very simple rules of cleanliness and hygiene were followed there was a diminution in the ravages of the plague, even without the aid of incantations. Such observations as these would have a tendency to awaken a suspicion in the minds of many of the physicians that disease was not a manifestation of the supernatural, but a natural phenomenon, to be treated by natural methods.

But, be the causes what they may, it is a fact that the thirteenth century marks a turning-point, or the beginning of an attitude of mind which resulted in bringing medicine to a much more rational position. Among the thirteenth-century physicians, two men are deserving of special mention. These are Arnald of Villanova (1235-1312) and Peter of Abano (1250-1315). Both these men suffered persecution for expressing their belief in natural, as against the supernatural, causes of disease, and at one time Arnald was obliged to flee from Barcelona for declaring that the "bulls" of popes were human works, and that "acts of charity were dearer to God than hecatombs." He was also accused of alchemy. Fleeing from persecution, he finally perished by shipwreck.

Arnald was the first great representative of the school of Montpellier. He devoted much time to the study of chemicals, and was active in attempting to re-establish the teachings of Hippocrates and Galen. He was one of the first of a long line of alchemists who, for several succeeding centuries, expended so much time and energy in attempting to find the "elixir of life." The Arab discovery of alcohol first deluded him into the belief that the "elixir" had at last been found; but later he discarded it and made extensive experiments with brandy, employing it in the treatment of certain diseases--the first record of the administration of this liquor as a medicine. Arnald also revived the search for some anaesthetic that would produce insensibility to pain in surgical operations. This idea was not original with him, for since very early times physicians had attempted to discover such an anaesthetic, and even so early a writer as Herodotus tells how the Scythians, by inhalation of the vapors of some kind of hemp, produced complete insensibility. It may have been these writings that stimulated Arnald to search for such an anaesthetic. In a book usually credited to him, medicines are named and methods of administration described which will make the patient insensible to pain, so that "he may be cut and feel nothing, as though he were dead." For this purpose a mixture of opium, mandragora, and henbane is to be used. This mixture was held at the patient's nostrils much as ether and chloroform are administered by the modern surgeon. The method was modified by Hugo of Lucca (died in 1252 or 1268), who added certain other narcotics, such as hemlock, to the mixture, and boiled a new sponge in this decoction. After boiling for a certain time, this sponge was dried, and when wanted for use was dipped in hot water and applied to the nostrils.

Just how frequently patients recovered from the administration of such a combination of powerful poisons does not appear, but the percentage of deaths must have been very high, as the practice was generally condemned. Insensibility could have been produced only by swallowing large quantities of the liquid, which dripped into the nose and mouth when the sponge was applied, and a lethal quantity might thus be swallowed. The method was revived, with various modifications, from time to time, but as often fell into disuse. As late as 1782 it was sometimes attempted, and in that year the King of Poland is said to have been completely anaesthetized and to have recovered, after a painless amputation had been performed by the surgeons.

Peter of Abano was one of the first great men produced by the University of Padua. His fate

would have been even more tragic than that of the shipwrecked Arnald had he not cheated the purifying fagots of the church by dying opportunely on the eve of his execution for heresy. But if his spirit had cheated the fanatics, his body could not, and his bones were burned for his heresy. He had dared to deny the existence of a devil, and had suggested that the case of a patient who lay in a trance for three days might help to explain some miracles, like the raising of Lazarus.

His great work was Conciliator Differentiarum, an attempt to reconcile physicians and philosophers. But his researches were not confined to medicine, for he seems to have had an inkling of the hitherto unknown fact that air possesses weight, and his calculation of the length of the year at three hundred and sixty-five days, six hours, and four minutes, is exceptionally accurate for the age in which he lived. He was probably the first of the Western writers to teach that the brain is the source of the nerves, and the heart the source of the vessels. From this it is seen that he was groping in the direction of an explanation of the circulation of the blood, as demonstrated by Harvey three centuries later.

The work of Arnald and Peter of Abano in "reviving" medicine was continued actively by Mondino (1276-1326) of Bologna, the "restorer of anatomy," and by Guy of Chauliac: (born about 1300), the "restorer of surgery." All through the early Middle Ages dissections of human bodies had been forbidden, and even dissection of the lower animals gradually fell into disrepute because physicians detected in such practices were sometimes accused of sorcery. Before the close of the thirteenth century, however, a reaction had begun, physicians were protected, and dissections were occasionally sanctioned by the ruling monarch. Thus Emperor Frederick H. (1194-1250 A.D.)--whose services to science we have already had occasion to mention--ordered that at least one human body should be dissected by physicians in his kingdom every five years. By the time of Mondino dissections were becoming more frequent, and he himself is known to have dissected and demonstrated several bodies. His writings on anatomy have been called merely plagiarisms of Galen, but in all probability be made many discoveries independently, and on the whole, his work may be taken as more advanced than Galen's. His description of the heart is particularly accurate, and he seems to have come nearer to determining the course of the blood in its circulation than any of his predecessors. In this quest he was greatly handicapped by the prevailing belief in the idea that blood-vessels must contain air as well as blood, and this led him to assume that one of the cavities of the heart contained "spirits," or air. It is probable, however, that his accurate observations, so far as they went, were helpful stepping-stones to Harvey in his discovery of the circulation.

Guy of Chauliac, whose innovations in surgery reestablished that science on a firm basis, was not only one of the most cultured, but also the most practical surgeon of his time. He had great reverence for the works of Galen, Albucasis, and others of his noted predecessors; but this reverence did not blind him to their mistakes nor prevent him from using rational methods of treatment far in advance of theirs. His practicality is shown in some of his simple but useful inventions for the sick-room, such as the device of a rope, suspended from the ceiling over the bed, by which a patient may move himself about more easily; and in some of his improvements in surgical dressings, such as stiffening bandages by dipping them in the white of an egg so that they are held firmly. He treated broken limbs in the suspended cradle still in use, and introduced the method of making "traction" on a broken limb by means of a weight and pulley, to prevent deformity through shortening of the member. He was one of the first physicians to recognize the utility of spectacles, and recommended them in cases not amenable to treatment with lotions and

eye-waters. In some of his surgical operations, such as trephining for fracture of the skull, his technique has been little improved upon even in modern times. In one of these operations he successfully removed a portion of a man's brain.

Surgery was undoubtedly stimulated greatly at this period by the constant wars. Lay physicians, as a class, had been looked down upon during the Dark Ages; but with the beginning of the return to rationalism, the services of surgeons on the battle-field, to remove missiles from wounds, and to care for wounds and apply dressings, came to be more fully appreciated. In return for his labors the surgeon was thus afforded better opportunities for observing wounds and diseases, which led naturally to a gradual improvement in surgical methods.

FIFTEENTH-CENTURY MEDICINE

The thirteenth and fourteenth centuries had seen some slight advancement in the science of medicine; at least, certain surgeons and physicians, if not the generality, had made advances; but it was not until the fifteenth century that the general revival of medical learning became assured. In this movement, naturally, the printing-press played an all-important part. Medical books, hitherto practically inaccessible to the great mass of physicians, now became common, and this output of reprints of Greek and Arabic treatises revealed the fact that many of the supposed true copies were spurious. These discoveries very naturally aroused all manner of doubt and criticism, which in turn helped in the development of independent thought.

A certain manuscript of the great Cornelius Celsus, the De Medicine, which had been lost for many centuries, was found in the church of St. Ambrose, at Milan, in 1443, and was at once put into print. The effect of the publication of this book, which had lain in hiding for so many centuries, was a revelation, showing the medical profession how far most of their supposed true copies of Celsus had drifted away from the original. The indisputable authenticity of this manuscript, discovered and vouched for by the man who shortly after became Pope Nicholas V., made its publication the more impressive. The output in book form of other authorities followed rapidly, and the manifest discrepancies between such teachers as Celsus, Hippocrates, Galen, and Pliny heightened still more the growing spirit of criticism.

These doubts resulted in great controversies as to the proper treatment of certain diseases, some physicians following Hippocrates, others Galen or Celsus, still others the Arabian masters. One of the most bitter of these contests was over the question of "revulsion," and "derivation"--that is, whether in cases of pleurisy treated by bleeding, the venesection should be made at a point distant from the seat of the disease, as held by the "revulsionists," or at a point nearer and on the same side of the body, as practised by the "derivationists." That any great point for discussion could be raised in the fifteenth or sixteenth centuries on so simple a matter as it seems to-day shows how necessary to the progress of medicine was the discovery of the circulation of the blood made by Harvey two centuries later. After Harvey's discovery no such discussion could have been possible, because this discovery made it evident that as far as the general effect upon the circulation is concerned, it made little difference whether the bleeding was done near a diseased part or remote from it. But in the sixteenth century this question was the all-absorbing one among the doctors. At one time the faculty of Paris condemned "derivation"; but the supporters of this method carried the war still higher, and Emperor Charles V. himself was appealed to. He reversed the decision of the Paris faculty, and decided in favor of "derivation." His decision was further supported by Pope Clement VII., although the discussion dragged on

until cut short by Harvey's discovery.

But a new form of injury now claimed the attention of the surgeons, something that could be decided by neither Greek nor Arabian authors, as the treatment of gun-shot wounds was, for obvious reasons, not given in their writings. About this time, also, came the great epidemics, "the sweating sickness" and scurvy; and upon these subjects, also, the Greeks and Arabians were silent. John of Vigo, in his book, the Practica Copiosa, published in 1514, and repeated in many editions, became the standard authority on all these subjects, and thus supplanted the works of the ancient writers.

According to Vigo, gun-shot wounds differed from the wounds made by ordinary weapons--that is, spear, arrow, sword, or axe--in that the bullet, being round, bruised rather than cut its way through the tissues; it burned the flesh; and, worst of all, it poisoned it. Vigo laid especial stress upon treating this last condition, recommending the use of the cautery or the oil of elder, boiling hot. It is little wonder that gun-shot wounds were so likely to prove fatal. Yet, after all, here was the germ of the idea of antisepsis.

NEW BEGINNINGS IN GENERAL SCIENCE

We have dwelt thus at length on the subject of medical science, because it was chiefly in this field that progress was made in the Western world during the mediaeval period, and because these studies furnished the point of departure for the revival all along the line. It will be understood, however, from what was stated in the preceding chapter, that the Arabian influences in particular were to some extent making themselves felt along other lines. The opportunity afforded a portion of the Western world--notably Spain and Sicily --to gain access to the scientific ideas of antiquity through Arabic translations could not fail of influence. Of like character, and perhaps even more pronounced in degree, was the influence wrought by the Byzantine refugees, who, when Constantinople began to be threatened by the Turks, migrated to the West in considerable numbers, bringing with them a knowledge of Greek literature and a large number of precious works which for centuries had been quite forgotten or absolutely ignored in Italy. Now Western scholars began to take an interest in the Greek language, which had been utterly neglected since the beginning of the Middle Ages. Interesting stories are told of the efforts made by such men as Cosmo de' Medici to gain possession of classical manuscripts. The revival of learning thus brought about had its first permanent influence in the fields of literature and art, but its effect on science could not be long delayed. Quite independently of the Byzantine influence, however, the striving for better intellectual things had manifested itself in many ways before the close of the thirteenth century. An illustration of this is found in the almost simultaneous development of centres of teaching, which developed into the universities of Italy, France, England, and, a little later, of Germany.

The regular list of studies that came to be adopted everywhere comprised seven nominal branches, divided into two groups--the so-called quadrivium, comprising music, arithmetic, geometry, and astronomy; and the trivium comprising grammar, rhetoric, and logic. The vagueness of implication of some of these branches gave opportunity to the teacher for the promulgation of almost any knowledge of which he might be possessed, but there can be no doubt that, in general, science had but meagre share in the curriculum. In so far as it was given representation, its chief field must have been Ptolemaic astronomy. The utter lack of scientific thought and scientific method is illustrated most vividly in the works of the greatest men of that

period--such men as Albertus Magnus, Thomas Aquinas, Bonaventura, and the hosts of other scholastics of lesser rank. Yet the mental awakening implied in their efforts was sure to extend to other fields, and in point of fact there was at least one contemporary of these great scholastics whose mind was intended towards scientific subjects, and who produced writings strangely at variance in tone and in content with the others. This anachronistic thinker was the English monk, Roger Bacon.

ROGER BACON

Bacon was born in 1214 and died in 1292. By some it is held that he was not appreciated in his own time because he was really a modern scientist living in an age two centuries before modern science or methods of modern scientific thinking were known. Such an estimate, however, is a manifest exaggeration of the facts, although there is probably a grain of truth in it withal. His learning certainly brought him into contact with the great thinkers of the time, and his writings caused him to be imprisoned by his fellow-churchmen at different times, from which circumstances we may gather that he was advanced thinker, even if not a modern scientist.

Although Bacon was at various times in durance, or under surveillance, and forbidden to write, he was nevertheless a marvellously prolific writer, as is shown by the numerous books and unpublished manuscripts of his still extant. His master-production was the Opus Majus. In Part IV. of this work he attempts to show that all sciences rest ultimately on mathematics; but Part V., which treats of perspective, is of particular interest to modern scientists, because in this he discusses reflection and refraction, and the properties of mirrors and lenses. In this part, also, it is evident that he is making use of such Arabian writers as Alkindi and Alhazen, and this is of especial interest, since it has been used by his detractors, who accuse him of lack of originality, to prove that his seeming inventions and discoveries were in reality adaptations of the Arab scientists. It is difficult to determine just how fully such criticisms are justified. It is certain, however, that in this part he describes the anatomy of the eye with great accuracy, and discusses mirrors and lenses.

The magnifying power of the segment of a glass sphere had been noted by Alhazen, who had observed also that the magnification was increased by increasing the size of the segment used. Bacon took up the discussion of the comparative advantages of segments, and in this discussion seems to show that he understood how to trace the progress of the rays of light through a spherical transparent body, and how to determine the place of the image. He also described a method of constructing a telescope, but it is by no means clear that he had ever actually constructed such an instrument. It is also a mooted question as to whether his instructions as to the construction of such an instrument would have enabled any one to construct one. The vagaries of the names of terms as he uses them allow such latitude in interpretation that modern scientists are not agreed as to the practicability of Bacon's suggestions. For example, he constantly refers to force under such names as virtus, species, imago, agentis, and a score of other names, and this naturally gives rise to the great differences in the interpretations of his writings, with corresponding differences in estimates of them.

The claim that Bacon originated the use of lenses, in the form of spectacles, cannot be proven. Smith has determined that as early as the opening years of the fourteenth century such lenses were in use, but this proves nothing as regards Bacon's connection with their invention. The knowledge of lenses seems to be very ancient, if we may judge from the convex lens of rock

crystal found by Layard in his excavations at Nimrud. There is nothing to show, however, that the ancients ever thought of using them to correct defects of vision. Neither, apparently, is it feasible to determine whether the idea of such an application originated with Bacon.

Another mechanical discovery about which there has been a great deal of discussion is Bacon's supposed invention of gunpowder. It appears that in a certain passage of his work he describes the process of making a substance that is, in effect, ordinary gunpowder; but it is more than doubtful whether he understood the properties of the substance he describes. It is fairly well established, however, that in Bacon's time gunpowder was known to the Arabs, so that it should not be surprising to find references made to it in Bacon's work, since there is reason to believe that he constantly consulted Arabian writings.

The great merit of Bacon's work, however, depends on the principles taught as regards experiment and the observation of nature, rather than on any single invention. He had the all-important idea of breaking with tradition. He championed unfettered inquiry in every field of thought. He had the instinct of a scientific worker--a rare instinct indeed in that age. Nor need we doubt that to the best of his opportunities he was himself an original investigator.

LEONARDO DA VINCI

The relative infertility of Bacon's thought is shown by the fact that he founded no school and left no trace of discipleship. The entire century after his death shows no single European name that need claim the attention of the historian of science. In the latter part of the fifteenth century, however, there is evidence of a renaissance of science no less than of art. The German Muller became famous under the latinized named of Regio Montanus (1437-1472), although his actual scientific attainments would appear to have been important only in comparison with the utter ignorance of his contemporaries. The most distinguished worker of the new era was the famous Italian Leonardo da Vinci--a man who has been called by Hamerton the most universal genius that ever lived. Leonardo's position in the history of art is known to every one. With that, of course, we have no present concern; but it is worth our while to inquire at some length as to the famous painter's accomplishments as a scientist.

From a passage in the works of Leonardo, first brought to light by Venturi,[1] it would seem that the great painter anticipated Copernicus in determining the movement of the earth. He made mathematical calculations to prove this, and appears to have reached the definite conclusion that the earth does move--or what amounts to the same thing, that the sun does not move. Muntz is authority for the statement that in one of his writings he declares, "Il sole non si mouve"--the sun does not move.[2]

Among his inventions is a dynamometer for determining the traction power of machines and animals, and his experiments with steam have led some of his enthusiastic partisans to claim for him priority to Watt in the invention of the steam-engine. In these experiments, however, Leonardo seems to have advanced little beyond Hero of Alexandria and his steam toy. Hero's steam-engine did nothing but rotate itself by virtue of escaping jets of steam forced from the bent tubes, while Leonardo's "steam-engine" "drove a ball weighing one talent over a distance of six stadia." In a manuscript now in the library of the Institut de France, Da Vinci describes this engine minutely. The action of this machine was due to the sudden conversion of small quantities of water into steam ("smoke," as he called it) by coming suddenly in contact with a heated

surface in a proper receptacle, the rapidly formed steam acting as a propulsive force after the manner of an explosive. It is really a steam-gun, rather than a steam-engine, and it is not unlikely that the study of the action of gunpowder may have suggested it to Leonardo.

It is believed that Leonardo is the true discoverer of the camera-obscura, although the Neapolitan philosopher, Giambattista Porta, who was not born until some twenty years after the death of Leonardo, is usually credited with first describing this device. There is little doubt, however, that Da Vinci understood the principle of this mechanism, for he describes how such a camera can be made by cutting a small, round hole through the shutter of a darkened room, the reversed image of objects outside being shown on the opposite wall.

Like other philosophers in all ages, he had observed a great number of facts which he was unable to explain correctly. But such accumulations of scientific observations are always interesting, as showing how many centuries of observation frequently precede correct explanation. He observed many facts about sounds, among others that blows struck upon a bell produced sympathetic sounds in a bell of the same kind; and that striking the string of a lute produced vibration in corresponding strings of lutes strung to the same pitch. He knew, also, that sounds could be heard at a distance at sea by listening at one end of a tube, the other end of which was placed in the water; and that the same expedient worked successfully on land, the end of the tube being placed against the ground.

The knowledge of this great number of unexplained facts is often interpreted by the admirers of Da Vinci, as showing an almost occult insight into science many centuries in advance of his time. Such interpretations, however, are illusive. The observation, for example, that a tube placed against the ground enables one to hear movements on the earth at a distance, is not in itself evidence of anything more than acute scientific observation, as a similar method is in use among almost every race of savages, notably the American Indians. On the other hand, one is inclined to give credence to almost any story of the breadth of knowledge of the man who came so near anticipating Hutton, Lyell, and Darwin in his interpretation of the geological records as he found them written on the rocks.

It is in this field of geology that Leonardo is entitled to the greatest admiration by modern scientists. He had observed the deposit of fossil shells in various strata of rocks, even on the tops of mountains, and he rejected once for all the theory that they had been deposited there by the Deluge. He rightly interpreted their presence as evidence that they had once been deposited at the bottom of the sea. This process he assumed bad taken hundreds and thousands of centuries, thus tacitly rejecting the biblical tradition as to the date of the creation.

Notwithstanding the obvious interest that attaches to the investigations of Leonardo, it must be admitted that his work in science remained almost as infertile as that of his great precursor, Bacon. The really stimulative work of this generation was done by a man of affairs, who knew little of theoretical science except in one line, but who pursued that one practical line until he achieved a wonderful result. This man was Christopher Columbus. It is not necessary here to tell the trite story of his accomplishment. Suffice it that his practical demonstration of the rotundity of the earth is regarded by most modern writers as marking an epoch in history. With the year of his voyage the epoch of the Middle Ages is usually regarded as coming to an end. It must not be supposed that any very sudden change came over the aspect of scholarship of the time, but the preliminaries of great things had been achieved, and when Columbus made his famous voyage in

1492, the man was already alive who was to bring forward the first great vitalizing thought in the field of pure science that the Western world had originated for more than a thousand years. This man bore the name of Kopernik, or in its familiar Anglicized form, Copernicus. His life work and that of his disciples will claim our attention in the succeeding chapter.

IV. THE NEW COSMOLOGY--COPERNICUS TO KEPLER AND GALILEO

We have seen that the Ptolemaic astronomy, which was the accepted doctrine throughout the Middle Ages, taught that the earth is round. Doubtless there was a popular opinion current which regarded the earth as flat, but it must be understood that this opinion had no champions among men of science during the Middle Ages. When, in the year 1492, Columbus sailed out to the west on his memorable voyage, his expectation of reaching India had full scientific warrant, however much it may have been scouted by certain ecclesiastics and by the average man of the period. Nevertheless, we may well suppose that the successful voyage of Columbus, and the still more demonstrative one made about thirty years later by Magellan, gave the theory of the earth's rotundity a certainty it could never previously have had. Alexandrian geographers had measured the size of the earth, and had not hesitated to assert that by sailing westward one might reach India. But there is a wide gap between theory and practice, and it required the voyages of Columbus and his successors to bridge that gap.

After the companions of Magellan completed the circumnavigation of the globe, the general shape of our earth would, obviously, never again be called in question. But demonstration of the sphericity of the earth had, of course, no direct bearing upon the question of the earth's position in the universe. Therefore the voyage of Magellan served to fortify, rather than to dispute, the Ptolemaic theory. According to that theory, as we have seen, the earth was supposed to lie immovable at the centre of the universe; the various heavenly bodies, including the sun, revolving about it in eccentric circles. We have seen that several of the ancient Greeks, notably Aristarchus, disputed this conception, declaring for the central position of the sun in the universe, and the motion of the earth and other planets about that body. But this revolutionary theory seemed so opposed to the ordinary observation that, having been discountenanced by Hipparchus and Ptolemy, it did not find a single important champion for more than a thousand years after the time of the last great Alexandrian astronomer.

The first man, seemingly, to hark back to the Aristarchian conception in the new scientific era that was now dawning was the noted cardinal, Nikolaus of Cusa, who lived in the first half of the fifteenth century, and was distinguished as a philosophical writer and mathematician. His De Docta Ignorantia expressly propounds the doctrine of the earth's motion. No one, however, paid the slightest attention to his suggestion, which, therefore, merely serves to furnish us with another interesting illustration of the futility of propounding even a correct hypothesis before the time is ripe to receive it--particularly if the hypothesis is not fully fortified by reasoning based on experiment or observation.

The man who was destined to put forward the theory of the earth's motion in a way to command attention was born in 1473, at the village of Thorn, in eastern Prussia. His name was Nicholas Copernicus. There is no more famous name in the entire annals of science than this, yet posterity has never been able fully to establish the lineage of the famous expositor of the true doctrine of the solar system. The city of Thorn lies in a province of that border territory which was then under control of Poland, but which subsequently became a part of Prussia. It is claimed that the

aspects of the city were essentially German, and it is admitted that the mother of Copernicus belonged to that race. The nationality of the father is more in doubt, but it is urged that Copernicus used German as his mother-tongue. His great work was, of course, written in Latin, according to the custom of the time; but it is said that, when not employing that language, he always wrote in German. The disputed nationality of Copernicus strongly suggests that he came of a mixed racial lineage, and we are reminded again of the influences of those ethnical minglings to which we have previously more than once referred. The acknowledged centres of civilization towards the close of the fifteenth century were Italy and Spain. Therefore, the birthplace of Copernicus lay almost at the confines of civilization, reminding us of that earlier period when Greece was the centre of culture, but when the great Greek thinkers were born in Asia Minor and in Italy.

As a young man, Copernicus made his way to Vienna to study medicine, and subsequently he journeyed into Italy and remained there many years, About the year 1500 he held the chair of mathematics in a college at Rome. Subsequently he returned to his native land and passed his remaining years there, dying at Domkerr, in Frauenburg, East Prussia, in the year 1543.

It would appear that Copernicus conceived the idea of the heliocentric system of the universe while he was a comparatively young man, since in the introduction to his great work, which he addressed to Pope Paul III., he states that he has pondered his system not merely nine years, in accordance with the maxim of Horace, but well into the fourth period of nine years. Throughout a considerable portion of this period the great work of Copernicus was in manuscript, but it was not published until the year of his death. The reasons for the delay are not very fully established. Copernicus undoubtedly taught his system throughout the later decades of his life. He himself tells us that he had even questioned whether it were not better for him to confine himself to such verbal teaching, following thus the example of Pythagoras. Just as his life was drawing to a close, he decided to pursue the opposite course, and the first copy of his work is said to have been placed in his hands as he lay on his deathbed.

The violent opposition which the new system met from ecclesiastical sources led subsequent commentators to suppose that Copernicus had delayed publication of his work through fear of the church authorities. There seems, however, to be no direct evidence for this opinion. It has been thought significant that Copernicus addressed his work to the pope. It is, of course, quite conceivable that the aged astronomer might wish by this means to demonstrate that he wrote in no spirit of hostility to the church. His address to the pope might have been considered as a desirable shield precisely because the author recognized that his work must needs meet with ecclesiastical criticism. Be that as it may, Copernicus was removed by death from the danger of attack, and it remained for his disciples of a later generation to run the gauntlet of criticism and suffer the charges of heresy.

The work of Copernicus, published thus in the year 1543 at Nuremberg, bears the title De Orbium Coelestium Revolutionibus.

It is not necessary to go into details as to the cosmological system which Copernicus advocated, since it is familiar to every one. In a word, he supposed the sun to be the centre of all the planetary motions, the earth taking its place among the other planets, the list of which, as known at that time, comprised Mercury, Venus, the Earth, Mars, Jupiter, and Saturn. The fixed stars were alleged to be stationary, and it was necessary to suppose that they are almost infinitely

distant, inasmuch as they showed to the observers of that time no parallax; that is to say, they preserved the same apparent position when viewed from the opposite points of the earth's orbit.

But let us allow Copernicus to speak for himself regarding his system, His exposition is full of interest. We quote first the introduction just referred to, in which appeal is made directly to the pope.

"I can well believe, most holy father, that certain people, when they hear of my attributing motion to the earth in these books of mine, will at once declare that such an opinion ought to be rejected. Now, my own theories do not please me so much as not to consider what others may judge of them. Accordingly, when I began to reflect upon what those persons who accept the stability of the earth, as confirmed by the opinion of many centuries, would say when I claimed that the earth moves, I hesitated for a long time as to whether I should publish that which I have written to demonstrate its motion, or whether it would not be better to follow the example of the Pythagoreans, who used to hand down the secrets of philosophy to their relatives and friends only in oral form. As I well considered all this, I was almost impelled to put the finished work wholly aside, through the scorn I had reason to anticipate on account of the newness and apparent contrariness to reason of my theory.

"My friends, however, dissuaded me from such a course and admonished me that I ought to publish my book, which had lain concealed in my possession not only nine years, but already into four times the ninth year. Not a few other distinguished and very learned men asked me to do the same thing, and told me that I ought not, on account of my anxiety, to delay any longer in consecrating my work to the general service of mathematicians.

"But your holiness will perhaps not so much wonder that I have dared to bring the results of my night labors to the light of day, after having taken so much care in elaborating them, but is waiting instead to hear how it entered my mind to imagine that the earth moved, contrary to the accepted opinion of mathematicians--nay, almost contrary to ordinary human understanding. Therefore I will not conceal from your holiness that what moved me to consider another way of reckoning the motions of the heavenly bodies was nothing else than the fact that the mathematicians do not agree with one another in their investigations. In the first place, they are so uncertain about the motions of the sun and moon that they cannot find out the length of a full year. In the second place, they apply neither the same laws of cause and effect, in determining the motions of the sun and moon and of the five planets, nor the same proofs. Some employ only concentric circles, others use eccentric and epicyclic ones, with which, however, they do not fully attain the desired end. They could not even discover nor compute the main thing--namely, the form of the universe and the symmetry of its parts. It was with them as if some should, from different places, take hands, feet, head, and other parts of the body, which, although very beautiful, were not drawn in their proper relations, and, without making them in any way correspond, should construct a monster instead of a human being.

"Accordingly, when I had long reflected on this uncertainty of mathematical tradition, I took the trouble to read again the books of all the philosophers I could get hold of, to see if some one of them had not once believed that there were other motions of the heavenly bodies. First I found in Cicero that Niceties had believed in the motion of the earth. Afterwards I found in Plutarch, likewise, that some others had held the same opinion. This induced me also to begin to consider the movability of the earth, and, although the theory appeared contrary to reason, I did so

because I knew that others before me had been allowed to assume rotary movements at will, in order to explain the phenomena of these celestial bodies. I was of the opinion that I, too, might be permitted to see whether, by presupposing motion in the earth, more reliable conclusions than hitherto reached could not be discovered for the rotary motions of the spheres. And thus, acting on the hypothesis of the motion which, in the following book, I ascribe to the earth, and by long and continued observations, I have finally discovered that if the motion of the other planets be carried over to the relation of the earth and this is made the basis for the rotation of every star, not only will the phenomena of the planets be explained thereby, but also the laws and the size of the stars; all their spheres and the heavens themselves will appear so harmoniously connected that nothing could be changed in any part of them without confusion in the remaining parts and in the whole universe. I do not doubt that clever and learned men will agree with me if they are willing fully to comprehend and to consider the proofs which I advance in the book before us. In order, however, that both the learned and the unlearned may see that I fear no man's judgment, I wanted to dedicate these, my night labors, to your holiness, rather than to any one else, because you, even in this remote corner of the earth where I live, are held to be the greatest in dignity of station and in love for all sciences and for mathematics, so that you, through your position and judgment, can easily suppress the bites of slanderers, although the proverb says that there is no remedy against the bite of calumny."

In chapter X. of book I., "On the Order of the Spheres," occurs a more detailed presentation of the system, as follows:

"That which Martianus Capella, and a few other Latins, very well knew, appears to me extremely noteworthy. He believed that Venus and Mercury revolve about the sun as their centre and that they cannot go farther away from it than the circles of their orbits permit, since they do not revolve about the earth like the other planets. According to this theory, then, Mercury's orbit would be included within that of Venus, which is more than twice as great, and would find room enough within it for its revolution.

"If, acting upon this supposition, we connect Saturn, Jupiter, and Mars with the same centre, keeping in mind the greater extent of their orbits, which include the earth's sphere besides those of Mercury and Venus, we cannot fail to see the explanation of the regular order of their motions. He is certain that Saturn, Jupiter, and Mars are always nearest the earth when they rise in the evening--that is, when they appear over against the sun, or the earth stands between them and the sun--but that they are farthest from the earth when they set in the evening--that is, when we have the sun between them and the earth. This proves sufficiently that their centre belongs to the sun and is the same about which the orbits of Venus and Mercury circle. Since, however, all have one centre, it is necessary for the space intervening between the orbits of Venus and Mars to include the earth with her accompanying moon and all that is beneath the moon; for the moon, which stands unquestionably nearest the earth, can in no way be separated from her, especially as there is sufficient room for the moon in the aforesaid space. Hence we do not hesitate to claim that the whole system, which includes the moon with the earth for its centre, makes the round of that great circle between the planets, in yearly motion about the sun, and revolves about the centre of the universe, in which the sun rests motionless, and that all which looks like motion in the sun is explained by the motion of the earth. The extent of the universe, however, is so great that, whereas the distance of the earth from the sun is considerable in comparison with the size of the other planetary orbits, it disappears when compared with the sphere of the fixed stars. I hold

this to be more easily comprehensible than when the mind is confused by an almost endless number of circles, which is necessarily the case with those who keep the earth in the middle of the universe. Although this may appear incomprehensible and contrary to the opinion of many, I shall, if God wills, make it clearer than the sun, at least to those who are not ignorant of mathematics.

"The order of the spheres is as follows: The first and lightest of all the spheres is that of the fixed stars, which includes itself and all others, and hence is motionless as the place in the universe to which the motion and position of all other stars is referred.

"Then follows the outermost planet, Saturn, which completes its revolution around the sun in thirty years; next comes Jupiter with a twelve years' revolution; then Mars, which completes its course in two years. The fourth one in order is the yearly revolution which includes the earth with the moon's orbit as an epicycle. In the fifth place is Venus with a revolution of nine months. The sixth place is taken by Mercury, which completes its course in eighty days. In the middle of all stands the sun, and who could wish to place the lamp of this most beautiful temple in another or better place. Thus, in fact, the sun, seated upon the royal throne, controls the family of the stars which circle around him. We find in their order a harmonious connection which cannot be found elsewhere. Here the attentive observer can see why the waxing and waning of Jupiter seems greater than with Saturn and smaller than with Mars, and again greater with Venus than with Mercury. Also, why Saturn, Jupiter, and Mars are nearer to the earth when they rise in the evening than when they disappear in the rays of the sun. More prominently, however, is it seen in the case of Mars, which when it appears in the heavens at night, seems to equal Jupiter in size, but soon afterwards is found among the stars of second magnitude. All of this results from the same cause--namely, from the earth's motion. The fact that nothing of this is to be seen in the case of the fixed stars is a proof of their immeasurable distance, which makes even the orbit of yearly motion or its counterpart invisible to us."[1]

The fact that the stars show no parallax had been regarded as an important argument against the motion of the earth, and it was still so considered by the opponents of the system of Copernicus. It had, indeed, been necessary for Aristarchus to explain the fact as due to the extreme distance of the stars; a perfectly correct explanation, but one that implies distances that are altogether inconceivable. It remained for nineteenth-century astronomers to show, with the aid of instruments of greater precision, that certain of the stars have a parallax. But long before this demonstration had been brought forward, the system of Copernicus had been accepted as a part of common knowledge.

While Copernicus postulated a cosmical scheme that was correct as to its main features, he did not altogether break away from certain defects of the Ptolemaic hypothesis. Indeed, he seems to have retained as much of this as practicable, in deference to the prejudice of his time. Thus he records the planetary orbits as circular, and explains their eccentricities by resorting to the theory of epicycles, quite after the Ptolemaic method. But now, of course, a much more simple mechanism sufficed to explain the planetary motions, since the orbits were correctly referred to the central sun and not to the earth.

Needless to say, the revolutionary conception of Copernicus did not meet with immediate acceptance. A number of prominent astronomers, however, took it up almost at once, among these being Rhaeticus, who wrote a commentary on the evolutions; Erasmus Reinhold, the author

of the Prutenic tables; Rothmann, astronomer to the Landgrave of Hesse, and Maestlin, the instructor of Kepler. The Prutenic tables, just referred to, so called because of their Prussian origin, were considered an improvement on the tables of Copernicus, and were highly esteemed by the astronomers of the time. The commentary of Rhaeticus gives us the interesting information that it was the observation of the orbit of Mars and of the very great difference between his apparent diameters at different times which first led Copernicus to conceive the heliocentric idea. Of Reinhold it is recorded that he considered the orbit of Mercury elliptical, and that he advocated a theory of the moon, according to which her epicycle revolved on an elliptical orbit, thus in a measure anticipating one of the great discoveries of Kepler to which we shall refer presently. The Landgrave of Hesse was a practical astronomer, who produced a catalogue of fixed stars which has been compared with that of Tycho Brahe. He was assisted by Rothmann and by Justus Byrgius. Maestlin, the preceptor of Kepler, is reputed to have been the first modern observer to give a correct explanation of the light seen on portions of the moon not directly illumined by the sun. He explained this as not due to any proper light of the moon itself, but as light reflected from the earth. Certain of the Greek philosophers, however, are said to have given the same explanation, and it is alleged also that Leonardo da Vinci anticipated Maestlin in this regard.[2]

While, various astronomers of some eminence thus gave support to the Copernican system, almost from the beginning, it unfortunately chanced that by far the most famous of the immediate successors of Copernicus declined to accept the theory of the earth's motion. This was Tycho Brahe, one of the greatest observing astronomers of any age. Tycho Brahe was a Dane, born at Knudstrup in the year 1546. He died in 1601 at Prague, in Bohemia. During a considerable portion of his life he found a patron in Frederick, King of Denmark, who assisted him to build a splendid observatory on the Island of Huene. On the death of his patron Tycho moved to Germany, where, as good luck would have it, he came in contact with the youthful Kepler, and thus, no doubt, was instrumental in stimulating the ambitions of one who in later years was to be known as a far greater theorist than himself. As has been said, Tycho rejected the Copernican theory of the earth's motion. It should be added, however, that he accepted that part of the Copernican theory which makes the sun the centre of all the planetary motions, the earth being excepted. He thus developed a system of his own, which was in some sort a compromise between the Ptolemaic and the Copernican systems. As Tycho conceived it, the sun revolves about the earth, carrying with it the planets-Mercury, Venus, Mars, Jupiter, and Saturn, which planets have the sun and not the earth as the centre of their orbits. This cosmical scheme, it should be added, may be made to explain the observed motions of the heavenly bodies, but it involves a much more complex mechanism than is postulated by the Copernican theory.

Various explanations have been offered of the conservatism which held the great Danish astronomer back from full acceptance of the relatively simple and, as we now know, correct Copernican doctrine. From our latter-day point of view, it seems so much more natural to accept than to reject the Copernican system, that we find it difficult to put ourselves in the place of a sixteenth-century observer. Yet if we recall that the traditional view, having warrant of acceptance by nearly all thinkers of every age, recorded the earth as a fixed, immovable body, we shall see that our surprise should be excited rather by the thinker who can break away from this view than by the one who still tends to cling to it.

Moreover, it is useless to attempt to disguise the fact that something more than a mere vague

tradition was supposed to support the idea of the earth's overshadowing importance in the cosmical scheme. The sixteenth-century mind was overmastered by the tenets of ecclesiasticism, and it was a dangerous heresy to doubt that the Hebrew writings, upon which ecclesiasticism based its claim, contained the last word regarding matters of science. But the writers of the Hebrew text had been under the influence of that Babylonian conception of the universe which accepted the earth as unqualifiedly central--which, indeed, had never so much as conceived a contradictory hypothesis; and so the Western world, which had come to accept these writings as actually supernatural in origin, lay under the spell of Oriental ideas of a pre-scientific era. In our own day, no one speaking with authority thinks of these Hebrew writings as having any scientific weight whatever. Their interest in this regard is purely antiquarian; hence from our changed point of view it seems scarcely credible that Tycho Brahe can have been in earnest when he quotes the Hebrew traditions as proof that the sun revolves about the earth. Yet we shall see that for almost three centuries after the time of Tycho, these same dreamings continued to be cited in opposition to those scientific advances which new observations made necessary; and this notwithstanding the fact that the Oriental phrasing is, for the most part, poetically ambiguous and susceptible of shifting interpretations, as the criticism of successive generations has amply testified.

As we have said, Tycho Brahe, great observer as he was, could not shake himself free from the Oriental incubus. He began his objections, then, to the Copernican system by quoting the adverse testimony of a Hebrew prophet who lived more than a thousand years B.C. All of this shows sufficiently that Tycho Brahe was not a great theorist. He was essentially an observer, but in this regard he won a secure place in the very first rank. Indeed, he was easily the greatest observing astronomer since Hipparchus, between whom and himself there were many points of resemblance. Hipparchus, it will be recalled, rejected the Aristarchian conception of the universe just as Tycho rejected the conception of Copernicus.

But if Tycho propounded no great generalizations, the list of specific advances due to him is a long one, and some of these were to prove important aids in the hands of later workers to the secure demonstration of the Copernican idea. One of his most important series of studies had to do with comets. Regarding these bodies there had been the greatest uncertainty in the minds of astronomers. The greatest variety of opinions regarding them prevailed; they were thought on the one hand to be divine messengers, and on the other to be merely igneous phenomena of the earth's atmosphere. Tycho Brahe declared that a comet which he observed in the year 1577 had no parallax, proving its extreme distance. The observed course of the comet intersected the planetary orbits, which fact gave a quietus to the long-mooted question as to whether the Ptolemaic spheres were transparent solids or merely imaginary; since the comet was seen to intersect these alleged spheres, it was obvious that they could not be the solid substance that they were commonly imagined to be, and this fact in itself went far towards discrediting the Ptolemaic system. It should be recalled, however, that this supposition of tangible spheres for the various planetary and stellar orbits was a mediaeval interpretation of Ptolemy's theory rather than an interpretation of Ptolemy himself, there being nothing to show that the Alexandrian astronomer regarded his cycles and epicycles as other than theoretical.

An interesting practical discovery made by Tycho was his method of determining the latitude of a place by means of two observations made at an interval of twelve hours. Hitherto it had been necessary to observe the sun's angle on the equinoctial days, a period of six months being

therefore required. Tycho measured the angle of elevation of some star situated near the pole, when on the meridian, and then, twelve hours later, measured the angle of elevation of the same star when it again came to the meridian at the opposite point of its apparent circle about the polestar. Half the sum of these angles gives the latitude of the place of observation.

As illustrating the accuracy of Tycho's observations, it may be noted that he rediscovered a third inequality of the moon's motion at its variation, he, in common with other European astronomers, being then quite unaware that this inequality had been observed by an Arabian astronomer. Tycho proved also that the angle of inclination of the moon's orbit to the ecliptic is subject to slight variation.

The very brilliant new star which shone forth suddenly in the constellation of Cassiopeia in the year 1572, was made the object of special studies by Tycho, who proved that the star had no sensible parallax and consequently was far beyond the planetary regions. The appearance of a new star was a phenomenon not unknown to the ancients, since Pliny records that Hipparchus was led by such an appearance to make his catalogue of the fixed stars. But the phenomenon is sufficiently uncommon to attract unusual attention. A similar phenomenon occurred in the year 1604, when the new star--in this case appearing in the constellation of Serpentarius--was explained by Kepler as probably proceeding from a vast combustion. This explanation--in which Kepler is said to have followed. Tycho--is fully in accord with the most recent theories on the subject, as we shall see in due course. It is surprising to hear Tycho credited with so startling a theory, but, on the other hand, such an explanation is precisely what should be expected from the other astronomer named. For Johann Kepler, or, as he was originally named, Johann von Kappel, was one of the most speculative astronomers of any age. He was forever theorizing, but such was the peculiar quality of his mind that his theories never satisfied him for long unless he could put them to the test of observation. Thanks to this happy combination of qualities, Kepler became the discoverer of three famous laws of planetary motion which lie at the very foundation of modern astronomy, and which were to be largely instrumental in guiding Newton to his still greater generalization. These laws of planetary motion were vastly important as corroborating the Copernican theory of the universe, though their position in this regard was not immediately recognized by contemporary thinkers. Let us examine with some detail into their discovery, meantime catching a glimpse of the life history of the remarkable man whose name they bear.

JOHANN KEPLER AND THE LAWS OF PLANETARY MOTION

Johann Kepler was born the 27th of December, 1571, in the little town of Weil, in Wurtemburg. He was a weak, sickly child, further enfeebled by a severe attack of small-pox. It would seem paradoxical to assert that the parents of such a genius were mismated, but their home was not a happy one, the mother being of a nervous temperament, which perhaps in some measure accounted for the genius of the child. The father led the life of a soldier, and finally perished in the campaign against the Turks. Young Kepler's studies were directed with an eye to the ministry. After a preliminary training he attended the university at Tubingen, where he came under the influence of the celebrated Maestlin and became his life-long friend.

Curiously enough, it is recorded that at first Kepler had no taste for astronomy or for mathematics. But the doors of the ministry being presently barred to him, he turned with enthusiasm to the study of astronomy, being from the first an ardent advocate of the Copernican system. His teacher, Maestlin, accepted the same doctrine, though he was obliged, for theological

reasons, to teach the Ptolemaic system, as also to oppose the Gregorian reform of the calendar.

The Gregorian calendar, it should be explained, is so called because it was instituted by Pope Gregory XIII., who put it into effect in the year 1582, up to which time the so-called Julian calendar, as introduced by Julius Caesar, had been everywhere accepted in Christendom. This Julian calendar, as we have seen, was a great improvement on preceding ones, but still lacked something of perfection inasmuch as its theoretical day differed appreciably from the actual day. In the course of fifteen hundred years, since the time of Caesar, this defect amounted to a discrepancy of about eleven days. Pope Gregory proposed to correct this by omitting ten days from the calendar, which was done in September, 1582. To prevent similar inaccuracies in the future, the Gregorian calendar provided that once in four centuries the additional day to make a leap-year should be omitted, the date selected for such omission being the last year of every fourth century. Thus the years 1500, 1900, and 2300, A.D., would not be leap-years. By this arrangement an approximate rectification of the calendar was effected, though even this does not make it absolutely exact.

Such a rectification as this was obviously desirable, but there was really no necessity for the omission of the ten days from the calendar. The equinoctial day had shifted so that in the year 1582 it fell on the 10th of March and September. There was no reason why it should not have remained there. It would greatly have simplified the task of future historians had Gregory contented himself with providing for the future stability of the calendar without making the needless shift in question. We are so accustomed to think of the 21st of March and 21st of September as the natural periods of the equinox, that we are likely to forget that these are purely arbitrary dates for which the 10th might have been substituted without any inconvenience or inconsistency.

But the opposition to the new calendar, to which reference has been made, was not based on any such considerations as these. It was due, largely at any rate, to the fact that Germany at this time was under sway of the Lutheran revolt against the papacy. So effective was the opposition that the Gregorian calendar did not come into vogue in Germany until the year 1699. It may be added that England, under stress of the same manner of prejudice, held out against the new reckoning until the year 1751, while Russia does not accept it even now.

As the Protestant leaders thus opposed the papal attitude in a matter of so practical a character as the calendar, it might perhaps have been expected that the Lutherans would have had a leaning towards the Copernican theory of the universe, since this theory was opposed by the papacy. Such, however, was not the case. Luther himself pointed out with great strenuousness, as a final and demonstrative argument, the fact that Joshua commanded the sun and not the earth to stand still; and his followers were quite as intolerant towards the new teaching as were their ultramontane opponents. Kepler himself was, at various times, to feel the restraint of ecclesiastical opposition, though he was never subjected to direct persecution, as was his friend and contemporary, Galileo. At the very outset of Kepler's career there was, indeed, question as to the publication of a work he had written, because that work took for granted the truth of the Copernican doctrine. This work appeared, however, in the year 1596. It bore the title Mysterium Cosmographium, and it attempted to explain the positions of the various planetary bodies. Copernicus had devoted much time to observation of the planets with reference to measuring their distance, and his efforts had been attended with considerable success. He did not, indeed,

know the actual distance of the sun, and, therefore, was quite unable to fix the distance of any planet; but, on the other hand, he determined the relative distance of all the planets then known, as measured in terms of the sun's distance, with remarkable accuracy.

With these measurements as a guide, Kepler was led to a very fanciful theory, according to which the orbits of the five principal planets sustain a peculiar relation to the five regular solids of geometry. His theory was this: "Around the orbit of the earth describe a dodecahedron--the circle comprising it will be that of Mars; around Mars describe a tetrahedron--the circle comprising it will be that of Jupiter; around Jupiter describe a cube--the circle comprising it will be that of Saturn; now within the earth's orbit inscribe an icosahedron--the inscribed circle will be that of Venus; in the orbit of Venus inscribe an octahedron --the circle inscribed will be that of Mercury."[3]

Though this arrangement was a fanciful one, which no one would now recall had not the theorizer obtained subsequent fame on more substantial grounds, yet it evidenced a philosophical spirit on the part of the astronomer which, misdirected as it was in this instance, promised well for the future. Tycho Brahe, to whom a copy of the work was sent, had the acumen to recognize it as a work of genius. He summoned the young astronomer to be his assistant at Prague, and no doubt the association thus begun was instrumental in determining the character of Kepler's future work. It was precisely the training in minute observation that could avail most for a mind which, like Kepler's, tended instinctively to the formulation of theories. When Tycho Brahe died, in 1601, Kepler became his successor. In due time he secured access to all the unpublished observations of his great predecessor, and these were of inestimable value to him in the progress of his own studies.

Kepler was not only an ardent worker and an enthusiastic theorizer, but he was an indefatigable writer, and it pleased him to take the public fully into his confidence, not merely as to his successes, but as to his failures. Thus his works elaborate false theories as well as correct ones, and detail the observations through which the incorrect guesses were refuted by their originator. Some of these accounts are highly interesting, but they must not detain us here. For our present purpose it must suffice to point out the three important theories, which, as culled from among a score or so of incorrect ones, Kepler was able to demonstrate to his own satisfaction and to that of subsequent observers. Stated in a few words, these theories, which have come to bear the name of Kepler's Laws, are the following:

1. That the planetary orbits are not circular, but elliptical, the sun occupying one focus of the ellipses.

2. That the speed of planetary motion varies in different parts of the orbit in such a way that an imaginary line drawn from the sun to the planet--that is to say, the radius vector of the planet's orbit--always sweeps the same area in a given time.

These two laws Kepler published as early as 1609. Many years more of patient investigation were required before he found out the secret of the relation between planetary distances and times of revolution which his third law expresses. In 1618, however, he was able to formulate this relation also, as follows:

3. The squares of the distance of the various planets from the sun are proportional to the cubes of

their periods of revolution about the sun.

All these laws, it will be observed, take for granted the fact that the sun is the centre of the planetary orbits. It must be understood, too, that the earth is constantly regarded, in accordance with the Copernican system, as being itself a member of the planetary system, subject to precisely the same laws as the other planets. Long familiarity has made these wonderful laws of Kepler seem such a matter of course that it is difficult now to appreciate them at their full value. Yet, as has been already pointed out, it was the knowledge of these marvellously simple relations between the planetary orbits that laid the foundation for the Newtonian law of universal gravitation. Contemporary judgment could not, of course, anticipate this culmination of a later generation. What it could understand was that the first law of Kepler attacked one of the most time-honored of metaphysical conceptions--namely, the Aristotelian idea that the circle is the perfect figure, and hence that the planetary orbits must be circular. Not even Copernicus had doubted the validity of this assumption. That Kepler dared dispute so firmly fixed a belief, and one that seemingly had so sound a philosophical basis, evidenced the iconoclastic nature of his genius. That he did not rest content until he had demonstrated the validity of his revolutionary assumption shows how truly this great theorizer made his hypotheses subservient to the most rigid inductions.

GALILEO GALILEI

While Kepler was solving these riddles of planetary motion, there was an even more famous man in Italy whose championship of the Copernican doctrine was destined to give the greatest possible publicity to the new ideas. This was Galileo Galilei, one of the most extraordinary scientific observers of any age. Galileo was born at Pisa, on the 18th of February (old style), 1564. The day of his birth is doubly memorable, since on the same day the greatest Italian of the preceding epoch, Michael Angelo, breathed his last. Persons fond of symbolism have found in the coincidence a forecast of the transit from the artistic to the scientific epoch of the later Renaissance. Galileo came of an impoverished noble family. He was educated for the profession of medicine, but did not progress far before his natural proclivities directed him towards the physical sciences. Meeting with opposition in Pisa, he early accepted a call to the chair of natural philosophy in the University of Padua, and later in life he made his home at Florence. The mechanical and physical discoveries of Galileo will claim our attention in another chapter. Our present concern is with his contribution to the Copernican theory.

Galileo himself records in a letter to Kepler that he became a convert to this theory at an early day. He was not enabled, however, to make any marked contribution to the subject, beyond the influence of his general teachings, until about the year 1610. The brilliant contributions which he made were due largely to a single discovery--namely, that of the telescope. Hitherto the astronomical observations had been made with the unaided eye. Glass lenses had been known since the thirteenth century, but, until now, no one had thought of their possible use as aids to distant vision. The question of priority of discovery has never been settled. It is admitted, however, that the chief honors belong to the opticians of the Netherlands.

As early as the year 1590 the Dutch optician Zacharias Jensen placed a concave and a convex lens respectively at the ends of a tube about eighteen inches long, and used this instrument for the purpose of magnifying small objects--producing, in short, a crude microscope. Some years later, Johannes Lippershey, of whom not much is known except that he died in 1619,

experimented with a somewhat similar combination of lenses, and made the startling observation that the weather-vane on a distant church-steeple seemed to be brought much nearer when viewed through the lens. The combination of lenses he employed is that still used in the construction of opera-glasses; the Germans still call such a combination a Dutch telescope.

Doubtless a large number of experimenters took the matter up and the fame of the new instrument spread rapidly abroad. Galileo, down in Italy, heard rumors of this remarkable contrivance, through the use of which it was said "distant objects might be seen as clearly as those near at hand." He at once set to work to construct for himself a similar instrument, and his efforts were so far successful that at first he "saw objects three times as near and nine times enlarged." Continuing his efforts, he presently so improved his glass that objects were enlarged almost a thousand times and made to appear thirty times nearer than when seen with the naked eye. Naturally enough, Galileo turned this fascinating instrument towards the skies, and he was almost immediately rewarded by several startling discoveries. At the very outset, his magnifying-glass brought to view a vast number of stars that are invisible to the naked eye, and enabled the observer to reach the conclusion that the hazy light of the Milky Way is merely due to the aggregation of a vast number of tiny stars.

Turning his telescope towards the moon, Galileo found that body rough and earth-like in contour, its surface covered with mountains, whose height could be approximately measured through study of their shadows. This was disquieting, because the current Aristotelian doctrine supposed the moon, in common with the planets, to be a perfectly spherical, smooth body. The metaphysical idea of a perfect universe was sure to be disturbed by this seemingly rough workmanship of the moon. Thus far, however, there was nothing in the observations of Galileo to bear directly upon the Copernican theory; but when an inspection was made of the planets the case was quite different. With the aid of his telescope, Galileo saw that Venus, for example, passes through phases precisely similar to those of the moon, due, of course, to the same cause. Here, then, was demonstrative evidence that the planets are dark bodies reflecting the light of the sun, and an explanation was given of the fact, hitherto urged in opposition to the Copernican theory, that the inferior planets do not seem many times brighter when nearer the earth than when in the most distant parts of their orbits; the explanation being, of course, that when the planets are between the earth and the sun only a small portion of their illumined surfaces is visible from the earth.

On inspecting the planet Jupiter, a still more striking revelation was made, as four tiny stars were observed to occupy an equatorial position near that planet, and were seen, when watched night after night, to be circling about the planet, precisely as the moon circles about the earth. Here, obviously, was a miniature solar system--a tangible object-lesson in the Copernican theory. In honor of the ruling Florentine house of the period, Galileo named these moons of Jupiter, Medicean stars.

Turning attention to the sun itself, Galileo observed on the surface of that luminary a spot or blemish which gradually changed its shape, suggesting that changes were taking place in the substance of the sun--changes obviously incompatible with the perfect condition demanded by the metaphysical theorists. But however disquieting for the conservative, the sun's spots served a most useful purpose in enabling Galileo to demonstrate that the sun itself revolves on its axis, since a given spot was seen to pass across the disk and after disappearing to reappear in due

course. The period of rotation was found to be about twenty-four days.

It must be added that various observers disputed priority of discovery of the sun's spots with Galileo. Unquestionably a sun-spot had been seen by earlier observers, and by them mistaken for the transit of an inferior planet. Kepler himself had made this mistake. Before the day of the telescope, he had viewed the image of the sun as thrown on a screen in a camera-obscura, and had observed a spot on the disk which be interpreted as representing the planet Mercury, but which, as is now known, must have been a sun-spot, since the planetary disk is too small to have been revealed by this method. Such observations as these, however interesting, cannot be claimed as discoveries of the sun-spots. It is probable, however, that several discoverers (notably Johann Fabricius) made the telescopic observation of the spots, and recognized them as having to do with the sun's surface, almost simultaneously with Galileo. One of these claimants was a Jesuit named Scheiner, and the jealousy of this man is said to have had a share in bringing about that persecution to which we must now refer.

There is no more famous incident in the history of science than the heresy trial through which Galileo was led to the nominal renunciation of his cherished doctrines. There is scarcely another incident that has been commented upon so variously. Each succeeding generation has put its own interpretation on it. The facts, however, have been but little questioned. It appears that in the year 1616 the church became at last aroused to the implications of the heliocentric doctrine of the universe. Apparently it seemed clear to the church authorities that the authors of the Bible believed the world to be immovably fixed at the centre of the universe. Such, indeed, would seem to be the natural inference from various familiar phrases of the Hebrew text, and what we now know of the status of Oriental science in antiquity gives full warrant to this interpretation. There is no reason to suppose that the conception of the subordinate place of the world in the solar system had ever so much as occurred, even as a vague speculation, to the authors of Genesis. In common with their contemporaries, they believed the earth to be the all-important body in the universe, and the sun a luminary placed in the sky for the sole purpose of giving light to the earth. There is nothing strange, nothing anomalous, in this view; it merely reflects the current notions of Oriental peoples in antiquity. What is strange and anomalous is the fact that the Oriental dreamings thus expressed could have been supposed to represent the acme of scientific knowledge. Yet such a hold had these writings taken upon the Western world that not even a Galileo dared contradict them openly; and when the church fathers gravely declared the heliocentric theory necessarily false, because contradictory to Scripture, there were probably few people in Christendom whose mental attitude would permit them justly to appreciate the humor of such a pronouncement. And, indeed, if here and there a man might have risen to such an appreciation, there were abundant reasons for the repression of the impulse, for there was nothing humorous about the response with which the authorities of the time were wont to meet the expression of iconoclastic opinions. The burning at the stake of Giordano Bruno, in the year 1600, was, for example, an object-lesson well calculated to restrain the enthusiasm of other similarly minded teachers.

Doubtless it was such considerations that explained the relative silence of the champions of the Copernican theory, accounting for the otherwise inexplicable fact that about eighty years elapsed after the death of Copernicus himself before a single text-book expounded his theory. The text-book which then appeared, under date of 1622, was written by the famous Kepler, who perhaps was shielded in a measure from the papal consequences of such hardihood by the fact of

residence in a Protestant country. Not that the Protestants of the time favored the heliocentric doctrine--we have already quoted Luther in an adverse sense--but of course it was characteristic of the Reformation temper to oppose any papal pronouncement, hence the ultramontane declaration of 1616 may indirectly have aided the doctrine which it attacked, by making that doctrine less obnoxious to Lutheran eyes. Be that as it may, the work of Kepler brought its author into no direct conflict with the authorities. But the result was quite different when, in 1632, Galileo at last broke silence and gave the world, under cover of the form of dialogue, an elaborate exposition of the Copernican theory. Galileo, it must be explained, had previously been warned to keep silent on the subject, hence his publication doubly offended the authorities. To be sure, he could reply that his dialogue introduced a champion of the Ptolemaic system to dispute with the upholder of the opposite view, and that, both views being presented with full array of argument, the reader was left to reach a verdict for himself, the author having nowhere pointedly expressed an opinion. But such an argument, of course, was specious, for no one who read the dialogue could be in doubt as to the opinion of the author. Moreover, it was hinted that Simplicio, the character who upheld the Ptolemaic doctrine and who was everywhere worsted in the argument, was intended to represent the pope himself--a suggestion which probably did no good to Galileo's cause.

The character of Galileo's artistic presentation may best be judged from an example, illustrating the vigorous assault of Salviati, the champion of the new theory, and the feeble retorts of his conservative antagonist:

"Salviati. Let us then begin our discussion with the consideration that, whatever motion may be attributed to the earth, yet we, as dwellers upon it, and hence as participators in its motion, cannot possibly perceive anything of it, presupposing that we are to consider only earthly things. On the other hand, it is just as necessary that this same motion belong apparently to all other bodies and visible objects, which, being separated from the earth, do not take part in its motion. The correct method to discover whether one can ascribe motion to the earth, and what kind of motion, is, therefore, to investigate and observe whether in bodies outside the earth a perceptible motion may be discovered which belongs to all alike. Because a movement which is perceptible only in the moon, for instance, and has nothing to do with Venus or Jupiter or other stars, cannot possibly be peculiar to the earth, nor can its seat be anywhere else than in the moon. Now there is one such universal movement which controls all others--namely, that which the sun, moon, the other planets, the fixed stars--in short, the whole universe, with the single exception of the earth--appears to execute from east to west in the space of twenty-four hours. This now, as it appears at the first glance anyway, might just as well be a motion of the earth alone as of all the rest of the universe with the exception of the earth, for the same phenomena would result from either hypothesis. Beginning with the most general, I will enumerate the reasons which seem to speak in favor of the earth's motion. When we merely consider the immensity of the starry sphere in comparison with the smallness of the terrestrial ball, which is contained many million times in the former, and then think of the rapidity of the motion which completes a whole rotation in one day and night, I cannot persuade myself how any one can hold it to be more reasonable and credible that it is the heavenly sphere which rotates, while the earth stands still.

"Simplicio. I do not well understand how that powerful motion may be said to as good as not exist for the sun, the moon, the other planets, and the innumerable host of fixed stars. Do you call that nothing when the sun goes from one meridian to another, rises up over this horizon and sinks

behind that one, brings now day, and now night; when the moon goes through similar changes, and the other planets and fixed stars in the same way?

"Salviati. All the changes you mention are such only in respect to the earth. To convince yourself of it, only imagine the earth out of existence. There would then be no rising and setting of the sun or of the moon, no horizon, no meridian, no day, no night--in short, the said motion causes no change of any sort in the relation of the sun to the moon or to any of the other heavenly bodies, be they planets or fixed stars. All changes are rather in respect to the earth; they may all be reduced to the simple fact that the sun is first visible in China, then in Persia, afterwards in Egypt, Greece, France, Spain, America, etc., and that the same thing happens with the moon and the other heavenly bodies. Exactly the same thing happens and in exactly the same way if, instead of disturbing so large a part of the universe, you let the earth revolve about itself. The difficulty is, however, doubled, inasmuch as a second very important problem presents itself. If, namely, that powerful motion is ascribed to the heavens, it is absolutely necessary to regard it as opposed to the individual motion of all the planets, every one of which indubitably has its own very leisurely and moderate movement from west to east. If, on the other hand, you let the earth move about itself, this opposition of motion disappears.

"The improbability is tripled by the complete overthrow of that order which rules all the heavenly bodies in which the revolving motion is definitely established. The greater the sphere is in such a case, so much longer is the time required for its revolution; the smaller the sphere the shorter the time. Saturn, whose orbit surpasses those of all the planets in size, traverses it in thirty years. Jupiter[4] completes its smaller course in twelve years, Mars in two; the moon performs its much smaller revolution within a month. Just as clearly in the Medicean stars, we see that the one nearest Jupiter completes its revolution in a very short time--about forty-two hours; the next in about three and one-half days, the third in seven, and the most distant one in sixteen days. This rule, which is followed throughout, will still remain if we ascribe the twenty-four-hourly motion to a rotation of the earth. If, however, the earth is left motionless, we must go first from the very short rule of the moon to ever greater ones--to the two-yearly rule of Mars, from that to the twelve-yearly one of Jupiter, from here to the thirty-yearly one of Saturn, and then suddenly to an incomparably greater sphere, to which also we must ascribe a complete rotation in twenty-four hours. If, however, we assume a motion of the earth, the rapidity of the periods is very well preserved; from the slowest sphere of Saturn we come to the wholly motionless fixed stars. We also escape thereby a fourth difficulty, which arises as soon as we assume that there is motion in the sphere of the stars. I mean the great unevenness in the movement of these very stars, some of which would have to revolve with extraordinary rapidity in immense circles, while others moved very slowly in small circles, since some of them are at a greater, others at a less, distance from the pole. That is likewise an inconvenience, for, on the one hand, we see all those stars, the motion of which is indubitable, revolve in great circles, while, on the other hand, there seems to be little object in placing bodies, which are to move in circles, at an enormous distance from the centre and then let them move in very small circles. And not only are the size of the different circles and therewith the rapidity of the movement very different in the different fixed stars, but the same stars also change their orbits and their rapidity of motion. Therein consists the fifth inconvenience. Those stars, namely, which were at the equator two thousand years ago, and hence described great circles in their revolutions, must to-day move more slowly and in smaller circles, because they are many degrees removed from it. It will even happen, after not so very long a time, that one of those which have hitherto been continually in motion will finally

coincide with the pole and stand still, but after a period of repose will again begin to move. The other stars in the mean while, which unquestionably move, all have, as was said, a great circle for an orbit and keep this unchangeably.

"The improbability is further increased--this may be considered the sixth inconvenience--by the fact that it is impossible to conceive what degree of solidity those immense spheres must have, in the depths of which so many stars are fixed so enduringly that they are kept revolving evenly in spite of such difference of motion without changing their respective positions. Or if, according to the much more probable theory, the heavens are fluid, and every star describes an orbit of its own, according to what law then, or for what reason, are their orbits so arranged that, when looked at from the earth, they appear to be contained in one single sphere? To attain this it seems to me much easier and more convenient to make them motionless instead of moving, just as the paving-stones on the market-place, for instance, remain in order more easily than the swarms of children running about on them.

"Finally, the seventh difficulty: If we attribute the daily rotation to the higher region of the heavens, we should have to endow it with force and power sufficient to carry with it the innumerable host of the fixed stars --every one a body of very great compass and much larger than the earth--and all the planets, although the latter, like the earth, move naturally in an opposite direction. In the midst of all this the little earth, single and alone, would obstinately and wilfully withstand such force--a supposition which, it appears to me, has much against it. I could also not explain why the earth, a freely poised body, balancing itself about its centre, and surrounded on all sides by a fluid medium, should not be affected by the universal rotation. Such difficulties, however, do not confront us if we attribute motion to the earth--such a small, insignificant body in comparison with the whole universe, and which for that very reason cannot exercise any power over the latter.

"Simplicio. You support your arguments throughout, it seems to me, on the greater ease and simplicity with which the said effects are produced. You mean that as a cause the motion of the earth alone is just as satisfactory as the motion of all the rest of the universe with the exception of the earth; you hold the actual event to be much easier in the former case than in the latter. For the ruler of the universe, however, whose might is infinite, it is no less easy to move the universe than the earth or a straw balm. But if his power is infinite, why should not a greater, rather than a very small, part of it be revealed to me?

"Salviati. If I had said that the universe does not move on account of the impotence of its ruler, I should have been wrong and your rebuke would have been in order. I admit that it is just as easy for an infinite power to move a hundred thousand as to move one. What I said, however, does not refer to him who causes the motion, but to that which is moved. In answer to your remark that it is more fitting for an infinite power to reveal a large part of itself rather than a little, I answer that, in relation to the infinite, one part is not greater than another, if both are finite. Hence it is unallowable to say that a hundred thousand is a larger part of an infinite number than two, although the former is fifty thousand times greater than the latter. If, therefore, we consider the moving bodies, we must unquestionably regard the motion of the earth as a much simpler process than that of the universe; if, furthermore, we direct our attention to so many other simplifications which may be reached only by this theory, the daily movement of the earth must appear much more probable than the motion of the universe without the earth, for, according to

Aristotle's just axiom, 'Frustra fit per plura, quod potest fieri per p auciora' (It is vain to expend many means where a few are sufficient)."[2]

The work was widely circulated, and it was received with an interest which bespeaks a wide-spread undercurrent of belief in the Copernican doctrine. Naturally enough, it attracted immediate attention from the church authorities. Galileo was summoned to appear at Rome to defend his conduct. The philosopher, who was now in his seventieth year, pleaded age and infirmity. He had no desire for personal experience of the tribunal of the Inquisition; but the mandate was repeated, and Galileo went to Rome. There, as every one knows, he disavowed any intention to oppose the teachings of Scripture, and formally renounced the heretical doctrine of the earth's motion. According to a tale which so long passed current that every historian must still repeat it though no one now believes it authentic, Galileo qualified his renunciation by muttering to himself, "E pur si muove" (It does move, none the less), as he rose to his feet and retired from the presence of his persecutors. The tale is one of those fictions which the dramatic sense of humanity is wont to impose upon history, but, like most such fictions, it expresses the spirit if not the letter of truth; for just as no one believes that Galileo's lips uttered the phrase, so no one doubts that the rebellious words were in his mind.

After his formal renunciation, Galileo was allowed to depart, but with the injunction that he abstain in future from heretical teaching. The remaining ten years of his life were devoted chiefly to mechanics, where his experiments fortunately opposed the Aristotelian rather than the Hebrew teachings. Galileo's death occurred in 1642, a hundred years after the death of Copernicus. Kepler had died thirteen years before, and there remained no astronomer in the field who is conspicuous in the history of science as a champion of the Copernican doctrine. But in truth it might be said that the theory no longer needed a champion. The researches of Kepler and Galileo had produced a mass of evidence for the Copernican theory which amounted to demonstration. A generation or two might be required for this evidence to make itself everywhere known among men of science, and of course the ecclesiastical authorities must be expected to stand by their guns for a somewhat longer period. In point of fact, the ecclesiastical ban was not technically removed by the striking of the Copernican books from the list of the Index Expurgatorius until the year 1822, almost two hundred years after the date of Galileo's dialogue. But this, of course, is in no sense a guide to the state of general opinion regarding the theory. We shall gain a true gauge as to this if we assume that the greater number of important thinkers had accepted the heliocentric doctrine before the middle of the seventeenth century, and that before the close of that century the old Ptolemaic idea had been quite abandoned. A wonderful revolution in man's estimate of the universe had thus been effected within about two centuries after the birth of Copernicus.

V. GALILEO AND THE NEW PHYSICS

After Galileo had felt the strong hand of the Inquisition, in 1632, he was careful to confine his researches, or at least his publications, to topics that seemed free from theological implications. In doing so he reverted to the field of his earliest studies --namely, the field of mechanics; and the Dialoghi delle Nuove Scienze, which he finished in 1636, and which was printed two years later, attained a celebrity no less than that of the heretical dialogue that had preceded it. The later work was free from all apparent heresies, yet perhaps it did more towards the establishment of the Copernican doctrine, through the teaching of correct mechanical principles, than the other

work had accomplished by a more direct method.

Galileo's astronomical discoveries were, as we have seen, in a sense accidental; at least, they received their inception through the inventive genius of another. His mechanical discoveries, on the other hand, were the natural output of his own creative genius. At the very beginning of his career, while yet a very young man, though a professor of mathematics at Pisa, he had begun that onslaught upon the old Aristotelian ideas which he was to continue throughout his life. At the famous leaning tower in Pisa, the young iconoclast performed, in the year 1590, one of the most theatrical demonstrations in the history of science. Assembling a multitude of champions of the old ideas, he proposed to demonstrate the falsity of the Aristotelian doctrine that the velocity of falling bodies is proportionate to their weight. There is perhaps no fact more strongly illustrative of the temper of the Middle Ages than the fact that this doctrine, as taught by the Aristotelian philosopher, should so long have gone unchallenged. Now, however, it was put to the test; Galileo released a half-pound weight and a hundred-pound cannon-ball from near the top of the tower, and, needless to say, they reached the ground together. Of course, the spectators were but little pleased with what they saw. They could not doubt the evidence of their own senses as to the particular experiment in question; they could suggest, however, that the experiment involved a violation of the laws of nature through the practice of magic. To controvert so firmly established an idea savored of heresy. The young man guilty of such iconoclasm was naturally looked at askance by the scholarship of his time. Instead of being applauded, he was hissed, and he found it expedient presently to retire from Pisa.

Fortunately, however, the new spirit of progress had made itself felt more effectively in some other portions of Italy, and so Galileo found a refuge and a following in Padua, and afterwards in Florence; and while, as we have seen, he was obliged to curb his enthusiasm regarding the subject that was perhaps nearest his heart--the promulgation of the Copernican theory--yet he was permitted in the main to carry on his experimental observations unrestrained. These experiments gave him a place of unquestioned authority among his contemporaries, and they have transmitted his name to posterity as that of one of the greatest of experimenters and the virtual founder of modern mechanical science. The experiments in question range over a wide field; but for the most part they have to do with moving bodies and with questions of force, or, as we should now say, of energy. The experiment at the leaning tower showed that the velocity of falling bodies is independent of the weight of the bodies, provided the weight is sufficient to overcome the resistance of the atmosphere. Later experiments with falling bodies led to the discovery of laws regarding the accelerated velocity of fall. Such velocities were found to bear a simple relation to the period of time from the beginning of the fall. Other experiments, in which balls were allowed to roll down inclined planes, corroborated the observation that the pull of gravitation gave a velocity proportionate to the length of fall, whether such fall were direct or in a slanting direction.

These studies were associated with observations on projectiles, regarding which Galileo was the first to entertain correct notions. According to the current idea, a projectile fired, for example, from a cannon, moved in a straight horizontal line until the propulsive force was exhausted, and then fell to the ground in a perpendicular line. Galileo taught that the projectile begins to fall at once on leaving the mouth of the cannon and traverses a parabolic course. According to his idea, which is now familiar to every one, a cannon-ball dropped from the level of the cannon's muzzle will strike the ground simultaneously with a ball fired horizontally from the cannon. As to the

paraboloid course pursued by the projectile, the resistance of the air is a factor which Galileo could not accurately compute, and which interferes with the practical realization of his theory. But this is a minor consideration. The great importance of his idea consists in the recognition that such a force as that of gravitation acts in precisely the same way upon all unsupported bodies, whether or not such bodies be at the same time acted upon by a force of translation.

Out of these studies of moving bodies was gradually developed a correct notion of several important general laws of mechanics--laws a knowledge of which was absolutely essential to the progress of physical science. The belief in the rotation of the earth made necessary a clear conception that all bodies at the surface of the earth partake of that motion quite independently of their various observed motions in relation to one another. This idea was hard to grasp, as an oft-repeated argument shows. It was asserted again and again that, if the earth rotates, a stone dropped from the top of a tower could not fall at the foot of the tower, since the earth's motion would sweep the tower far away from its original position while the stone is in transit.

This was one of the stock arguments against the earth's motion, yet it was one that could be refuted with the greatest ease by reasoning from strictly analogous experiments. It might readily be observed, for example, that a stone dropped from a moving cart does not strike the ground directly below the point from which it is dropped, but partakes of the forward motion of the cart. If any one doubt this he has but to jump from a moving cart to be given a practical demonstration of the fact that his entire body was in some way influenced by the motion of translation. Similarly, the simple experiment of tossing a ball from the deck of a moving ship will convince any one that the ball partakes of the motion of the ship, so that it can be manipulated precisely as if the manipulator were standing on the earth. In short, every-day experience gives us illustrations of what might be called compound motion, which makes it seem altogether plausible that, if the earth is in motion, objects at its surface will partake of that motion in a way that does not interfere with any other movements to which they may be subjected. As the Copernican doctrine made its way, this idea of compound motion naturally received more and more attention, and such experiments as those of Galileo prepared the way for a new interpretation of the mechanical principles involved.

The great difficulty was that the subject of moving bodies had all along been contemplated from a wrong point of view. Since force must be applied to an object to put it in motion, it was perhaps not unnaturally assumed that similar force must continue to be applied to keep the object in motion. When, for example, a stone is thrown from the hand, the direct force applied necessarily ceases as soon as the projectile leaves the hand. The stone, nevertheless, flies on for a certain distance and then falls to the ground. How is this flight of the stone to be explained? The ancient philosophers puzzled more than a little over this problem, and the Aristotelians reached the conclusion that the motion of the hand had imparted a propulsive motion to the air, and that this propulsive motion was transmitted to the stone, pushing it on. Just how the air took on this propulsive property was not explained, and the vagueness of thought that characterized the time did not demand an explanation. Possibly the dying away of ripples in water may have furnished, by analogy, an explanation of the gradual dying out of the impulse which propels the stone.

All of this was, of course, an unfortunate maladjustment of the point of view. As every one nowadays knows, the air retards the progress of the stone, enabling the pull of gravitation to drag it to the earth earlier than it otherwise could. Were the resistance of the air and the pull of

gravitation removed, the stone as projected from the hand would fly on in a straight line, at an unchanged velocity, forever. But this fact, which is expressed in what we now term the first law of motion, was extremely difficult to grasp. The first important step towards it was perhaps implied in Galileo's study of falling bodies. These studies, as we have seen, demonstrated that a half-pound weight and a hundred-pound weight fall with the same velocity. It is, however, matter of common experience that certain bodies, as, for example, feathers, do not fall at the same rate of speed with these heavier bodies. This anomaly demands an explanation, and the explanation is found in the resistance offered the relatively light object by the air. Once the idea that the air may thus act as an impeding force was grasped, the investigator of mechanical principles had entered on a new and promising course.

Galileo could not demonstrate the retarding influence of air in the way which became familiar a generation or two later; he could not put a feather and a coin in a vacuum tube and prove that the two would there fall with equal velocity, because, in his day, the air-pump had not yet been invented. The experiment was made only a generation after the time of Galileo, as we shall see; but, meantime, the great Italian had fully grasped the idea that atmospheric resistance plays a most important part in regard to the motion of falling and projected bodies. Thanks largely to his own experiments, but partly also to the efforts of others, he had come, before the end of his life, pretty definitely to realize that the motion of a projectile, for example, must be thought of as inherent in the projectile itself, and that the retardation or ultimate cessation of that motion is due to the action of antagonistic forces. In other words, he had come to grasp the meaning of the first law of motion. It remained, however, for the great Frenchman Descartes to give precise expression to this law two years after Galileo's death. As Descartes expressed it in his Principia Philosophiae, published in 1644, any body once in motion tends to go on in a straight line, at a uniform rate of speed, forever. Contrariwise, a stationary body will remain forever at rest unless acted on by some disturbing force.

This all-important law, which lies at the very foundation of all true conceptions of mechanics, was thus worked out during the first half of the seventeenth century, as the outcome of numberless experiments for which Galileo's experiments with failing bodies furnished the foundation. So numerous and so gradual were the steps by which the reversal of view regarding moving bodies was effected that it is impossible to trace them in detail. We must be content to reflect that at the beginning of the Galilean epoch utterly false notions regarding the subject were entertained by the very greatest philosophers--by Galileo himself, for example, and by Kepler--whereas at the close of that epoch the correct and highly illuminative view had been attained.

We must now consider some other experiments of Galileo which led to scarcely less-important results. The experiments in question had to do with the movements of bodies passing down an inclined plane, and with the allied subject of the motion of a pendulum. The elaborate experiments of Galileo regarding the former subject were made by measuring the velocity of a ball rolling down a plane inclined at various angles. He found that the velocity acquired by a ball was proportional to the height from which the ball descended regardless of the steepness of the incline. Experiments were made also with a ball rolling down a curved gutter, the curve representing the are of a circle. These experiments led to the study of the curvilinear motions of a weight suspended by a cord; in other words, of the pendulum.

Regarding the motion of the pendulum, some very curious facts were soon ascertained. Galileo

found, for example, that a pendulum of a given length performs its oscillations with the same frequency though the arc described by the pendulum be varied greatly.[1] He found, also, that the rate of oscillation for pendulums of different lengths varies according to a simple law. In order that one pendulum shall oscillate one-half as fast as another, the length of the pendulums must be as four to one. Similarly, by lengthening the pendulums nine times, the oscillation is reduced to one-third, In other words, the rate of oscillation of pendulums varies inversely as the square of their length. Here, then, is a simple relation between the motions of swinging bodies which suggests the relation which Kepler bad discovered between the relative motions of the planets. Every such discovery coming in this age of the rejuvenation of experimental science had a peculiar force in teaching men the all-important lesson that simple laws lie back of most of the diverse phenomena of nature, if only these laws can be discovered.

Galileo further observed that his pendulum might be constructed of any weight sufficiently heavy readily to overcome the atmospheric resistance, and that, with this qualification, neither the weight nor the material had any influence upon the time of oscillation, this being solely determined by the length of the cord. Naturally, the practical utility of these discoveries was not overlooked by Galileo. Since a pendulum of a given length oscillates with unvarying rapidity, here is an obvious means of measuring time. Galileo, however, appears not to have met with any great measure of success in putting this idea into practice. It remained for the mechanical ingenuity of Huyghens to construct a satisfactory pendulum clock.

As a theoretical result of the studies of rolling and oscillating bodies, there was developed what is usually spoken of as the third law of motion--namely, the law that a given force operates upon a moving body with an effect proportionate to its effect upon the same body when at rest. Or, as Whewell states the law: "The dynamical effect of force is as the statical effect; that is, the velocity which any force generates in a given time, when it puts the body in motion, is proportional to the pressure which this same force produces in a body at rest."[2] According to the second law of motion, each one of the different forces, operating at the same time upon a moving body, produces the same effect as if it operated upon the body while at rest.

STEVINUS AND THE LAW OF EQUILIBRIUM

It appears, then, that the mechanical studies of Galileo, taken as a whole, were nothing less than revolutionary. They constituted the first great advance upon the dynamic studies of Archimedes, and then led to the secure foundation for one of the most important of modern sciences. We shall see that an important company of students entered the field immediately after the time of Galileo, and carried forward the work he had so well begun. But before passing on to the consideration of their labors, we must consider work in allied fields of two men who were contemporaries of Galileo and whose original labors were in some respects scarcely less important than his own. These men are the Dutchman Stevinus, who must always be remembered as a co-laborer with Galileo in the foundation of the science of dynamics, and the Englishman Gilbert, to whom is due the unqualified praise of first subjecting the phenomenon of magnetism to a strictly scientific investigation.

Stevinus was born in the year 1548, and died in 1620. He was a man of a practical genius, and he attracted the attention of his non-scientific contemporaries, among other ways, by the construction of a curious land-craft, which, mounted on wheels, was to be propelled by sails like a boat. Not only did he write a book on this curious horseless carriage, but he put his idea into

practical application, producing a vehicle which actually traversed the distance between Scheveningen and Petton, with no fewer than twenty-seven passengers, one of them being Prince Maurice of Orange. This demonstration was made about the year 1600. It does not appear, however, that any important use was made of the strange vehicle; but the man who invented it put his mechanical ingenuity to other use with better effect. It was he who solved the problem of oblique forces, and who discovered the important hydrostatic principle that the pressure of fluids is proportionate to their depth, without regard to the shape of the including vessel.

The study of oblique forces was made by Stevinus with the aid of inclined planes. His most demonstrative experiment was a very simple one, in which a chain of balls of equal weight was hung from a triangle; the triangle being so constructed as to rest on a horizontal base, the oblique sides bearing the relation to each other of two to one. Stevinus found that his chain of balls just balanced when four balls were on the longer side and two on the shorter and steeper side. The balancing of force thus brought about constituted a stable equilibrium, Stevinus being the first to discriminate between such a condition and the unbalanced condition called unstable equilibrium. By this simple experiment was laid the foundation of the science of statics. Stevinus had a full grasp of the principle which his experiment involved, and he applied it to the solution of oblique forces in all directions. Earlier investigations of Stevinus were published in 1608. His collected works were published at Leyden in 1634.

This study of the equilibrium of pressure of bodies at rest led Stevinus, not unnaturally, to consider the allied subject of the pressure of liquids. He is to be credited with the explanation of the so-called hydrostatic paradox. The familiar modern experiment which illustrates this paradox is made by inserting a long perpendicular tube of small caliber into the top of a tight barrel. On filling the barrel and tube with water, it is possible to produce a pressure which will burst the barrel, though it be a strong one, and though the actual weight of water in the tube is comparatively insignificant. This illustrates the fact that the pressure at the bottom of a column of liquid is proportionate to the height of the column, and not to its bulk, this being the hydrostatic paradox in question. The explanation is that an enclosed fluid under pressure exerts an equal force upon all parts of the circumscribing wall; the aggregate pressure may, therefore, be increased indefinitely by increasing the surface. It is this principle, of course, which is utilized in the familiar hydrostatic press. Theoretical explanations of the pressure of liquids were supplied a generation or two later by numerous investigators, including Newton, but the practical refoundation of the science of hydrostatics in modern times dates from the experiments of Stevinus.

GALILEO AND THE EQUILIBRIUM OF FLUIDS

Experiments of an allied character, having to do with the equilibrium of fluids, exercised the ingenuity of Galileo. Some of his most interesting experiments have to do with the subject of floating bodies. It will be recalled that Archimedes, away back in the Alexandrian epoch, had solved the most important problems of hydrostatic equilibrium. Now, however, his experiments were overlooked or forgotten, and Galileo was obliged to make experiments anew, and to combat fallacious views that ought long since to have been abandoned. Perhaps the most illuminative view of the spirit of the times can be gained by quoting at length a paper of Galileo's, in which he details his own experiments with floating bodies and controverts the views of his opponents. The paper has further value as illustrating Galileo's methods both as experimenter and as speculative

reasoner.

The current view, which Galileo here undertakes to refute, asserts that water offers resistance to penetration, and that this resistance is instrumental in determining whether a body placed in water will float or sink. Galileo contends that water is non-resistant, and that bodies float or sink in virtue of their respective weights. This, of course, is merely a restatement of the law of Archimedes. But it remains to explain the fact that bodies of a certain shape will float, while bodies of the same material and weight, but of a different shape, will sink. We shall see what explanation Galileo finds of this anomaly as we proceed.

In the first place, Galileo makes a cone of wood or of wax, and shows that when it floats with either its point or its base in the water, it displaces exactly the same amount of fluid, although the apex is by its shape better adapted to overcome the resistance of the water, if that were the cause of buoyancy. Again, the experiment may be varied by tempering the wax with filings of lead till it sinks in the water, when it will be found that in any figure the same quantity of cork must be added to it to raise the surface.

"But," says Galileo, "this silences not my antagonists; they say that all the discourse hitherto made by me imports little to them, and that it serves their turn; that they have demonstrated in one instance, and in such manner and figure as pleases them best --namely, in a board and in a ball of ebony--that one when put into the water sinks to the bottom, and that the other stays to swim on the top; and the matter being the same, and the two bodies differing in nothing but in figure, they affirm that with all perspicuity they have demonstrated and sensibly manifested what they undertook. Nevertheless, I believe, and think I can prove, that this very experiment proves nothing against my theory. And first, it is false that the ball sinks and the board not; for the board will sink, too, if you do to both the figures as the words of our question require; that is, if you put them both in the water; for to be in the water implies to be placed in the water, and by Aristotle's own definition of place, to be placed imports to be environed by the surface of the ambient body; but when my antagonists show the floating board of ebony, they put it not into the water, but upon the water; where, being detained by a certain impediment (of which more anon), it is surrounded, partly with water, partly with air, which is contrary to our agreement, for that was that bodies should be in the water, and not part in the water, part in the air.

"I will not omit another reason, founded also upon experience, and, if I deceive not myself, conclusive against the notion that figure, and the resistance of the water to penetration, have anything to do with the buoyancy of bodies. Choose a piece of wood or other matter, as, for instance, walnut-wood, of which a ball rises from the bottom of the water to the surface more slowly than a ball of ebony of the same size sinks, so that, clearly, the ball of ebony divides the water more readily in sinking than the ball of wood does in rising. Then take a board of walnut-tree equal to and like the floating one of my antagonists; and if it be true that this latter floats by reason of the figure being unable to penetrate the water, the other of walnut-tree, without a question, if thrust to the bottom, ought to stay there, as having the same impeding figure, and being less apt to overcome the said resistance of the water. But if we find by experience that not only the thin board, but every other figure of the same walnut-tree, will return to float, as unquestionably we shall, then I must desire my opponents to forbear to attribute the floating of the ebony to the figure of the board, since the resistance of the water is the same in rising as in sinking, and the force of ascension of the walnut-tree is less than the ebony's force for going to

the bottom.

"Now let us return to the thin plate of gold or silver, or the thin board of ebony, and let us lay it lightly upon the water, so that it may stay there without sinking, and carefully observe the effect. It will appear clearly that the plates are a considerable matter lower than the surface of the water, which rises up and makes a kind of rampart round them on every side. But if it has already penetrated and overcome the continuity of the water, and is of its own nature heavier than the water, why does it not continue to sink, but stop and suspend itself in that little dimple that its weight has made in the water? My answer is, because in sinking till its surface is below the water, which rises up in a bank round it, it draws after and carries along with it the air above it, so that that which, in this case, descends in the water is not only the board of ebony or the plate of iron, but a compound of ebony and air, from which composition results a solid no longer specifically heavier than the water, as was the ebony or gold alone. But, gentlemen, we want the same matter; you are to alter nothing but the shape, and, therefore, have the goodness to remove this air, which may be done simply by washing the surface of the board, for the water having once got between the board and the air will run together, and the ebony will go to the bottom; and if it does not, you have won the day.

"But methinks I hear some of my antagonists cunningly opposing this, and telling me that they will not on any account allow their boards to be wetted, because the weight of the water so added, by making it heavier than it was before, draws it to the bottom, and that the addition of new weight is contrary to our agreement, which was that the matter should be the same.

"To this I answer, first, that nobody can suppose bodies to be put into the water without their being wet, nor do I wish to do more to the board than you may do to the ball. Moreover, it is not true that the board sinks on account of the weight of the water added in the washing; for I will put ten or twenty drops on the floating board, and so long as they stand separate it shall not sink; but if the board be taken out and all that water wiped off, and the whole surface bathed with one single drop, and put it again upon the water, there is no question but it will sink, the other water running to cover it, being no longer hindered by the air. In the next place, it is altogether false that water can in any way increase the weight of bodies immersed in it, for water has no weight in water, since it does not sink. Now just as he who should say that brass by its own nature sinks, but that when formed into the shape of a kettle it acquires from that figure the virtue of lying in water without sinking, would say what is false, because that is not purely brass which then is put into the water, but a compound of brass and air; so is it neither more nor less false that a thin plate of brass or ebony swims by virtue of its dilated and broad figure. Also, I cannot omit to tell my opponents that this conceit of refusing to bathe the surface of the board might beget an opinion in a third person of a poverty of argument on their side, especially as the conversation began about flakes of ice, in which it would be simple to require that the surfaces should be kept dry; not to mention that such pieces of ice, whether wet or dry, always float, and so my antagonists say, because of their shape.

"Some may wonder that I affirm this power to be in the air of keeping plate of brass or silver above water, as if in a certain sense I would attribute to the air a kind of magnetic virtue for sustaining heavy bodies with which it is in contact. To satisfy all these doubts I have contrived the following experiment to demonstrate how truly the air does support these bodies; for I have found, when one of these bodies which floats when placed lightly on the water is thoroughly

bathed and sunk to the bottom, that by carrying down to it a little air without otherwise touching it in the least, I am able to raise and carry it back to the top, where it floats as before. To this effect, I take a ball of wax, and with a little lead make it just heavy enough to sink very slowly to the bottom, taking care that its surface be quite smooth and even. This, if put gently into the water, submerges almost entirely, there remaining visible only a little of the very top, which, so long as it is joined to the air, keeps the ball afloat; but if we take away the contact of the air by wetting this top, the ball sinks to the bottom and remains there. Now to make it return to the surface by virtue of the air which before sustained it, thrust into the water a glass with the mouth downward, which will carry with it the air it contains, and move this down towards the ball until you see, by the transparency of the glass, that the air has reached the top of it; then gently draw the glass upward, and you will see the ball rise, and afterwards stay on the top of the water, if you carefully part the glass and water without too much disturbing it."[3]

It will be seen that Galileo, while holding in the main to a correct thesis, yet mingles with it some false ideas. At the very outset, of course, it is not true that water has no resistance to penetration; it is true, however, in the sense in which Galileo uses the term--that is to say, the resistance of the water to penetration is not the determining factor ordinarily in deciding whether a body sinks or floats. Yet in the case of the flat body it is not altogether inappropriate to say that the water resists penetration and thus supports the body. The modern physicist explains the phenomenon as due to surface-tension of the fluid. Of course, Galileo's disquisition on the mixing of air with the floating body is utterly fanciful. His experiments were beautifully exact; his theorizing from them was, in this instance, altogether fallacious. Thus, as already intimated, his paper is admirably adapted to convey a double lesson to the student of science.

WILLIAM GILBERT AND THE STUDY OF MAGNETISM

It will be observed that the studies of Galileo and Stevinus were chiefly concerned with the force of gravitation. Meanwhile, there was an English philosopher of corresponding genius, whose attention was directed towards investigation of the equally mysterious force of terrestrial magnetism. With the doubtful exception of Bacon, Gilbert was the most distinguished man of science in England during the reign of Queen Elizabeth. He was for many years court physician, and Queen Elizabeth ultimately settled upon him a pension that enabled him to continue his researches in pure science.

His investigations in chemistry, although supposed to be of great importance, are mostly lost; but his great work, De Magnete, on which he labored for upwards of eighteen years, is a work of sufficient importance, as Hallam says, "to raise a lasting reputation for its author." From its first appearance it created a profound impression upon the learned men of the continent, although in England Gilbert's theories seem to have been somewhat less favorably received. Galileo freely expressed his admiration for the work and its author; Bacon, who admired the author, did not express the same admiration for his theories; but Dr. Priestley, later, declared him to be "the father of modern electricity."

Strangely enough, Gilbert's book had never been translated into English, or apparently into any other language, until recent years, although at the time of its publication certain learned men, unable to read the book in the original, had asked that it should be. By this neglect, or oversight, a great number of general readers as well as many scientists, through succeeding centuries, have been deprived of the benefit of writings that contained a good share of the fundamental facts

about magnetism as known to-day.

Gilbert was the first to discover that the earth is a great magnet, and he not only gave the name of "pole" to the extremities of the magnetic needle, but also spoke of these "poles" as north and south pole, although he used these names in the opposite sense from that in which we now use them, his south pole being the extremity which pointed towards the north, and vice versa. He was also first to make use of the terms "electric force," "electric emanations," and "electric attractions."

It is hardly necessary to say that some of the views taken by Gilbert, many of his theories, and the accuracy of some of his experiments have in recent times been found to be erroneous. As a pioneer in an unexplored field of science, however, his work is remarkably accurate. "On the whole," says Dr. John Robinson, "this performance contains more real information than any writing of the age in which he lived, and is scarcely exceeded by any that has appeared since."[4]

In the preface to his work Gilbert says: "Since in the discovery of secret things, and in the investigation of hidden causes, stronger reasons are obtained from sure experiments and demonstrated arguments than from probable conjectures and the opinions of philosophical speculators of the common sort, therefore, to the end of that noble substance of that great loadstone, our common mother (the earth), still quite unknown, and also that the forces extraordinary and exalted of this globe may the better be understood, we have decided, first, to begin with the common stony and ferruginous matter, and magnetic bodies, and the part of the earth that we may handle and may perceive with senses, and then to proceed with plain magnetic experiments, and to penetrate to the inner parts of the earth."[5]

Before taking up the demonstration that the earth is simply a giant loadstone, Gilbert demonstrated in an ingenious way that every loadstone, of whatever size, has definite and fixed poles. He did this by placing the stone in a metal lathe and converting it into a sphere, and upon this sphere demonstrated how the poles can be found. To this round loadstone he gave the name of terrella--that is, little earth.

"To find, then, poles answering to the earth," he says, "take in your hand the round stone, and lay on it a needle or a piece of iron wire: the ends of the wire move round their middle point, and suddenly come to a standstill. Now, with ochre or with chalk, mark where the wire lies still and sticks. Then move the middle or centre of the wire to another spot, and so to a third and fourth, always marking the stone along the length of the wire where it stands still; the lines so marked will exhibit meridian circles, or circles like meridians, on the stone or terrella; and manifestly they will all come together at the poles of the stone. The circle being continued in this way, the poles appear, both the north and the south, and betwixt these, midway, we may draw a large circle for an equator, as is done by the astronomer in the heavens and on his spheres, and by the geographer on the terrestrial globe."[6]

Gilbert had tried the familiar experiment of placing the loadstone on a float in water, and observed that the poles always revolved until they pointed north and south, which he explained as due to the earth's magnetic attraction. In this same connection he noticed that a piece of wrought iron mounted on a cork float was attracted by other metals to a slight degree, and he observed also that an ordinary iron bar, if suspended horizontally by a thread, assumes invariably a north and south direction. These, with many other experiments of a similar nature, convinced

him that the earth "is a magnet and a loadstone," which he says is a "new and till now unheard-of view of the earth."

Fully to appreciate Gilbert's revolutionary views concerning the earth as a magnet, it should be remembered that numberless theories to explain the action of the electric needle had been advanced. Columbus and Paracelsus, for example, believed that the magnet was attracted by some point in the heavens, such as a magnetic star. Gilbert himself tells of some of the beliefs that had been held by his predecessors, many of whom he declares "wilfully falsify." One of his first steps was to refute by experiment such assertions as that of Cardan, that "a wound by a magnetized needle was painless"; and also the assertion of Fracastoni that loadstone attracts silver; or that of Scalinger, that the diamond will attract iron; and the statement of Matthiolus that "iron rubbed with garlic is no longer attracted to the loadstone."

Gilbert made extensive experiments to explain the dipping of the needle, which had been first noticed by William Norman. His deduction as to this phenomenon led him to believe that this was also explained by the magnetic attraction of the earth, and to predict where the vertical dip would be found. These deductions seem the more wonderful because at the time he made them the dip had just been discovered, and had not been studied except at London. His theory of the dip was, therefore, a scientific prediction, based on a preconceived hypothesis. Gilbert found the dip to be 72 degrees at London; eight years later Hudson found the dip at 75 degrees 22' north latitude to be 89 degrees 30'; but it was not until over two hundred years later, in 1831, that the vertical dip was first observed by Sir James Ross at about 70 degrees 5' north latitude, and 96 degrees 43' west longitude. This was not the exact point assumed by Gilbert, and his scientific predictions, therefore, were not quite correct; but such comparatively slight and excusable errors mar but little the excellence of his work as a whole.

A brief epitome of some of his other important discoveries suffices to show that the exalted position in science accorded him by contemporaries, as well as succeeding generations of scientists, was well merited. He was first to distinguish between magnetism and electricity, giving the latter its name. He discovered also the "electrical charge," and pointed the way to the discovery of insulation by showing that the charge could be retained some time in the excited body by covering it with some non-conducting substance, such as silk; although, of course, electrical conduction can hardly be said to have been more than vaguely surmised, if understood at all by him. The first electrical instrument ever made, and known as such, was invented by him, as was also the first magnetometer, and the first electrical indicating device. Although three centuries have elapsed since his death, the method of magnetizing iron first introduced by him is in common use to-day.

He made exhaustive experiments with a needle balanced on a pivot to see how many substances he could find which, like amber, on being rubbed affected the needle. In this way he discovered that light substances were attracted by alum, mica, arsenic, sealing-wax, lac sulphur, slags, beryl, amethyst, rock-crystal, sapphire, jet, carbuncle, diamond, opal, Bristol stone, glass, glass of antimony, gum-mastic, hard resin, rock-salt, and, of course, amber. He discovered also that atmospheric conditions affected the production of electricity, dryness being unfavorable and moisture favorable.

Galileo's estimate of this first electrician is the verdict of succeeding generations. "I extremely admire and envy this author," he said. "I think him worthy of the greatest praise for the many

new and true observations which he has made, to the disgrace of so many vain and fabling authors."

STUDIES OF LIGHT, HEAT, AND ATMOSPHERIC PRESSURE

We have seen that Gilbert was by no means lacking in versatility, yet the investigations upon which his fame is founded were all pursued along one line, so that the father of magnetism may be considered one of the earliest of specialists in physical science. Most workers of the time, on the other band, extended their investigations in many directions. The sum total of scientific knowledge of that day had not bulked so large as to exclude the possibility that one man might master it all. So we find a Galileo, for example, making revolutionary discoveries in astronomy, and performing fundamental experiments in various fields of physics. Galileo's great contemporary, Kepler, was almost equally versatile, though his astronomical studies were of such pre-eminent importance that his other investigations sink into relative insignificance. Yet he performed some notable experiments in at least one department of physics. These experiments had to do with the refraction of light, a subject which Kepler was led to investigate, in part at least, through his interest in the telescope.

We have seen that Ptolemy in the Alexandrian time, and Alhazen, the Arab, made studies of refraction. Kepler repeated their experiments, and, striving as always to generalize his observations, he attempted to find the law that governed the observed change of direction which a ray of light assumes in passing from one medium to another. Kepler measured the angle of refraction by means of a simple yet ingenious trough-like apparatus which enabled him to compare readily the direct and refracted rays. He discovered that when a ray of light passes through a glass plate, if it strikes the farther surface of the glass at an angle greater than 45 degrees it will be totally refracted instead of passing through into the air. He could not well fail to know that different mediums refract light differently, and that for the same medium the amount of light valies with the change in the angle of incidence. He was not able, however, to generalize his observations as he desired, and to the last the law that governs refraction escaped him. It remained for Willebrord Snell, a Dutchman, about the year 1621, to discover the law in question, and for Descartes, a little later, to formulate it. Descartes, indeed, has sometimes been supposed to be the discoverer of the law. There is reason to believe that he based his generalizations on the experiment of Snell, though he did not openly acknowledge his indebtedness. The law, as Descartes expressed it, states that the sine of the angle of incidence bears a fixed ratio to the sine of the angle of refraction for any given medium. Here, then, was another illustration of the fact that almost infinitely varied phenomena may be brought within the scope of a simple law. Once the law had been expressed, it could be tested and verified with the greatest ease; and, as usual, the discovery being made, it seems surprising that earlier investigators--in particular so sagacious a guesser as Kepler--should have missed it.

Galileo himself must have been to some extent a student of light, since, as we have seen, he made such notable contributions to practical optics through perfecting the telescope; but he seems not to have added anything to the theory of light. The subject of heat, however, attracted his attention in a somewhat different way, and he was led to the invention of the first contrivance for measuring temperatures. His thermometer was based on the afterwards familiar principle of the expansion of a liquid under the influence of heat; but as a practical means of measuring temperature it was a very crude affair, because the tube that contained the measuring liquid was

exposed to the air, hence barometric changes of pressure vitiated the experiment. It remained for Galileo's Italian successors of the Accademia del Cimento of Florence to improve upon the apparatus, after the experiments of Torricelli--to which we shall refer in a moment--had thrown new light on the question of atmospheric pressure. Still later the celebrated Huygens hit upon the idea of using the melting and the boiling point of water as fixed points in a scale of measurements, which first gave definiteness to thermometric tests.

TORRICELLI

In the closing years of his life Galileo took into his family, as his adopted disciple in science, a young man, Evangelista Torricelli (1608-1647), who proved himself, during his short lifetime, to be a worthy follower of his great master. Not only worthy on account of his great scientific discoveries, but grateful as well, for when he had made the great discovery that the "suction" made by a vacuum was really nothing but air pressure, and not suction at all, he regretted that so important a step in science might not have been made by his great teacher, Galileo, instead of by himself. "This generosity of Torricelli," says Playfair, "was, perhaps, rarer than his genius: there are more who might have discovered the suspension of mercury in the barometer than who would have been willing to part with the honor of the discovery to a master or a friend."

Torricelli's discovery was made in 1643, less than two years after the death of his master. Galileo had observed that water will not rise in an exhausted tube, such as a pump, to a height greater than thirty-three feet, but he was never able to offer a satisfactory explanation of the principle. Torricelli was able to demonstrate that the height at which the water stood depended upon nothing but its weight as compared with the weight of air. If this be true, it is evident that any fluid will be supported at a definite height, according to its relative weight as compared with air. Thus mercury, which is about thirteen times more dense than water, should only rise to one-thirteenth the height of a column of water--that is, about thirty inches. Reasoning in this way, Torricelli proceeded to prove that his theory was correct. Filling a long tube, closed at one end, with mercury, he inverted the tube with its open orifice in a vessel of mercury. The column of mercury fell at once, but at a height of about thirty inches it stopped and remained stationary, the pressure of the air on the mercury in the vessel maintaining it at that height. This discovery was a shattering blow to the old theory that had dominated that field of physics for so many centuries. It was completely revolutionary to prove that, instead of a mysterious something within the tube being responsible for the suspension of liquids at certain heights, it was simply the ordinary atmospheric pressure mysterious enough, it is true--pushing upon them from without. The pressure exerted by the atmosphere was but little understood at that time, but Torricelli's discovery aided materially in solving the mystery. The whole class of similar phenomena of air pressure, which had been held in the trammel of long-established but false doctrines, was now reduced to one simple law, and the door to a solution of a host of unsolved problems thrown open.

It had long been suspected and believed that the density of the atmosphere varies at certain times. That the air is sometimes "heavy" and at other times "light" is apparent to the senses without scientific apparatus for demonstration. It is evident, then, that Torricelli's column of mercury should rise and fall just in proportion to the lightness or heaviness of the air. A short series of observations proved that it did so, and with those observations went naturally the observations as to changes in the weather. It was only necessary, therefore, to scratch a scale on the glass tube,

indicating relative atmospheric pressures, and the Torricellian barometer was complete.

Such a revolutionary theory and such an important discovery were, of course, not to be accepted without controversy, but the feeble arguments of the opponents showed how untenable the old theory had become. In 1648 Pascal suggested that if the theory of the pressure of air upon the mercury was correct, it could be demonstrated by ascending a mountain with the mercury tube. As the air was known to get progressively lighter from base to summit, the height of the column should be progressively lessened as the ascent was made, and increase again on the descent into the denser air. The experiment was made on the mountain called the Puy-de-Dome, in Auvergne, and the column of mercury fell and rose progressively through a space of about three inches as the ascent and descent were made.

This experiment practically sealed the verdict on the new theory, but it also suggested something more. If the mercury descended to a certain mark on the scale on a mountain-top whose height was known, why was not this a means of measuring the heights of all other elevations? And so the beginning was made which, with certain modifications and corrections in details, is now the basis of barometrical measurements of heights.

In hydraulics, also, Torricelli seems to have taken one of the first steps. He did this by showing that the water which issues from a hole in the side or bottom of a vessel does so at the same velocity as that which a body would acquire by falling from the level of the surface of the water to that of the orifice. This discovery was of the greatest importance to a correct understanding of the science of the motions of fluids. He also discovered the valuable mechanical principle that if any number of bodies be connected so that by their motion there is neither ascent nor descent of their centre of gravity, these bodies are in equilibrium.

Besides making these discoveries, he greatly improved the microscope and the telescope, and invented a simple microscope made of a globule of glass. In 1644 he published a tract on the properties of the cycloid in which he suggested a solution of the problem of its quadrature. As soon as this pamphlet appeared its author was accused by Gilles Roberval (1602-1675) of having appropriated a solution already offered by him. This led to a long debate, during which Torricelli was seized with a fever, from the effects of which he died, in Florence, October 25, 1647. There is reason to believe, however, that while Roberval's discovery was made before Torricelli's, the latter reached his conclusions independently.

VI. TWO PSEUDO-SCIENCES--ALCHEMY AND ASTROLOGY

In recent chapters we have seen science come forward with tremendous strides. A new era is obviously at hand. But we shall misconceive the spirit of the times if we fail to understand that in the midst of all this progress there was still room for mediaeval superstition and for the pursuit of fallacious ideals. Two forms of pseudo-science were peculiarly prevalent --alchemy and astrology. Neither of these can with full propriety be called a science, yet both were pursued by many of the greatest scientific workers of the period. Moreover, the studies of the alchemist may with some propriety be said to have laid the foundation for the latter-day science of chemistry; while astrology was closely allied to astronomy, though its relations to that science are not as intimate as has sometimes been supposed.

Just when the study of alchemy began is undetermined. It was certainly of very ancient origin,

perhaps Egyptian, but its most flourishing time was from about the eighth century A.D. to the eighteenth century. The stories of the Old Testament formed a basis for some of the strange beliefs regarding the properties of the magic "elixir," or "philosopher's stone." Alchemists believed that most of the antediluvians, perhaps all of them, possessed a knowledge of this stone. How, otherwise, could they have prolonged their lives to nine and a half centuries? And Moses was surely a first-rate alchemist, as is proved by the story of the Golden Calf.[1] After Aaron had made the calf of gold, Moses performed the much more difficult task of grinding it to powder and "strewing it upon the waters," thus showing that he had transmuted it into some lighter substance.

But antediluvians and Biblical characters were not the only persons who were thought to have discovered the coveted. "elixir." Hundreds of aged mediaeval chemists were credited with having made the discovery, and were thought to be living on through the centuries by its means. Alaies de Lisle, for example, who died in 1298, at the age of 110, was alleged to have been at the point of death at the age of fifty, but just at this time he made the fortunate discovery of the magic stone, and so continued to live in health and affluence for sixty years more. And De Lisle was but one case among hundreds.

An aged and wealthy alchemist could claim with seeming plausibility that he was prolonging his life by his magic; whereas a younger man might assert that, knowing the great secret, he was keeping himself young through the centuries. In either case such a statement, or rumor, about a learned and wealthy alchemist was likely to be believed, particularly among strangers; and as such a man would, of course, be the object of much attention, the claim was frequently made by persons seeking notoriety. One of the most celebrated of these impostors was a certain Count de Saint-Germain, who was connected with the court of Louis XV. His statements carried the more weight because, having apparently no means of maintenance, he continued to live in affluence year after year--for two thousand years, as he himself admitted--by means of the magic stone. If at any time his statements were doubted, he was in the habit of referring to his valet for confirmation, this valet being also under the influence of the elixir of life.

"Upon one occasion his master was telling a party of ladies and gentlemen, at dinner, some conversation he had had in Palestine, with King Richard I., of England, whom he described as a very particular friend of his. Signs of astonishment and incredulity were visible on the faces of the company, upon which Saint-Germain very coolly turned to his servant, who stood behind his chair, and asked him if he had not spoken the truth. 'I really cannot say,' replied the man, without moving a muscle; 'you forget, sir, I have been only five hundred years in your service.' 'Ah, true,' said his master, 'I remember now; it was a little before your time!' "[2]

In the time of Saint-Germain, only a little over a century ago, belief in alchemy had almost disappeared, and his extraordinary tales were probably regarded in the light of amusing stories. Still there was undoubtedly a lingering suspicion in the minds of many that this man possessed some peculiar secret. A few centuries earlier his tales would hardly have been questioned, for at that time the belief in the existence of this magic something was so strong that the search for it became almost a form of mania; and once a man was seized with it, lie gambled away health, position, and life itself in pursuing the coveted stake. An example of this is seen in Albertus Magnus, one of the most learned men of his time, who it is said resigned his position as bishop of Ratisbon in order that he might pursue his researches in alchemy.

If self-sacrifice was not sufficient to secure the prize, crime would naturally follow, for there could be no limit to the price of the stakes in this game. The notorious Marechal de Reys, failing to find the coveted stone by ordinary methods of laboratory research, was persuaded by an impostor that if he would propitiate the friendship of the devil the secret would be revealed. To this end De Reys began secretly capturing young children as they passed his castle and murdering them. When he was at last brought to justice it was proved that he had murdered something like a hundred children within a period of three years. So, at least, runs one version of the story of this perverted being.

Naturally monarchs, constantly in need of funds, were interested in these alchemists. Even sober England did not escape, and Raymond Lully, one of the most famous of the thirteenth and fourteenth century alchemists, is said to have been secretly invited by King Edward I. (or II.) to leave Milan and settle in England. According to some accounts, apartments were assigned to his use in the Tower of London, where he is alleged to have made some six million pounds sterling for the monarch, out of iron, mercury, lead, and pewter.

Pope John XXII., a friend and pupil of the alchemist Arnold de Villeneuve, is reported to have learned the secrets of alchemy from his master. Later he issued two bulls against "pretenders" in the art, which, far from showing his disbelief, were cited by alchemists as proving that he recognized pretenders as distinct from true masters of magic.

To moderns the attitude of mind of the alchemist is difficult to comprehend. It is, perhaps, possible to conceive of animals or plants possessing souls, but the early alchemist attributed the same thing--or something kin to it--to metals also. Furthermore, just as plants germinated from seeds, so metals were supposed to germinate also, and hence a constant growth of metals in the ground. To prove this the alchemist cited cases where previously exhausted gold-mines were found, after a lapse of time, to contain fresh quantities of gold. The "seed" of the remaining particles of gold had multiplied and increased. But this germinating process could only take place under favorable conditions, just as the seed of a plant must have its proper surroundings before germinating; and it was believed that the action of the philosopher's stone was to hasten this process, as man may hasten the growth of plants by artificial means. Gold was looked upon as the most perfect metal, and all other metals imperfect, because not yet "purified." By some alchemists they were regarded as lepers, who, when cured of their leprosy, would become gold. And since nature intended that all things should be perfect, it was the aim of the alchemist to assist her in this purifying process, and incidentally to gain wealth and prolong his life.

By other alchemists the process of transition from baser metals into gold was conceived to be like a process of ripening fruit. The ripened product was gold, while the green fruit, in various stages of maturity, was represented by the base metals. Silver, for example, was more nearly ripe than lead; but the difference was only one of "digestion," and it was thought that by further "digestion" lead might first become silver and eventually gold. In other words, Nature had not completed her work, and was wofully slow at it at best; but man, with his superior faculties, was to hasten the process in his laboratories--if he could but hit upon the right method of doing so.

It should not be inferred that the alchemist set about his task of assisting nature in a haphazard way, and without training in the various alchemic laboratory methods. On the contrary, he usually served a long apprenticeship in the rudiments of his calling. He was obliged to learn, in a general way, many of the same things that must be understood in either chemical or alchemical

laboratories. The general knowledge that certain liquids vaporize at lower temperatures than others, and that the melting-points of metals differ greatly, for example, was just as necessary to alchemy as to chemistry. The knowledge of the gross structure, or nature, of materials was much the same to the alchemist as to the chemist, and, for that matter, many of the experiments in calcining, distilling, etc., were practically identical.

To the alchemist there were three principles--salt, sulphur, and mercury--and the sources of these principles were the four elements--earth, water, fire, and air. These four elements were accountable for every substance in nature. Some of the experiments to prove this were so illusive, and yet apparently so simple, that one is not surprised that it took centuries to disprove them. That water was composed of earth and air seemed easily proven by the simple process of boiling it in a tea-kettle, for the residue left was obviously an earthy substance, whereas the steam driven off was supposed to be air. The fact that pure water leaves no residue was not demonstrated until after alchemy had practically ceased to exist. It was possible also to demonstrate that water could be turned into fire by thrusting a red-hot poker under a bellglass containing a dish of water. Not only did the quantity of water diminish, but, if a lighted candle was thrust under the glass, the contents ignited and burned, proving, apparently, that water had been converted into fire. These, and scores of other similar experiments, seemed so easily explained, and to accord so well with the "four elements" theory, that they were seldom questioned until a later age of inductive science.

But there was one experiment to which the alchemist pinned his faith in showing that metals could be "killed" and "revived," when proper means were employed. It had been known for many centuries that if any metal, other than gold or silver, were calcined in an open crucible, it turned, after a time, into a peculiar kind of ash. This ash was thought by the alchemist to represent the death of the metal. But if to this same ash a few grains of wheat were added and heat again applied to the crucible, the metal was seen to "rise from its ashes" and resume its original form--a well-known phenomenon of reducing metals from oxides by the use of carbon, in the form of wheat, or, for that matter, any other carbonaceous substance. Wheat was, therefore, made the symbol of the resurrection of the life eternal. Oats, corn, or a piece of charcoal would have "revived" the metals from the ashes equally well, but the mediaeval alchemist seems not to have known this. However, in this experiment the metal seemed actually to be destroyed and revivified, and, as science had not as yet explained this striking phenomenon, it is little wonder that it deceived the alchemist.

Since the alchemists pursued their search of the magic stone in such a methodical way, it would seem that they must have some idea of the appearance of the substance they sought. Probably they did, each according to his own mental bias; but, if so, they seldom committed themselves to writing, confining their discourses largely to speculations as to the properties of this illusive substance. Furthermore, the desire for secrecy would prevent them from expressing so important a piece of information. But on the subject of the properties, if not on the appearance of the "essence," they were voluminous writers. It was supposed to be the only perfect substance in existence, and to be confined in various substances, in quantities proportionate to the state of perfection of the substance. Thus, gold being most nearly perfect would contain more, silver less, lead still less, and so on. The "essence" contained in the more nearly perfect metals was thought to be more potent, a very small quantity of it being capable of creating large quantities of gold and of prolonging life indefinitely.

It would appear from many of the writings of the alchemists that their conception of nature and the supernatural was so confused and entangled in an inexplicable philosophy that they themselves did not really understand the meaning of what they were attempting to convey. But it should not be forgotten that alchemy was kept as much as possible from the ignorant general public, and the alchemists themselves had knowledge of secret words and expressions which conveyed a definite meaning to one of their number, but which would appear a meaningless jumble to an outsider. Some of these writers declared openly that their writings were intended to convey an entirely erroneous impression, and were sent out only for that purpose.

However, while it may have been true that the vagaries of their writings were made purposely, the case is probably more correctly explained by saying that the very nature of the art made definite statements impossible. They were dealing with something that did not exist--could not exist. Their attempted descriptions became, therefore, the language of romance rather than the language of science.

But if the alchemists themselves were usually silent as to the appearance of the actual substance of the philosopher's stone, there were numberless other writers who were less reticent. By some it was supposed to be a stone, by others a liquid or elixir, but more commonly it was described as a black powder. It also possessed different degrees of efficiency according to its degrees of purity, certain forms only possessing the power of turning base metals into gold, while others gave eternal youth and life or different degrees of health. Thus an alchemist, who had made a partial discovery of this substance, could prolong life a certain number of years only, or, possessing only a small and inadequate amount of the magic powder, he was obliged to give up the ghost when the effect of this small quantity had passed away.

This belief in the supernatural power of the philosopher's stone to prolong life and heal diseases was probably a later phase of alchemy, possibly developed by attempts to connect the power of the mysterious essence with Biblical teachings. The early Roman alchemists, who claimed to be able to transmute metals, seem not to have made other claims for their magic stone.

By the fifteenth century the belief in the philosopher's stone had become so fixed that governments began to be alarmed lest some lucky possessor of the secret should flood the country with gold, thus rendering the existing coin of little value. Some little consolation was found in the thought that in case all the baser metals were converted into gold iron would then become the "precious metal," and would remain so until some new philosopher's stone was found to convert gold back into iron--a much more difficult feat, it was thought. However, to be on the safe side, the English Parliament, in 1404, saw fit to pass an act declaring the making of gold and silver to be a felony. Nevertheless, in 1455, King Henry VI. granted permission to several "knights, citizens of London, chemists, and monks" to find the philosopher's stone, or elixir, that the crown might thus be enabled to pay off its debts. The monks and ecclesiastics were supposed to be most likely to discover the secret process, since "they were such good artists in transubstantiating bread and wine."

In Germany the emperors Maximilian I., Rudolf II., and Frederick II. gave considerable attention to the search, and the example they set was followed by thousands of their subjects. It is said that some noblemen developed the unpleasant custom of inviting to their courts men who were reputed to have found the stone, and then imprisoning the poor alchemists until they had made a certain quantity of gold, stimulating their activity with tortures of the most atrocious kinds. Thus

this danger of being imprisoned and held for ransom until some fabulous amount of gold should be made became the constant menace of the alchemist. It was useless for an alchemist to plead poverty once it was noised about that he had learned the secret. For how could such a man be poor when, with a piece of metal and a few grains of magic powder, he was able to provide himself with gold? It was, therefore, a reckless alchemist indeed who dared boast that he had made the coveted discovery.

The fate of a certain indiscreet alchemist, supposed by many to have been Seton, a Scotchman, was not an uncommon one. Word having been brought to the elector of Saxony that this alchemist was in Dresden and boasting of his powers, the elector caused him to be arrested and imprisoned. Forty guards were stationed to see that he did not escape and that no one visited him save the elector himself. For some time the elector tried by argument and persuasion to penetrate his secret or to induce him to make a certain quantity of gold; but as Seton steadily refused, the rack was tried, and for several months he suffered torture, until finally, reduced to a mere skeleton, be was rescued by a rival candidate of the elector, a Pole named Michael Sendivogins, who drugged the guards. However, before Seton could be "persuaded" by his new captor, he died of his injuries.

But Sendivogins was also ambitious in alchemy, and, since Seton was beyond his reach, he took the next best step and married his widow. From her, as the story goes, he received an ounce of black powder--the veritable philosopher's stone. With this he manufactured great quantities of gold, even inviting Emperor Rudolf II. to see him work the miracle. That monarch was so impressed that he caused a tablet to be inserted in the wall of the room in which he had seen the gold made.

Sendivogins had learned discretion from the misfortune of Seton, so that he took the precaution of concealing most of the precious powder in a secret chamber of his carriage when he travelled, having only a small quantity carried by his steward in a gold box. In particularly dangerous places, he is said to have exchanged clothes with his coachman, making the servant take his place in the carriage while he mounted the box.

About the middle of the seventeenth century alchemy took such firm root in the religious field that it became the basis of the sect known as the Rosicrucians. The name was derived from the teaching of a German philosopher, Rosenkreutz, who, having been healed of a dangerous illness by an Arabian supposed to possess the philosopher's stone, returned home and gathered about him a chosen band of friends, to whom he imparted the secret. This sect came rapidly into prominence, and for a short time at least created a sensation in Europe, and at the time were credited with having "refined and spiritualized" alchemy. But by the end of the seventeenth century their number had dwindled to a mere handful, and henceforth they exerted little influence.

Another and earlier religious sect was the Aureacrucians, founded by Jacob Bohme, a shoemaker, born in Prussia in 1575. According to his teachings the philosopher's stone could be discovered by a diligent search of the Old and the New Testaments, and more particularly the Apocalypse, which contained all the secrets of alchemy. This sect found quite a number of followers during the life of Bohme, but gradually died out after his death; not, however, until many of its members had been tortured for heresy, and one at least, Kuhlmann, of Moscow, burned as a sorcerer.

The names of the different substances that at various times were thought to contain the large quantities of the "essence" during the many centuries of searching for it, form a list of practically all substances that were known, discovered, or invented during the period. Some believed that acids contained the substance; others sought it in minerals or in animal or vegetable products; while still others looked to find it among the distilled "spirits"--the alcoholic liquors and distilled products. On the introduction of alcohol by the Arabs that substance became of all-absorbing interest, and for a long time allured the alchemist into believing that through it they were soon to be rewarded. They rectified and refined it until "sometimes it was so strong that it broke the vessels containing it," but still it failed in its magic power. Later, brandy was substituted for it, and this in turn discarded for more recent discoveries.

There were always, of course, two classes of alchemists: serious investigators whose honesty could not be questioned, and clever impostors whose legerdemain was probably largely responsible for the extended belief in the existence of the philosopher's stone. Sometimes an alchemist practised both, using the profits of his sleight-of-hand to procure the means of carrying on his serious alchemical researches. The impostures of some of these jugglers deceived even the most intelligent and learned men of the time, and so kept the flame of hope constantly burning. The age of cold investigation had not arrived, and it is easy to understand how an unscrupulous mediaeval Hermann or Kellar might completely deceive even the most intelligent and thoughtful scholars. In scoffing at the credulity of such an age, it should not be forgotten that the "Keely motor" was a late nineteenth-century illusion.

But long before the belief in the philosopher's stone had died out, the methods of the legerdemain alchemist had been investigated and reported upon officially by bodies of men appointed to make such investigations, although it took several generations completely to overthrow a superstition that had been handed down through several thousand years. In April of 1772 Monsieur Geoffroy made a report to the Royal Academy of Sciences, at Paris, on the alchemic cheats principally of the sixteenth and seventeenth centuries. In this report he explains many of the seemingly marvellous feats of the unscrupulous alchemists. A very common form of deception was the use of a double-bottomed crucible. A copper or brass crucible was covered on the inside with a layer of wax, cleverly painted so as to resemble the ordinary metal. Between this layer of wax and the bottom of the crucible, however, was a layer of gold dust or silver. When the alchemist wished to demonstrate his power, he had but to place some mercury or whatever substance he chose in the crucible, heat it, throw in a grain or two of some mysterious powder, pronounce a few equally mysterious phrases to impress his audience, and, behold, a lump of precious metal would be found in the bottom of his pot. This was the favorite method of mediocre performers, but was, of course, easily detected.

An equally successful but more difficult way was to insert surreptitiously a lump of metal into the mixture, using an ordinary crucible. This required great dexterity, but was facilitated by the use of many mysterious ceremonies on the part of the operator while performing, just as the modern vaudeville performer diverts the attention of the audience to his right hand while his left is engaged in the trick. Such ceremonies were not questioned, for it was the common belief that the whole process "lay in the spirit as much as in the substance," many, as we have seen, regarding the whole process as a divine manifestation.

Sometimes a hollow rod was used for stirring the mixture in the crucible, this rod containing

gold dust, and having the end plugged either with wax or soft metal that was easily melted. Again, pieces of lead were used which had been plugged with lumps of gold carefully covered over; and a very simple and impressive demonstration was making use of a nugget of gold that had been coated over with quicksilver and tarnished so as to resemble lead or some base metal. When this was thrown into acid the coating was removed by chemical action, leaving the shining metal in the bottom of the vessel. In order to perform some of these tricks, it is obvious that the alchemist must have been well supplied with gold, as some of them, when performing before a royal audience, gave the products to their visitors. But it was always a paying investment, for once his reputation was established the gold-maker found an endless variety of ways of turning his alleged knowledge to account, frequently amassing great wealth.

Some of the cleverest of the charlatans often invited royal or other distinguished guests to bring with them iron nails to be turned into gold ones. They were transmuted in the alchemist's crucible before the eyes of the visitors, the juggler adroitly extracting the iron nail and inserting a gold one without detection. It mattered little if the converted gold nail differed in size and shape from the original, for this change in shape could be laid to the process of transmutation; and even the very critical were hardly likely to find fault with the exchange thus made. Furthermore, it was believed that gold possessed the property of changing its bulk under certain conditions, some of the more conservative alchemists maintaining that gold was only increased in bulk, not necessarily created, by certain forms of the magic stone. Thus a very proficient operator was thought to be able to increase a grain of gold into a pound of pure metal, while one less expert could only double, or possibly treble, its original weight.

The actual number of useful discoveries resulting from the efforts of the alchemists is considerable, some of them of incalculable value. Roger Bacon, who lived in the thirteenth century, while devoting much of his time to alchemy, made such valuable discoveries as the theory, at least, of the telescope, and probably gunpowder. Of this latter we cannot be sure that the discovery was his own and that he had not learned of it through the source of old manuscripts. But it is not impossible nor improbable that he may have hit upon the mixture that makes the explosives while searching for the philosopher's stone in his laboratory. "Von Helmont, in the same pursuit, discoverd the properties of gas," says Mackay; "Geber made discoveries in chemistry, which were equally important; and Paracelsus, amid his perpetual visions of the transmutation of metals, found that mercury was a remedy for one of the most odious and excruciating of all the diseases that afflict humanity."' As we shall see a little farther on, alchemy finally evolved into modern chemistry, but not until it had passed through several important transitional stages.

ASTROLOGY

In a general way modern astronomy may be considered as the outgrowth of astrology, just as modern chemistry is the result of alchemy. It is quite possible, however, that astronomy is the older of the two; but astrology must have developed very shortly after. The primitive astronomer, having acquired enough knowledge from his observations of the heavenly bodies to make correct predictions, such as the time of the coming of the new moon, would be led, naturally, to believe that certain predictions other than purely astronomical ones could be made by studying the heavens. Even if the astronomer himself did not believe this, some of his superstitious admirers would; for to the unscientific mind predictions of earthly events would surely seem no more

miraculous than correct predictions as to the future movements of the sun, moon, and stars. When astronomy had reached a stage of development so that such things as eclipses could be predicted with anything like accuracy, the occult knowledge of the astronomer would be unquestioned. Turning this apparently occult knowledge to account in a mercenary way would then be the inevitable result, although it cannot be doubted that many of the astrologers, in all ages, were sincere in their beliefs.

Later, as the business of astrology became a profitable one, sincere astronomers would find it expedient to practise astrology as a means of gaining a livelihood. Such a philosopher as Kepler freely admitted that he practised astrology "to keep from starving," although he confessed no faith in such predictions. "Ye otherwise philosophers," he said, "ye censure this daughter of astronomy beyond her deserts; know ye not that she must support her mother by her charms."

Once astrology had become an established practice, any considerable knowledge of astronomy was unnecessary, for as it was at best but a system of good guessing as to future events, clever impostors could thrive equally well without troubling to study astronomy. The celebrated astrologers, however, were usually astronomers as well, and undoubtedly based many of their predictions on the position and movements of the heavenly bodies. Thus, the casting of a horoscope that is, the methods by which the astrologers ascertained the relative position of the heavenly bodies at the time of a birth--was a simple but fairly exact procedure. Its basis was the zodiac, or the path traced by the sun in his yearly course through certain constellations. At the moment of the birth of a child, the first care of the astrologer was to note the particular part of the zodiac that appeared on the horizon. The zodiac was then divided into "houses"--that is, into twelve spaces--on a chart. In these houses were inserted the places of the planets, sun, and moon, with reference to the zodiac. When this chart was completed it made a fairly correct diagram of the heavens and the position of the heavenly bodies as they would appear to a person standing at the place of birth at a certain time.

Up to this point the process was a simple one of astronomy. But the next step--the really important one--that of interpreting this chart, was the one which called forth the skill and imagination of the astrologer. In this interpretation, not in his mere observations, lay the secret of his success. Nor did his task cease with simply foretelling future events that were to happen in the life of the newly born infant. He must not only point out the dangers, but show the means whereby they could be averted, and his prophylactic measures, like his predictions, were alleged to be based on his reading of the stars.

But casting a horoscope at the time of births was, of course, only a small part of the astrologer's duty. His offices were sought by persons of all ages for predictions as to their futures, the movements of an enemy, where to find stolen goods, and a host of everyday occurrences. In such cases it is more than probable that the astrologers did very little consulting of the stars in making their predictions. They became expert physiognomists and excellent judges of human nature, and were thus able to foretell futures with the same shrewdness and by the same methods as the modern "mediums," palmists, and fortune-tellers. To strengthen belief in their powers, it became a common thing for some supposedly lost document of the astrologer to be mysteriously discovered after an important event, this document purporting to foretell this very event. It was also a common practice with astrologers to retain, or have access to, their original charts, cleverly altering them from time to time to fit conditions.

The dangers attendant upon astrology were of such a nature that the lot of the astrologer was likely to prove anything but an enviable one. As in the case of the alchemist, the greater the reputation of an astrologer the greater dangers he was likely to fall into. If he became so famous that he was employed by kings or noblemen, his too true or too false prophecies were likely to bring him into disrepute--even to endanger his life.

Throughout the dark age the astrologers flourished, but the sixteenth and seventeenth centuries were the golden age of these impostors. A skilful astrologer was as much an essential to the government as the highest official, and it would have been a bold monarch, indeed, who would undertake any expedition of importance unless sanctioned by the governing stars as interpreted by these officials.

It should not be understood, however, that belief in astrology died with the advent of the Copernican doctrine. It did become separated from astronomy very shortly after, to be sure, and undoubtedly among the scientists it lost much of its prestige. But it cannot be considered as entirely passed away, even to-day, and even if we leave out of consideration street-corner "astrologers" and fortune-tellers, whose signs may be seen in every large city, there still remains quite a large class of relatively intelligent people who believe in what they call "the science of astrology." Needless to say, such people are not found among the scientific thinkers; but it is significant that scarcely a year passes that some book or pamphlet is not published by some ardent believer in astrology, attempting to prove by the illogical dogmas characteristic of unscientific thinkers that astrology is a science. The arguments contained in these pamphlets are very much the same as those of the astrologers three hundred years ago, except that they lack the quaint form of wording which is one of the features that lends interest to the older documents. These pamphlets need not be taken seriously, but they are interesting as exemplifying how difficult it is, even in an age of science, to entirely stamp out firmly established superstitions. Here are some of the arguments advanced in defence of astrology, taken from a little brochure entitled "Astrology Vindicated," published in 1898: It will be found that a person born when the Sun is in twenty degrees Scorpio has the left ear as his exceptional feature and the nose (Sagittarius) bent towards the left ear. A person born when the Sun is in any of the latter degrees of Taurus, say the twenty-fifth degree, will have a small, sharp, weak chin, curved up towards Gemini, the two vertical lines on the upper lip."[4] The time was when science went out of its way to prove that such statements were untrue; but that time is past, and such writers are usually classed among those energetic but misguided persons who are unable to distinguish between logic and sophistry.

In England, from the time of Elizabeth to the reign of William and Mary, judicial astrology was at its height. After the great London fire, in 1666, a committee of the House of Commons publicly summoned the famous astrologer, Lilly, to come before Parliament and report to them on his alleged prediction of the calamity that had befallen the city. Lilly, for some reason best known to himself, denied having made such a prediction, being, as he explained, "more interested in determining affairs of much more importance to the future welfare of the country." Some of the explanations of his interpretations will suffice to show their absurdities, which, however, were by no means regarded as absurdities at that time, for Lilly was one of the greatest astrologers of his day. He said that in 1588 a prophecy had been printed in Greek characters which foretold exactly the troubles of England between the years 1641. and 1660. "And after him shall come a dreadful dead man," ran the prophecy, "and with him a royal G of the best blood in

the world, and he shall have the crown and shall set England on the right way and put out all heresies. His interpretation of this was that, "Monkery being extinguished above eighty or ninety years, and the Lord General's name being Monk, is the dead man. The royal G or C (it is gamma in the Greek, intending C in the Latin, being the third letter in the alphabet) is Charles II., who, for his extraction, may be said to be of the best blood of the world."[5]

This may be taken as a fair sample of Lilly's interpretations of astrological prophesies, but many of his own writings, while somewhat more definite and direct, are still left sufficiently vague to allow his skilful interpretations to set right an apparent mistake. One of his famous documents was "The Starry Messenger," a little pamphlet purporting to explain the phenomenon of a "strange apparition of three suns" that were seen in London on November 19, 1644---the anniversary of the birth of Charles I., then the reigning monarch. This phenomenon caused a great stir among the English astrologers, coming, as it did, at a time of great political disturbance. Prophecies were numerous, and Lilly's brochure is only one of many that appeared at that time, most of which, however, have been lost. Lilly, in his preface, says: "If there be any of so prevaricate a judgment as to think that the apparition of these three Suns doth intimate no Novelle thing to happen in our own Climate, where they were manifestly visible, I shall lament their indisposition, and conceive their brains to be shallow, and voyde of understanding humanity, or notice of common History."

Having thus forgiven his few doubting readers, who were by no means in the majority in his day, he takes up in review the records of the various appearances of three suns as they have occurred during the Christian era, showing how such phenomena have governed certain human events in a very definite manner. Some of these are worth recording.

"Anno 66. A comet was seen, and also three Suns: In which yeer, Florus President of the Jews was by them slain. Paul writes to Timothy. The Christians are warned by a divine Oracle, and depart out of Jerusalem. Boadice a British Queen, killeth seventy thousand Romans. The Nazareni, a scurvie Sect, begun, that boasted much of Revelations and Visions. About a year after Nero was proclaimed enemy to the State of Rome."

Again, "Anno 1157, in September, there were seen three Suns together, in as clear weather as could be: And a few days after, in the same month, three Moons, and, in the Moon that stood in the middle, a white Crosse. Sueno, King of Denmark, at a great Feast, killeth Canutus: Sueno is himself slain, in pursuit of Waldemar. The Order of Eremites, according to the rule of Saint Augustine, begun this year; and in the next, the Pope submits to the Emperour: (was not this miraculous?) Lombardy was also adjudged to the Emperour."

Continuing this list of peculiar phenomena he comes down to within a few years of his own time.

"Anno 1622, three Suns appeared at Heidelberg. The woful Calamities that have ever since fallen upon the Palatinate, we are all sensible of, and of the loss of it, for any thing I see, for ever, from the right Heir. Osman the great Turk is strangled that year; and Spinola besiegeth Bergen up Zoom, etc."

Fortified by the enumeration of these past events, he then proceeds to make his deductions. "Only this I must tell thee," he writes, "that the interpretation I write is, I conceive, grounded upon probable foundations; and who lives to see a few years over his head, will easily perceive I

have unfolded as much as was fit to discover, and that my judgment was not a mile and a half from truth."

There is a great significance in this "as much as was fit to discover"--a mysterious something that Lilly thinks it expedient not to divulge. But, nevertheless, one would imagine that he was about to make some definite prediction about Charles I., since these three suns appeared upon his birthday and surely must portend something concerning him. But after rambling on through many pages of dissertations upon planets and prophecies, he finally makes his own indefinite prediction.

"O all you Emperors, Kings, Princes, Rulers and Magistrates of Europe, this unaccustomed Apparition is like the Handwriting in Daniel to some of you; it premonisheth you, above all other people, to make your peace with God in time. You shall every one of you smart, and every one of you taste (none excepted) the heavie hand of God, who will strengthen your subjects with invincible courage to suppress your misgovernments and Oppressions in Church or Common-wealth; . . . Those words are general: a word for my own country of England. . . . Look to yourselves; here's some monstrous death towards you. But to whom? wilt thou say. Herein we consider the Signe, Lord thereof, and the House; The Sun signifies in that Royal Signe, great ones; the House signifies captivity, poison, Treachery: From which is derived thus much, That some very great man, what King, Prince, Duke, or the like, I really affirm I perfectly know not, shall, I say, come to some such untimely end."[6]

Here is shown a typical example of astrological prophecy, which seems to tell something or nothing, according to the point of view of the reader. According to a believer in astrology, after the execution of Charles I., five years later, this could be made to seem a direct and exact prophecy. For example, he says: "You Kings, Princes, etc., ... it premonisheth you ... to make your peace with God.... Look to yourselves; here's some monstrous death towards you. ... That some very great man, what King, Prince, . shall, I say, come to such untimely end."

But by the doubter the complete prophecy could be shown to be absolutely indefinite, and applicable as much to the king of France or Spain as to Charles I., or to any king in the future, since no definite time is stated. Furthermore, Lilly distinctly states, "What King, Prince, Duke, or the like, I really affirm I perfectly know not"--which last, at least, was a most truthful statement. The same ingenuity that made "Gen. Monk" the "dreadful dead man," could easily make such a prediction apply to the execution of Charles I. Such a definite statement that, on such and such a day a certain number of years in the future, the monarch of England would be beheaded--such an exact statement can scarcely be found in any of the works on astrology. It should be borne in mind, also, that Lilly was of the Cromwell party and opposed to the king.

After the death of Charles I., Lilly admitted that the monarch had given him a thousand pounds to cast his horoscope. "I advised him," says Lilly, "to proceed eastwards; he went west, and all the world knows the result." It is an unfortunate thing for the cause of astrology that Lilly failed to mention this until after the downfall of the monarch. In fact, the sudden death, or decline in power, of any monarch, even to-day, brings out the perennial post-mortem predictions of astrologers.

We see how Lilly, an opponent of the king, made his so-called prophecy of the disaster of the king and his army. At the same time another celebrated astrologer and rival of Lilly, George

Wharton, also made some predictions about the outcome of the eventful march from Oxford. Wharton, unlike Lilly, was a follower of the king's party, but that, of course, should have had no influence in his "scientific" reading of the stars. Wharton's predictions are much less verbose than Lilly's, much more explicit, and, incidentally, much more incorrect in this particular instance. "The Moon Lady of the 12," he wrote, "and moving betwixt the 8 degree, 34 min., and 21 degree, 26 min. of Aquarius, gives us to understand that His Majesty shall receive much contentment by certain Messages brought him from foreign parts; and that he shall receive some sudden and unexpected supply of . . . by the means of some that assimilate the condition of his Enemies: And withal this comfort; that His Majesty shall be exceeding successful in Besieging Towns, Castles, or Forts, and in persuing the enemy.

"Mars his Sextile to the Sun, Lord of the Ascendant (which happeneth the 18 day of May) will encourage our Soldiers to advance with much alacrity and cheerfulness of spirit; to show themselves gallant in the most dangerous attempt.... And now to sum up all: It is most apparent to every impartial and ingenuous judgment; That although His Majesty cannot expect to be secured from every trivial disaster that may befall his army, either by the too much Presumption, Ignorance, or Negligence of some particular Persons (which is frequently incident and unavoidable in the best of Armies), yet the several positions of the Heavens duly considered and compared among themselves, as well in the prefixed Scheme as at the Quarterly Ingresses, do generally render His Majesty and his whole Army unexpectedly victorious and successful in all his designs; Believe it (London), thy Miseries approach, they are like to be many, great, and grievous, and not to be diverted, unless thou seasonably crave Pardon of God for being Nurse to this present Rebellion, and speedily submit to thy Prince's Mercy; Which shall be the daily Prayer of Geo. Wharton."[7]

In the light of after events, it is probable that Wharton's stock as an astrologer was not greatly enhanced by this document, at least among members of the Royal family. Lilly's book, on the other hand, became a favorite with the Parliamentary army.

After the downfall and death of Napoleon there were unearthed many alleged authentic astrological documents foretelling his ruin. And on the death of George IV., in 1830, there appeared a document (unknown, as usual, until that time) purporting to foretell the death of the monarch to the day, and this without the astrologer knowing that his horoscope was being cast for a monarch. A full account of this prophecy is told, with full belief, by Roback, a nineteenth-century astrologer. He says:

"In the year 1828, a stranger of noble mien, advanced in life, but possessing the most bland manners, arrived at the abode of a celebrated astrologer in London," asking that the learned man foretell his future. "The astrologer complied with the request of the mysterious visitor, drew forth his tables, consulted his ephemeris, and cast the horoscope or celestial map for the hour and the moment of the inquiry, according to the established rules of his art.

"The elements of his calculation were adverse, and a feeling of gloom cast a shade of serious thought, if not dejection, over his countenance.

" 'You are of high rank,' said the astrologer, as he calculated and looked on the stranger, 'and of illustrious title.' The stranger made a graceful inclination of the head in token of acknowledgment of the complimentary remarks, and the astrologer proceeded with his mission.

"The celestial signs were ominous of calamity to the stranger, who, probably observing a sudden change in the countenance of the astrologer, eagerly inquired what evil or good fortune had been assigned him by the celestial orbs.

'To the first part of your inquiry,' said the astrologer, 'I can readily reply. You have been a favorite of fortune; her smiles on you have been abundant, her frowns but few; you have had, perhaps now possess, wealth and power; the impossibility of their accomplishment is the only limit to the fulfilment of your desires.' "

" 'You have spoken truly of the past,' said the stranger. 'I have full faith in your revelations of the future: what say you of my pilgrimage in this life--is it short or long?'

" 'I regret,' replied the astrologer, in answer to this inquiry, 'to be the herald of ill, though TRUE, fortune; your sojourn on earth will be short.'

" 'How short?' eagerly inquired the excited and anxious stranger.

" 'Give me a momentary truce,' said the astrologer; 'I will consult the horoscope, and may possibly find some mitigating circumstances.'

"Having cast his eyes over the celestial map, and paused for some moments, he surveyed the countenance of the stranger with great sympathy, and said, 'I am sorry that I can find no planetary influences that oppose your destiny--your death will take place in two years.'

"The event justified the astrologic prediction: George IV. died on May 18, 1830, exactly two years from the day on which he had visited the astrologer."[8]

This makes a very pretty story, but it hardly seems like occult insight that an astrologer should have been able to predict an early death of a man nearly seventy years old, or to have guessed that his well-groomed visitor "had, perhaps now possesses, wealth and power." Here again, however, the point of view of each individual plays the governing part in determining the importance of such a document. To the scientist it proves nothing; to the believer in astrology, everything. The significant thing is that it appeared shortly AFTER the death of the monarch.

On the Continent astrologers were even more in favor than in England. Charlemagne, and some of his immediate successors, to be sure, attempted to exterminate them, but such rulers as Louis XI. and Catherine de' Medici patronized and encouraged them, and it was many years after the time of Copernicus before their influence was entirely stamped out even in official life. There can be no question that what gave the color of truth to many of the predictions was the fact that so many of the prophecies of sudden deaths and great conflagrations were known to have come true--in many instances were made to come true by the astrologer himself. And so it happened that when the prediction of a great conflagration at a certain time culminated in such a conflagration, many times a second but less-important burning took place, in which the ambitious astrologer, or his followers, took a central part about a stake, being convicted of incendiarism, which they had committed in order that their prophecies might be fulfilled.

But, on the other hand, these predictions were sometimes turned to account by interested friends to warn certain persons of approaching dangers.

For example, a certain astrologer foretold the death of Prince Alexander de' Medici. He not only foretold the death, but described so minutely the circumstances that would attend it, and gave such a correct description of the assassin who should murder the prince, that he was at once suspected of having a hand in the assassination. It developed later, however, that such was probably not the case; but that some friend of Prince Alexander, knowing of the plot to take his life, had induced the astrologer to foretell the event in order that the prince might have timely warning and so elude the conspirators.

The cause of the decline of astrology was the growing prevalence of the new spirit of experimental science. Doubtless the most direct blow was dealt by the Copernican theory. So soon as this was established, the recognition of the earth's subordinate place in the universe must have made it difficult for astronomers to be longer deceived by such coincidences as had sufficed to convince the observers of a more credulous generation. Tycho Brahe was, perhaps, the last astronomer of prominence who was a conscientious practiser of the art of the astrologer.

VII. FROM PARACELSUS TO HARVEY

PARACELSUS

In the year 1526 there appeared a new lecturer on the platform at the University at Basel--a small, beardless, effeminate-looking person--who had already inflamed all Christendom with his peculiar philosophy, his revolutionary methods of treating diseases, and his unparalleled success in curing them. A man who was to be remembered in after-time by some as the father of modern chemistry and the founder of modern medicine; by others as madman, charlatan, impostor; and by still others as a combination of all these. This soft-cheeked, effeminate, woman-hating man, whose very sex has been questioned, was Theophrastus von Hohenheim, better known as Paracelsus (1493-1541).

To appreciate his work, something must be known of the life of the man. He was born near Maria-Einsiedeln, in Switzerland, the son of a poor physician of the place. He began the study of medicine under the instruction of his father, and later on came under the instruction of several learned churchmen. At the age of sixteen he entered the University of Basel, but, soon becoming disgusted with the philosophical teachings of the time, he quitted the scholarly world of dogmas and theories and went to live among the miners in the Tyrol, in order that he might study nature and men at first hand. Ordinary methods of study were thrown aside, and he devoted his time to personal observation--the only true means of gaining useful knowledge, as he preached and practised ever after. Here he became familiar with the art of mining, learned the physical properties of minerals, ores, and metals, and acquired some knowledge of mineral waters. More important still, he came in contact with such diseases, wounds, and injuries as miners are subject to, and he tried his hand at the practical treatment of these conditions, untrammelled by the traditions of a profession in which his training had been so scant.

Having acquired some empirical skill in treating diseases, Paracelsus set out wandering from place to place all over Europe, gathering practical information as he went, and learning more and more of the medicinal virtues of plants and minerals. His wanderings covered a period of about ten years, at the end of which time he returned to Basel, where he was soon invited to give a course of lectures in the university.

These lectures were revolutionary in two respects--they were given in German instead of time-honored Latin, and they were based upon personal experience rather than upon the works of such writers as Galen and Avicenna. Indeed, the iconoclastic teacher spoke with open disparagement of these revered masters, and openly upbraided his fellow-practitioners for following their tenets. Naturally such teaching raised a storm of opposition among the older physicians, but for a time the unparalleled success of Paracelsus in curing diseases more than offset his unpopularity. Gradually, however, his bitter tongue and his coarse personality rendered him so unpopular, even among his patients, that, finally, his liberty and life being jeopardized, he was obliged to flee from Basel, and became a wanderer. He lived for brief periods in Colmar, Nuremberg, Appenzell, Zurich, Pfeffers, Augsburg, and several other cities, until finally at Salzburg his eventful life came to a close in 1541. His enemies said that he had died in a tavern from the effects of a protracted debauch; his supporters maintained that he had been murdered at the instigation of rival physicians and apothecaries.

But the effects of his teachings had taken firm root, and continued to spread after his death. He had shown the fallibility of many of the teachings of the hitherto standard methods of treating diseases, and had demonstrated the advantages of independent reasoning based on observation. In his Magicum he gives his reasons for breaking with tradition. "I did," he says, "embrace at the beginning these doctrines, as my adversaries (followers of Galen) have done, but since I saw that from their procedures nothing resulted but death, murder, stranglings, anchylosed limbs, paralysis, and so forth, that they held most diseases incurable. . . . therefore have I quitted this wretched art, and sought for truth in any other direction. I asked myself if there were no such thing as a teacher in medicine, where could I learn this art best? Nowhere better than the open book of nature, written with God's own finger." We shall see, however, that this "book of nature" taught Paracelsus some very strange lessons. Modesty was not one of these. "Now at this time," he declares, "I, Theophrastus Paracelsus, Bombast, Monarch of the Arcana, was endowed by God with special gifts for this end, that every searcher after this supreme philosopher's work may be forced to imitate and to follow me, be he Italian, Pole, Gaul, German, or whatsoever or whosoever he be. Come hither after me, all ye philosophers, astronomers, and spagirists. . . . I will show and open to you ... this corporeal regeneration."[1]

Paracelsus based his medical teachings on four "pillars" --philosophy, astronomy, alchemy, and virtue of the physician--a strange-enough equipment surely, and yet, properly interpreted, not quite so anomalous as it seems at first blush. Philosophy was the "gate of medicine," whereby the physician entered rightly upon the true course of learning; astronomy, the study of the stars, was all-important because "they (the stars) caused disease by their exhalations, as, for instance, the sun by excessive heat"; alchemy, as he interpreted it, meant the improvement of natural substances for man's benefit; while virtue in the physician was necessary since "only the virtuous are permitted to penetrate into the innermost nature of man and the universe."

All his writings aim to promote progress in medicine, and to hold before the physician a grand ideal of his profession. In this his views are wide and far-reaching, based on the relationship which man bears to nature as a whole; but in his sweeping condemnations he not only rejected Galenic therapeutics and Galenic anatomy, but condemned dissections of any kind. He laid the cause of all diseases at the door of the three mystic elements--salt, sulphur, and mercury. In health he supposed these to be mingled in the body so as to be indistinguishable; a slight separation of them produced disease; and death he supposed to be the result of their complete

separation. The spiritual agencies of diseases, he said, had nothing to do with either angels or devils, but were the spirits of human beings.

He believed that all food contained poisons, and that the function of digestion was to separate the poisonous from the nutritious. In the stomach was an archaeus, or alchemist, whose duty was to make this separation. In digestive disorders the archaeus failed to do this, and the poisons thus gaining access to the system were "coagulated" and deposited in the joints and various other parts of the body. Thus the deposits in the kidneys and tartar on the teeth were formed; and the stony deposits of gout were particularly familiar examples of this. All this is visionary enough, yet it shows at least a groping after rational explanations of vital phenomena.

Like most others of his time, Paracelsus believed firmly in the doctrine of "signatures"--a belief that every organ and part of the body had a corresponding form in nature, whose function was to heal diseases of the organ it resembled. The vagaries of this peculiar doctrine are too numerous and complicated for lengthy discussion, and varied greatly from generation to generation. In general, however, the theory may be summed up in the words of Paracelsus: "As a woman is known by her shape, so are the medicines." Hence the physicians were constantly searching for some object of corresponding shape to an organ of the body. The most natural application of this doctrine would be the use of the organs of the lower animals for the treatment of the corresponding diseased organs in man. Thus diseases of the heart were to be treated with the hearts of animals, liver disorders with livers, and so on. But this apparently simple form of treatment had endless modifications and restrictions, for not all animals were useful. For example, it was useless to give the stomach of an ox in gastric diseases when the indication in such cases was really for the stomach of a rat. Nor were the organs of animals the only "signatures" in nature. Plants also played a very important role, and the herb-doctors devoted endless labor to searching for such plants. Thus the blood-root, with its red juice, was supposed to be useful in blood diseases, in stopping hemorrhage, or in subduing the redness of an inflammation.

Paracelsus's system of signatures, however, was so complicated by his theories of astronomy and alchemy that it is practically beyond comprehension. It is possible that he himself may have understood it, but it is improbable that any one else did--as shown by the endless discussions that have taken place about it. But with all the vagaries of his theories he was still rational in his applications, and he attacked to good purpose the complicated "shot-gun" prescriptions of his contemporaries, advocating more simple methods of treatment.

The ever-fascinating subject of electricity, or, more specifically, "magnetism," found great favor with him, and with properly adjusted magnets he claimed to be able to cure many diseases. In epilepsy and lockjaw, for example, one had but to fasten magnets to the four extremities of the body, and then, "when the proper medicines were given," the cure would be effected. The easy loop-hole for excusing failure on the ground of improper medicines is obvious, but Paracelsus declares that this one prescription is of more value than "all the humoralists have ever written or taught."

Since Paracelsus condemned the study of anatomy as useless, he quite naturally regarded surgery in the same light. In this he would have done far better to have studied some of his predecessors, such as Galen, Paul of Aegina, and Avicenna. But instead of "cutting men to pieces," he taught that surgeons would gain more by devoting their time to searching for the universal panacea

which would cure all diseases, surgical as well as medical. In this we detect a taint of the popular belief in the philosopher's stone and the magic elixir of life, his belief in which have been stoutly denied by some of his followers. He did admit, however, that one operation alone was perhaps permissible--lithotomy, or the "cutting for stone."

His influence upon medicine rests undoubtedly upon his revolutionary attitude, rather than on any great or new discoveries made by him. It is claimed by many that he brought prominently into use opium and mercury, and if this were indisputably proven his services to medicine could hardly be overestimated. Unfortunately, however, there are good grounds for doubting that he was particularly influential in reintroducing these medicines. His chief influence may perhaps be summed up in a single phrase--he overthrew old traditions.

To Paracelsus's endeavors, however, if not to the actual products of his work, is due the credit of setting in motion the chain of thought that developed finally into scientific chemistry. Nor can the ultimate aim of the modern chemist seek a higher object than that of this sixteenth-century alchemist, who taught that "true alchemy has but one aim and object, to extract the quintessence of things, and to prepare arcana, tinctures, and elixirs which may restore to man the health and soundness he has lost."

THE GREAT ANATOMISTS

About the beginning of the sixteenth century, while Paracelsus was scoffing at the study of anatomy as useless, and using his influence against it, there had already come upon the scene the first of the great anatomists whose work was to make the century conspicuous in that branch of medicine.

The young anatomist Charles etienne (1503-1564) made one of the first noteworthy discoveries, pointing out for the first time that the spinal cord contains a canal, continuous throughout its length. He also made other minor discoveries of some importance, but his researches were completely overshadowed and obscured by the work of a young Fleming who came upon the scene a few years later, and who shone with such brilliancy in the medical world that he obscured completely the work of his contemporary until many years later. This young physician, who was destined to lead such an eventful career and meet such an untimely end as a martyr to science, was Andrew Vesalius (1514-1564), who is called the "greatest of anatomists." At the time he came into the field medicine was struggling against the dominating Galenic teachings and the theories of Paracelsus, but perhaps most of all against the superstitions of the time. In France human dissections were attended with such dangers that the young Vesalius transferred his field of labors to Italy, where such investigations were covertly permitted, if not openly countenanced.

From the very start the young Fleming looked askance at the accepted teachings of the day, and began a series of independent investigations based upon his own observations. The results of these investigations he gave in a treatise on the subject which is regarded as the first comprehensive and systematic work on human anatomy. This remarkable work was published in the author's twenty-eighth or twenty-ninth year. Soon after this Vesalius was invited as imperial physician to the court of Emperor Charles V. He continued to act in the same capacity at the court of Philip II., after the abdication of his patron. But in spite of this royal favor there was at work a factor more powerful than the influence of the monarch himself--an instrument that did

so much to retard scientific progress, and by which so many lives were brought to a premature close.

Vesalius had received permission from the kinsmen of a certain grandee to perform an autopsy. While making his observations the heart of the outraged body was seen to palpitate--so at least it was reported. This was brought immediately to the attention of the Inquisition, and it was only by the intervention of the king himself that the anatomist escaped the usual fate of those accused by that tribunal. As it was, he was obliged to perform a pilgrimage to the Holy Land. While returning from this he was shipwrecked, and perished from hunger and exposure on the island of Zante.

At the very time when the anatomical writings of Vesalius were startling the medical world, there was living and working contemporaneously another great anatomist, Eustachius (died 1574), whose records of his anatomical investigations were ready for publication only nine years after the publication of the work of Vesalius. Owing to the unfortunate circumstances of the anatomist, however, they were never published during his lifetime--not, in fact, until 1714. When at last they were given to the world as Anatomical Engravings, they showed conclusively that Eustachius was equal, if not superior to Vesalius in his knowledge of anatomy. It has been said of this remarkable collection of engravings that if they had been published when they were made in the sixteenth century, anatomy would have been advanced by at least two centuries. But be this as it may, they certainly show that their author was a most careful dissector and observer.

Eustachius described accurately for the first time certain structures of the middle ear, and rediscovered the tube leading from the ear to the throat that bears his name. He also made careful studies of the teeth and the phenomena of first and second dentition. He was not baffled by the minuteness of structures and where he was unable to study them with the naked eye he used glasses for the purpose, and resorted to macerations and injections for the study of certain complicated structures. But while the fruit of his pen and pencil were lost for more than a century after his death, the effects of his teachings were not; and his two pupils, Fallopius and Columbus, are almost as well known to-day as their illustrious teacher. Columbus (1490-1559) did much in correcting the mistakes made in the anatomy of the bones as described by Vesalius. He also added much to the science by giving correct accounts of the shape and cavities of the heart, and made many other discoveries of minor importance. Fallopius (1523-1562) added considerably to the general knowledge of anatomy, made several discoveries in the anatomy of the ear, and also several organs in the abdominal cavity.

At this time a most vitally important controversy was in progress as to whether or not the veins of the bodies were supplied with valves, many anatomists being unable to find them. etienne had first described these structures, and Vesalius had confirmed his observations. It would seem as if there could be no difficulty in settling the question as to the fact of such valves being present in the vessels, for the demonstration is so simple that it is now made daily by medical students in all physiological laboratories and dissecting-rooms. But many of the great anatomists of the sixteenth century were unable to make this demonstration, even when it had been brought to their attention by such an authority as Vesalius. Fallopius, writing to Vesalius on the subject in 1562, declared that he was unable to find such valves. Others, however, such as Eustachius and Fabricius (1537-1619), were more successful, and found and described these structures. But the purpose served by these valves was entirely misinterpreted. That they act in preventing the

backward flow of the blood in the veins on its way to the heart, just as the valves of the heart itself prevent regurgitation, has been known since the time of Harvey; but the best interpretation that could be given at that time, even by such a man as Fabricius, was that they acted in retarding the flow of the blood as it comes from the heart, and thus prevent its too rapid distribution throughout the body. The fact that the blood might have been going towards the heart, instead of coming from it, seems never to have been considered seriously until demonstrated so conclusively by Harvey.

Of this important and remarkable controversy over the valves in veins, Withington has this to say: "This is truly a marvellous story. A great Galenic anatomist is first to give a full and correct description of the valves and their function, but fails to see that any modification of the old view as to the motion of the blood is required. Two able dissectors carefully test their action by experiment, and come to a result. the exact reverse of the truth. Urged by them, the two foremost anatomists of the age make a special search for valves and fail to find them. Finally, passing over lesser peculiarities, an aged and honorable professor, who has lived through all this, calmly asserts that no anatomist, ancient or modern, has ever mentioned valves in veins till he discovered them in 1574!"[2]

Among the anatomists who probably discovered these valves was Michael Servetus (1511-1553); but if this is somewhat in doubt, it is certain that he discovered and described the pulmonary circulation, and had a very clear idea of the process of respiration as carried on in the lungs. The description was contained in a famous document sent to Calvin in 1545--a document which the reformer carefully kept for seven years in order that he might make use of some of the heretical statements it contained to accomplish his desire of bringing its writer to the stake. The awful fate of Servetus, the interesting character of the man, and the fact that he came so near to anticipating the discoveries of Harvey make him one of the most interesting figures in medical history.

In this document which was sent to Calvin, Servetus rejected the doctrine of natural, vital, and animal spirits, as contained in the veins, arteries, and nerves respectively, and made the all-important statement that the fluids contained in veins and arteries are the same. He showed also that the blood is "purged from fume" and purified by respiration in the lungs, and declared that there is a new vessel in the lungs, "formed out of vein and artery." Even at the present day there is little to add to or change in this description of Servetus's.

By keeping this document, pregnant with advanced scientific views, from the world, and in the end only using it as a means of destroying its author, the great reformer showed the same jealousy in retarding scientific progress as had his arch-enemies of the Inquisition, at whose dictates Vesalius became a martyr to science, and in whose dungeons etienne perished.

THE COMING OF HARVEY

The time was ripe for the culminating discovery of the circulation of the blood; but as yet no one had determined the all-important fact that there are two currents of blood in the body, one going to the heart, one coming from it. The valves in the veins would seem to show conclusively that the venous current did not come from the heart, and surgeons must have observed thousands of times the every-day phenomenon of congested veins at the distal extremity of a limb around which a ligature or constriction of any kind had been placed, and the simultaneous depletion of

the vessels at the proximal points above the ligature. But it should be remembered that inductive science was in its infancy. This was the sixteenth, not the nineteenth century, and few men had learned to put implicit confidence in their observations and convictions when opposed to existing doctrines. The time was at hand, however, when such a man was to make his appearance, and, as in the case of so many revolutionary doctrines in science, this man was an Englishman. It remained for William Harvey (1578-1657) to solve the great mystery which had puzzled the medical world since the beginning of history; not only to solve it, but to prove his case so conclusively and so simply that for all time his little booklet must he handed down as one of the great masterpieces of lucid and almost faultless demonstration.

Harvey, the son of a prosperous Kentish yeoman, was born at Folkestone. His education was begun at the grammar-school of Canterbury, and later he became a pensioner of Caius College, Cambridge. Soon after taking his degree of B.A., at the age of nineteen, he decided upon the profession of medicine, and went to Padua as a pupil of Fabricius and Casserius. Returning to England at the age of twenty-four, he soon after (1609) obtained the reversion of the post of physician to St. Bartholomew's Hospital, his application being supported by James I. himself. Even at this time he was a popular physician, counting among his patients such men as Francis Bacon. In 1618 he was appointed physician extraordinary to the king, and, a little later, physician in ordinary. He was in attendance upon Charles I. at the battle of Edgehill, in 1642, where, with the young Prince of Wales and the Duke of York, after seeking shelter under a hedge, he drew a book out of his pocket and, forgetful of the battle, became absorbed in study, until finally the cannon-balls from the enemy's artillery made him seek a more sheltered position.

On the fall of Charles I. he retired from practice, and lived in retirement with his brother. He was then well along in years, but still pursued his scientific researches with the same vigor as before, directing his attention chiefly to the study of embryology. On June 3, 1657, he was attacked by paralysis and died, in his eightieth year. He had lived to see his theory of the circulation accepted, several years before, by all the eminent anatomists of the civilized world.

A keenness in the observation of facts, characteristic of the mind of the man, had led Harvey to doubt the truth of existing doctrines as to the phenomena of the circulation. Galen had taught that "the arteries are filled, like bellows, because they are expanded," but Harvey thought that the action of spurting blood from a severed vessel disproved this. For the spurting was remittant, "now with greater, now with less impetus," and its greater force always corresponded to the expansion (diastole), not the contraction (systole) of the vessel. Furthermore, it was evident that contraction of the heart and the arteries was not simultaneous, as was commonly taught, because in that case there would be no marked propulsion of the blood in any direction; and there was no gainsaying the fact that the blood was forcibly propelled in a definite direction, and that direction away from the heart.

Harvey's investigations led him to doubt also the accepted theory that there was a porosity in the septum of tissue that divides the two ventricles of the heart. It seemed unreasonable to suppose that a thick fluid like the blood could find its way through pores so small that they could not be demonstrated by any means devised by man. In evidence that there could be no such openings he pointed out that, since the two ventricles contract at the same time, this process would impede rather than facilitate such an intra-ventricular passage of blood. But what seemed the most conclusive proof of all was the fact that in the foetus there existed a demonstrable opening

between the two ventricles, and yet this is closed in the fully developed heart. Why should Nature, if she intended that blood should pass between the two cavities, choose to close this opening and substitute microscopic openings in place of it? It would surely seem more reasonable to have the small perforations in the thin, easily permeable membrane of the foetus, and the opening in the adult heart, rather than the reverse. From all this Harvey drew his correct conclusions, declaring earnestly, "By Hercules, there ARE no such porosities, and they cannot be demonstrated."

Having convinced himself that no intra-ventricular opening existed, he proceeded to study the action of the heart itself, untrammelled by too much faith in established theories, and, as yet, with no theory of his own. He soon discovered that the commonly accepted theory of the heart striking against the chest-wall during the period of relaxation was entirely wrong, and that its action was exactly the reverse of this, the heart striking the chest-wall during contraction. Having thus disproved the accepted theory concerning the heart's action, he took up the subject of the action of arteries, and soon was able to demonstrate by vivisection that the contraction of the arteries was not simultaneous with contractions of the heart. His experiments demonstrated that these vessels were simply elastic tubes whose pulsations were "nothing else than the impulse of the blood within them." The reason that the arterial pulsation was not simultaneous with the heart-beat he found to be because of the time required to carry the impulse along the tube,

By a series of further careful examinations and experiments, which are too extended to be given here, he was soon able further to demonstrate the action and course of the blood during the contractions of the heart. His explanations were practically the same as those given to-day--first the contraction of the auricle, sending blood into the ventricle; then ventricular contraction, making the pulse, and sending the blood into the arteries. He had thus demonstrated what had not been generally accepted before, that the heart was an organ for the propulsion of blood. To make such a statement to-day seems not unlike the sober announcement that the earth is round or that the sun does not revolve about it. Before Harvey's time, however, it was considered as an organ that was "in some mysterious way the source of vitality and warmth, as an animated crucible for the concoction of blood and the generation of vital spirits."[3]

In watching the rapid and ceaseless contractions of the heart, Harvey was impressed with the fact that, even if a very small amount of blood was sent out at each pulsation, an enormous quantity must pass through the organ in a day, or even in an hour. Estimating the size of the cavities of the heart, and noting that at least a drachm must be sent out with each pulsation, it was evident that the two thousand beats given by a very slow human heart in an hour must send out some forty pounds of blood--more than twice the amount in the entire body. The question was, what became of it all? For it should be remembered that the return of the blood by the veins was unknown, and nothing like a "circulation" more than vaguely conceived even by Harvey himself. Once it could be shown that the veins were constantly returning blood to the heart, the discovery that the blood in some way passes from the arteries to the veins was only a short step. Harvey, by resorting to vivisections of lower animals and reptiles, soon demonstrated beyond question the fact that the veins do carry the return blood. "But this, in particular, can be shown clearer than daylight," says Harvey. "The vena cava enters the heart at an inferior portion, while the artery passes out above. Now if the vena cava be taken up with forceps or the thumb and finger, and the course of the blood intercepted for some distance below the heart, you will at once see it almost emptied between the fingers and the heart, the blood being exhausted by the heart's pulsation, the heart at

the same time becoming much paler even in its dilatation, smaller in size, owing to the deficiency of blood, and at length languid in pulsation, as if about to die. On the other hand, when you release the vein the heart immediately regains its color and dimensions. After that, if you leave the vein free and tie and compress the arteries at some distance from the heart, you will see, on the contrary, their included portion grow excessively turgid, the heart becoming so beyond measure, assuming a dark-red color, even to lividity, and at length so overloaded with blood as to seem in danger of suffocation; but when the obstruction is removed it returns to its normal condition, in size, color, and movement."[4]

This conclusive demonstration that the veins return the blood to the heart must have been most impressive to Harvey, who had been taught to believe that the blood current in the veins pursued an opposite course, and must have tended to shake his faith in all existing doctrines of the day.

His next step was the natural one of demonstrating that the blood passes from the arteries to the veins. He demonstrated conclusively that this did occur, but for once his rejection of the ancient writers and one modern one was a mistake. For Galen had taught, and had attempted to demonstrate, that there are sets of minute vessels connecting the arteries and the veins; and Servetus had shown that there must be such vessels, at least in the lungs.

However, the little flaw in the otherwise complete demonstration of Harvey detracts nothing from the main issue at stake. It was for others who followed to show just how these small vessels acted in effecting the transfer of the blood from artery to vein, and the grand general statement that such a transfer does take place was, after all, the all-important one, and the exact method of how it takes place a detail. Harvey's experiments to demonstrate that the blood passes from the arteries to the veins are so simply and concisely stated that they may best be given in his own words.

"I have here to cite certain experiments," he wrote, "from which it seems obvious that the blood enters a limb by the arteries, and returns from it by the veins; that the arteries are the vessels carrying the blood from the heart, and the veins the returning channels of the blood to the heart; that in the limbs and extreme parts of the body the blood passes either by anastomosis from the arteries into the veins, or immediately by the pores of the flesh, or in both ways, as has already been said in speaking of the passage of the blood through the lungs; whence it appears manifest that in the circuit the blood moves from thence hither, and hence thither; from the centre to the extremities, to wit, and from the extreme parts back again to the centre. Finally, upon grounds of circulation, with the same elements as before, it will be obvious that the quantity can neither be accounted for by the ingesta, nor yet be held necessary to nutrition.

"Now let any one make an experiment on the arm of a man, either using such a fillet as is employed in blood-letting or grasping the limb tightly with his hand, the best subject for it being one who is lean, and who has large veins, and the best time after exercise, when the body is warm, the pulse is full, and the blood carried in large quantities to the extremities, for all then is more conspicuous; under such circumstances let a ligature be thrown about the extremity and drawn as tightly as can be borne: it will first be perceived that beyond the ligature neither in the wrist nor anywhere else do the arteries pulsate, that at the same time immediately above the ligature the artery begins to rise higher at each diastole, to throb more violently, and to swell in its vicinity with a kind of tide, as if it strove to break through and overcome the obstacle to its current; the artery here, in short, appears as if it were permanently full. The hand under such

circumstances retains its natural color and appearances; in the course of time it begins to fall somewhat in temperature, indeed, but nothing is DRAWN into it.

"After the bandage has been kept on some short time in this way, let it be slackened a little, brought to the state or term of middling tightness which is used in bleeding, and it will be seen that the whole hand and arm will instantly become deeply suffused and distended, injected, gorged with blood, DRAWN, as it is said, by this middling ligature, without pain, or heat, or any horror of a vacuum, or any other cause yet indicated.

"As we have noted, in connection with the tight ligature, that the artery above the bandage was distended and pulsated, not below it, so, in the case of the moderately tight bandage, on the contrary, do we find that the veins below, never above, the fillet swell and become dilated, while the arteries shrink; and such is the degree of distention of the veins here that it is only very strong pressure that will force the blood beyond the fillet and cause any of the veins in the upper part of the arm to rise.

"From these facts it is easy for any careful observer to learn that the blood enters an extremity by the arteries; for when they are effectively compressed nothing is DRAWN to the member; the hand preserves its color; nothing flows into it, neither is it distended; but when the pressure is diminished, as it is with the bleeding fillet, it is manifest that the blood is instantly thrown in with force, for then the hand begins to swell; which is as much as to say that when the arteries pulsate the blood is flowing through them, as it is when the moderately tight ligature is applied; but when they do not pulsate, or when a tight ligature is used, they cease from transmitting anything; they are only distended above the part where the ligature is applied. The veins again being compressed, nothing can flow through them; the certain indication of which is that below the ligature they are much more tumid than above it, and than they usually appear when there is no bandage upon the arm.

"It therefore plainly appears that the ligature prevents the return of the blood through the veins to the parts above it, and maintains those beneath it in a state of permanent distention. But the arteries, in spite of the pressure, and under the force and impulse of the heart, send on the blood from the internal parts of the body to the parts beyond the bandage."[5]

This use of ligatures is very significant, because, as shown, a very tight ligature stops circulation in both arteries and veins, while a loose one, while checking the circulation in the veins, which lie nearer the surface and are not so directly influenced by the force of the heart, does not stop the passage of blood in the arteries, which are usually deeply imbedded in the tissues, and not so easily influenced by pressure from without.

The last step of Harvey's demonstration was to prove that the blood does flow along the veins to the heart, aided by the valves that had been the cause of so much discussion and dispute between the great sixteenth-century anatomists. Harvey not only demonstrated the presence of these valves, but showed conclusively, by simple experiments, what their function was, thus completing his demonstration of the phenomena of the circulation.

The final ocular demonstration of the passage of the blood from the arteries to the veins was not to be made until four years after Harvey's death. This process, which can be observed easily in the web of a frog's foot by the aid of a low-power lens, was first demonstrated by Marcello

Malpighi (1628-1694) in 1661. By the aid of a lens he first saw the small "capillary" vessels connecting the veins and arteries in a piece of dried lung. Taking his cue from this, he examined the lung of a turtle, and was able to see in it the passage of the corpuscles through these minute vessels, making their way along these previously unknown channels from the arteries into the veins on their journey back to the heart. Thus the work of Harvey, all but complete, was made absolutely entire by the great Italian. And all this in a single generation.

LEEUWENHOEK DISCOVERS BACTERIA

The seventeenth century was not to close, however, without another discovery in science, which, when applied to the causation of disease almost two centuries later, revolutionized therapeutics more completely than any one discovery. This was the discovery of microbes, by Antonius von Leeuwenhoek (1632-1723), in 1683. Von Leeuwenhoek discovered that "in the white matter between his teeth" there were millions of microscopic "animals"--more, in fact, than "there were human beings in the united Netherlands," and all "moving in the most delightful manner." There can be no question that he saw them, for we can recognize in his descriptions of these various forms of little "animals" the four principal forms of microbes--the long and short rods of bacilli and bacteria, the spheres of micrococci, and the corkscrew spirillum.

The presence of these microbes in his mouth greatly annoyed Antonius, and he tried various methods of getting rid of them, such as using vinegar and hot coffee. In doing this he little suspected that he was anticipating modern antiseptic surgery by a century and three-quarters, and to be attempting what antiseptic surgery is now able to accomplish. For the fundamental principle of antisepsis is the use of medicines for ridding wounds of similar microscopic organisms. Von Leenwenhoek was only temporarily successful in his attempts, however, and took occasion to communicate his discovery to the Royal Society of England, hoping that they would be "interested in this novelty." Probably they were, but not sufficiently so for any member to pursue any protracted investigations or reach any satisfactory conclusions, and the whole matter was practically forgotten until the middle of the nineteenth century.

VIII. MEDICINE IN THE SIXTEENTH AND SEVENTEENTH CENTURIES

Of the half-dozen surgeons who were prominent in the sixteenth century, Ambroise Pare (1517-1590), called the father of French surgery, is perhaps the most widely known. He rose from the position of a common barber to that of surgeon to three French monarchs, Henry II., Francis II., and Charles IX. Some of his mottoes are still first principles of the medical man. Among others are: "He who becomes a surgeon for the sake of money, and not for the sake of knowledge, will accomplish nothing"; and "A tried remedy is better than a newly invented." On his statue is his modest estimate of his work in caring for the wounded, "Je le pansay, Dieu le guarit"--I dressed him, God cured him.

It was in this dressing of wounds on the battlefield that he accidentally discovered how useless and harmful was the terribly painful treatment of applying boiling oil to gunshot wounds as advocated by John of Vigo. It happened that after a certain battle, where there was an unusually large number of casualties, Pare found, to his horror, that no more boiling oil was available for the surgeons, and that he should be obliged to dress the wounded by other simpler methods. To his amazement the results proved entirely satisfactory, and from that day he discarded the hot-oil treatment.

As Pare did not understand Latin he wrote his treatises in French, thus inaugurating a custom in France that was begun by Paracelsus in Germany half a century before. He reintroduced the use of the ligature in controlling hemorrhage, introduced the "figure of eight" suture in the operation for hare-lip, improved many of the medico-legal doctrines, and advanced the practice of surgery generally. He is credited with having successfully performed the operation for strangulated hernia, but he probably borrowed it from Peter Franco (1505-1570), who published an account of this operation in 1556. As this operation is considered by some the most important operation in surgery, its discoverer is entitled to more than passing notice, although he was despised and ignored by the surgeons of his time.

Franco was an illiterate travelling lithotomist--a class of itinerant physicians who were very generally frowned down by the regular practitioners of medicine. But Franco possessed such skill as an operator, and appears to have been so earnest in the pursuit of what he considered a legitimate calling, that he finally overcame the popular prejudice and became one of the salaried surgeons of the republic of Bern. He was the first surgeon to perform the suprapubic lithotomy operation--the removal of stone through the abdomen instead of through the perineum. His works, while written in an illiterate style, give the clearest descriptions of any of the early modern writers.

As the fame of Franco rests upon his operation for prolonging human life, so the fame of his Italian contemporary, Gaspar Tagliacozzi (1545-1599), rests upon his operation for increasing human comfort and happiness by restoring amputated noses. At the time in which he lived amputation of the nose was very common, partly from disease, but also because a certain pope had fixed the amputation of that member as the penalty for larceny. Tagliacozzi probably borrowed his operation from the East; but he was the first Western surgeon to perform it and describe it. So great was the fame of his operations that patients flocked to him from all over Europe, and each "went away with as many noses as he liked." Naturally, the man who directed his efforts to restoring structures that bad been removed by order of the Church was regarded in the light of a heretic by many theologians; and though he succeeded in cheating the stake or dungeon, and died a natural death, his body was finally cast out of the church in which it had been buried.

In the sixteenth century Germany produced a surgeon, Fabricius Hildanes (1560-1639), whose work compares favorably with that of Pare, and whose name would undoubtedly have been much better known had not the circumstances of the time in which he lived tended to obscure his merits. The blind followers of Paracelsus could see nothing outside the pale of their master's teachings, and the disastrous Thirty Years' War tended to obscure and retard all scientific advances in Germany. Unlike many of his fellow-surgeons, Hildanes was well versed in Latin and Greek; and, contrary to the teachings of Paracelsus, he laid particular stress upon the necessity of the surgeon having a thorough knowledge of anatomy. He had a helpmate in his wife, who was also something of a surgeon, and she is credited with having first made use of the magnet in removing particles of metal from the eye. Hildanes tells of a certain man who had been injured by a small piece of steel in the cornea, which resisted all his efforts to remove it. After observing Hildanes' fruitless efforts for a time, it suddenly occurred to his wife to attempt to make the extraction with a piece of loadstone. While the physician held open the two lids, his wife attempted to withdraw the steel with the magnet held close to the cornea, and after several efforts she was successful--which Hildanes enumerates as one of the advantages of being a

married man.

Hildanes was particularly happy in his inventions of surgical instruments, many of which were designed for locating and removing the various missiles recently introduced in warfare.

The seventeenth century, which was such a flourishing one for anatomy and physiology, was not as productive of great surgeons or advances in surgery as the sixteenth had been or the eighteenth was to be. There was a gradual improvement all along the line, however, and much of the work begun by such surgeons as Pare and Hildanes was perfected or improved. Perhaps the most progressive surgeon of the century was an Englishman, Richard Wiseman (1625-1686), who, like Harvey, enjoyed royal favor, being in the service of all the Stuart kings. He was the first surgeon to advocate primary amputation, in gunshot wounds, of the limbs, and also to introduce the treatment of aneurisms by compression; but he is generally rated as a conservative operator, who favored medication rather than radical operations, where possible.

In Italy, Marcus Aurelius Severinus (1580-1656) and Peter Marchettis (1589-1675) were the leading surgeons of their nation. Like many of his predecessors in Europe, Severinus ran amuck with the Holy Inquisition and fled from Naples. But the waning of the powerful arm of the Church is shown by the fact that he was brought back by the unanimous voice of the grateful citizens, and lived in safety despite the frowns of the theologians.

The sixteenth century cannot be said to have added much of importance in the field of practical medicine, and, as in the preceding and succeeding centuries, was at best only struggling along in the wake of anatomy, physiology, and surgery. In the seventeenth century, however, at least one discovery in therapeutics was made that has been an inestimable boon to humanity ever since. This was the introduction of cinchona bark (from which quinine is obtained) in 1640. But this century was productive of many medical SYSTEMS, and could boast of many great names among the medical profession, and, on the whole, made considerably more progress than the preceding century.

Of the founders of medical systems, one of the most widely known is Jan Baptista van Helmont (1578-1644), an eccentric genius who constructed a system of medicine of his own and for a time exerted considerable influence. But in the end his system was destined to pass out of existence, not very long after the death of its author. Van Helmont was not only a physician, but was master of all the other branches of learning of the time, taking up the study of medicine and chemistry as an after-thought, but devoting himself to them with the greatest enthusiasm once he had begun his investigations. His attitude towards existing doctrines was as revolutionary as that of Paracelsus, and he rejected the teachings of Galen and all the ancient writers, although retaining some of the views of Paracelsus. He modified the archaeus of Paracelsus, and added many complications to it. He believed the whole body to be controlled by an archaeus influus, the soul by the archaei insiti, and these in turn controlled by the central archeus. His system is too elaborate and complicated for full explanation, but its chief service to medicine was in introducing new chemical methods in the preparation of drugs. In this way he was indirectly connected with the establishment of the Iatrochemical school. It was he who first used the word "gas"--a word coined by him, along with many others that soon fell into disuse.

The principles of the Iatrochemical school were the use of chemical medicines, and a theory of pathology different from the prevailing "humoral" pathology. The founder of this school was

Sylvius (Franz de le Boe, 1614-1672), professor of medicine at Leyden. He attempted to establish a permanent system of medicine based on the newly discovered theory of the circulation and the new chemistry, but his name is remembered by medical men because of the fissure in the brain (fissure of Sylvius) that bears it. He laid great stress on the cause of fevers and other diseases as originating in the disturbances of the process of fermentation in the stomach. The doctrines of Sylvius spread widely over the continent, but were not generally accepted in England until modified by Thomas Willis (1622-1675), whose name, like that of Sylvius, is perpetuated by a structure in the brain named after him, the circle of Willis. Willis's descriptions of certain nervous diseases, and an account of diabetes, are the first recorded, and added materially to scientific medicine. These schools of medicine lasted until the end of the seventeenth century, when they were finally overthrown by Sydenham.

The Iatrophysical school (also called iatromathematical, iatromechanical, or physiatric) was founded on theories of physiology, probably by Borelli, of Naples (1608-1679), although Sanctorius; Sanctorius, a professor at Padua, was a precursor, if not directly interested in establishing it. Sanctorius discovered the fact that an "insensible perspiration" is being given off by the body continually, and was amazed to find that loss of weight in this way far exceeded the loss of weight by all other excretions of the body combined. He made this discovery by means of a peculiar weighing-machine to which a chair was attached, and in which he spent most of his time. Very naturally he overestimated the importance of this discovery, but it was, nevertheless, of great value in pointing out the hygienic importance of the care of the skin. He also introduced a thermometer which he advocated as valuable in cases of fever, but the instrument was probably not his own invention, but borrowed from his friend Galileo.

Harvey's discovery of the circulation of the blood laid the foundation of the Iatrophysical school by showing that this vital process was comparable to a hydraulic system. In his On the Motive of Animals, Borelli first attempted to account for the phenomena of life and diseases on these principles. The iatromechanics held that the great cause of disease is due to different states of elasticity of the solids of the body interfering with the movements of the fluids, which are themselves subject to changes in density, one or both of these conditions continuing to cause stagnation or congestion. The school thus founded by Borelli was the outcome of the unbounded enthusiasm, with its accompanying exaggeration of certain phenomena with the corresponding belittling of others that naturally follows such a revolutionary discovery as that of Harvey. Having such a founder as the brilliant Italian Borelli, it was given a sufficient impetus by his writings to carry it some distance before it finally collapsed. Some of the exaggerated mathematical calculations of Borelli himself are worth noting. Each heart-beat, as he calculated it, overcomes a resistance equal to one hundred and eighty thousand pounds;--the modern physiologist estimates its force at from five to nine ounces!

THOMAS SYDENHAM

But while the Continent was struggling with these illusive "systems," and dabbling in mystic theories that were to scarcely outlive the men who conceived

them, there appeared in England--the "land of common-sense," as a German scientist has called it--"a cool, clear, and unprejudiced spirit," who in the golden age of systems declined "to be like the man who builds the chambers of the upper story of his house before he had laid securely the foundation walls."[1] This man was Thomas Sydenham (1624-1689), who, while the great

Harvey was serving the king as surgeon, was fighting as a captain in the parliamentary army. Sydenham took for his guide the teachings of Hippocrates, modified to suit the advances that had been made in scientific knowledge since the days of the great Greek, and established, as a standard, observation and experience. He cared little for theory unless confirmed by practice, but took the Hippocratic view that nature cured diseases, assisted by the physician. He gave due credit, however, to the importance of the part played by the assistant. As he saw it, medicine could be advanced in three ways: (1) "By accurate descriptions or natural histories of diseases; (2) by establishing a fixed principle or method of treatment, founded upon experience; (3) by searching for specific remedies, which he believes must exist in considerable numbers, though he admits that the only one yet discovered is Peruvian bark."[2] As it happened, another equally specific remedy, mercury, when used in certain diseases, was already known to him, but he evidently did not recognize it as such.

The influence on future medicine of Sydenham's teachings was most pronounced, due mostly to his teaching of careful observation. To most physicians, however, he is now remembered chiefly for his introduction of the use of laudanum, still considered one of the most valuable remedies of modern pharmacopoeias. The German gives the honor of introducing this preparation to Paracelsus, but the English-speaking world will always believe that the credit should be given to Sydenham.

IX. PHILOSOPHER-SCIENTISTS AND NEW INSTITUTIONS OF LEARNING

We saw that in the old Greek days there was no sharp line of demarcation between the field of the philosopher and that of the scientist. In the Hellenistic epoch, however, knowledge became more specialized, and our recent chapters have shown us scientific investigators whose efforts were far enough removed from the intangibilities of the philosopher. It must not be overlooked, however, that even in the present epoch there were men whose intellectual efforts were primarily directed towards the subtleties of philosophy, yet who had also a penchant for strictly scientific imaginings, if not indeed for practical scientific experiments. At least three of these men were of sufficient importance in the history of the development of science to demand more than passing notice. These three are the Englishman Francis Bacon (1561-1626), the Frenchman Rene Descartes (1596-1650); and the German Gottfried Leibnitz (1646-1716). Bacon, as the earliest path-breaker, showed the way, theoretically at least, in which the sciences should be studied; Descartes, pursuing the methods pointed out by Bacon, carried the same line of abstract reason into practice as well; while Leibnitz, coming some years later, and having the advantage of the wisdom of his two great predecessors, was naturally influenced by both in his views of abstract scientific principles.

Bacon's career as a statesman and his faults and misfortunes as a man do not concern us here. Our interest in him begins with his entrance into Trinity College, Cambridge, where he took up the study of all the sciences taught there at that time. During the three years he became more and more convinced that science was not being studied in a profitable manner, until at last, at the end of his college course, he made ready to renounce the old Aristotelian methods of study and advance his theory of inductive study. For although he was a great admirer of Aristotle's work, he became convinced that his methods of approaching study were entirely wrong.

"The opinion of Aristotle," he says, in his De Argumentum Scientiarum, "seemeth to me a negligent opinion, that of those things which exist by nature nothing can be changed by custom;

using for example, that if a stone be thrown ten thousand times up it will not learn to ascend; and that by often seeing or hearing we do not learn to see or hear better. For though this principle be true in things wherein nature is peremptory (the reason whereof we cannot now stand to discuss), yet it is otherwise in things wherein nature admitteth a latitude. For he might see that a straight glove will come more easily on with use; and that a wand will by use bend otherwise than it grew; and that by use of the voice we speak louder and stronger; and that by use of enduring heat or cold we endure it the better, and the like; which latter sort have a nearer resemblance unto that subject of manners he handleth than those instances which he allegeth."[1]

These were his opinions, formed while a young man in college, repeated at intervals through his maturer years, and reiterated and emphasized in his old age. Masses of facts were to be obtained by observing nature at first hand, and from such accumulations of facts deductions were to be made. In short, reasoning was to be from the specific to the general, and not vice versa.

It was by his teachings alone that Bacon thus contributed to the foundation of modern science; and, while he was constantly thinking and writing on scientific subjects, he contributed little in the way of actual discoveries. "I only sound the clarion," he said, "but I enter not the battle."

The case of Descartes, however, is different. He both sounded the clarion and entered into the fight. He himself freely acknowledges his debt to Bacon for his teachings of inductive methods of study, but modern criticism places his work on the same plane as that of the great Englishman. "If you lay hold of any characteristic product of modern ways of thinking," says Huxley, "either in the region of philosophy or in that of science, you find the spirit of that thought, if not its form, has been present in the mind of the great Frenchman."[2]

Descartes, the son of a noble family of France, was educated by Jesuit teachers. Like Bacon, he very early conceived the idea that the methods of teaching and studying science were wrong, but be pondered the matter well into middle life before putting into writing his ideas of philosophy and science. Then, in his Discourse Touching the Method of Using One's Reason Rightly and of Seeking Scientific Truth, he pointed out the way of seeking after truth. His central idea in this was to emphasize the importance of DOUBT, and avoidance of accepting as truth anything that does not admit of absolute and unqualified proof. In reaching these conclusions he had before him the striking examples of scientific deductions by Galileo, and more recently the discovery of the circulation of the blood by Harvey. This last came as a revelation to scientists, reducing this seemingly occult process, as it did, to the field of mechanical phenomena. The same mechanical laws that governed the heavenly bodies, as shown by Galileo, governed the action of the human heart, and, for aught any one knew, every part of the body, and even the mind itself.

Having once conceived this idea, Descartes began a series of dissections and experiments upon the lower animals, to find, if possible, further proof of this general law. To him the human body was simply a machine, a complicated mechanism, whose functions were controlled just as any other piece of machinery. He compared the human body to complicated machinery run by waterfalls and complicated pipes. "The nerves of the machine which I am describing," he says, "may very well be compared to the pipes of these waterworks; its muscles and its tendons to the other various engines and springs which seem to move them; its animal spirits to the water which impels them, of which the heart is the fountain; while the cavities of the brain are the central office. Moreover, respiration and other such actions as are natural and usual in the body, and which depend on the course of the spirits, are like the movements of a clock, or a mill, which

may be kept up by the ordinary flow of water."[3]

In such passages as these Descartes anticipates the ideas of physiology of the present time. He believed that the functions are performed by the various organs of the bodies of animals and men as a mechanism, to which in man was added the soul. This soul he located in the pineal gland, a degenerate and presumably functionless little organ in the brain. For years Descartes's idea of the function of this gland was held by many physiologists, and it was only the introduction of modern high-power microscopy that reduced this also to a mere mechanism, and showed that it is apparently the remains of a Cyclopean eye once common to man's remote ancestors.

Descartes was the originator of a theory of the movements of the universe by a mechanical process--the Cartesian theory of vortices--which for several decades after its promulgation reigned supreme in science. It is the ingenuity of this theory, not the truth of its assertions, that still excites admiration, for it has long since been supplanted. It was certainly the best hitherto advanced--the best "that the observations of the age admitted," according to D'Alembert.

According to this theory the infinite universe is full of matter, there being no such thing as a vacuum. Matter, as Descartes believed, is uniform in character throughout the entire universe, and since motion cannot take place in any part of a space completely filled, without simultaneous movement in all other parts, there are constant more or less circular movements, vortices, or whirlpools of particles, varying, of course, in size and velocity. As a result of this circular movement the particles of matter tend to become globular from contact with one another. Two species of matter are thus formed, one larger and globular, which continue their circular motion with a constant tendency to fly from the centre of the axis of rotation, the other composed of the clippings resulting from the grinding process. These smaller "filings" from the main bodies, becoming smaller and smaller, gradually lose their velocity and accumulate in the centre of the vortex. This collection of the smaller matter in the centre of the vortex constitutes the sun or star, while the spherical particles propelled in straight lines from the centre towards the circumference of the vortex produce the phenomenon of light radiating from the central star. Thus this matter becomes the atmosphere revolving around the accumulation at the centre. But the small particles being constantly worn away from the revolving spherical particles in the vortex, become entangled in their passage, and when they reach the edge of the inner strata of solar dust they settle upon it and form what we call sun-spots. These are constantly dissolved and reformed, until sometimes they form a crust round the central nucleus.

As the expansive force of the star diminishes in the course of time, it is encroached upon by neighboring vortices. If the part of the encroaching star be of a less velocity than the star which it has swept up, it will presently lose its hold, and the smaller star pass out of range, becoming a comet. But if the velocity of the vortex into which the incrusted star settles be equivalent to that of the surrounded vortex, it will hold it as a captive, still revolving and "wrapt in its own firmament." Thus the several planets of our solar system have been captured and held by the sun-vortex, as have the moon and other satellites.

But although these new theories at first created great enthusiasm among all classes of philosophers and scientists, they soon came under the ban of the Church. While no actual harm came to Descartes himself, his writings were condemned by the Catholic and Protestant churches alike. The spirit of philosophical inquiry he had engendered, however, lived on, and is largely responsible for modern philosophy.

In many ways the life and works of Leibnitz remind us of Bacon rather than Descartes. His life was spent in filling high political positions, and his philosophical and scientific writings were by-paths of his fertile mind. He was a theoretical rather than a practical scientist, his contributions to science being in the nature of philosophical reasonings rather than practical demonstrations. Had he been able to withdraw from public life and devote himself to science alone, as Descartes did, he would undoubtedly have proved himself equally great as a practical worker. But during the time of his greatest activity in philosophical fields, between the years 1690 and 1716, he was all the time performing extraordinary active duties in entirely foreign fields. His work may be regarded, perhaps, as doing for Germany in particular what Bacon's did for England and the rest of the world in general.

Only a comparatively small part of his philosophical writings concern us here. According to his theory of the ultimate elements of the universe, the entire universe is composed of individual centres, or monads. To these monads he ascribed numberless qualities by which every phase of nature may be accounted. They were supposed by him to be percipient, self-acting beings, not under arbitrary control of the deity, and yet God himself was the original monad from which all the rest are generated. With this conception as a basis, Leibnitz deduced his doctrine of pre-established harmony, whereby the numerous independent substances composing the world are made to form one universe. He believed that by virtue of an inward energy monads develop themselves spontaneously, each being independent of every other. In short, each monad is a kind of deity in itself--a microcosm representing all the great features of the macrocosm.

It would be impossible clearly to estimate the precise value of the stimulative influence of these philosophers upon the scientific thought of their time. There was one way, however, in which their influence was made very tangible--namely, in the incentive they gave to the foundation of scientific societies.

SCIENTIFIC SOCIETIES

At the present time, when the elements of time and distance are practically eliminated in the propagation of news, and when cheap printing has minimized the difficulties of publishing scientific discoveries, it is difficult to understand the isolated position of the scientific investigation of the ages that preceded steam and electricity. Shut off from the world and completely out of touch with fellow-laborers perhaps only a few miles away, the investigators were naturally seriously handicapped; and inventions and discoveries were not made with the same rapidity that they would undoubtedly have been had the same men been receiving daily, weekly, or monthly communications from fellow-laborers all over the world, as they do to-day. Neither did they have the advantage of public or semi-public laboratories, where they were brought into contact with other men, from whom to gather fresh trains of thought and receive the stimulus of their successes or failures. In the natural course of events, however, neighbors who were interested in somewhat similar pursuits, not of the character of the rivalry of trade or commerce, would meet more or less frequently and discuss their progress. The mutual advantages of such intercourse would be at once appreciated; and it would be but a short step from the casual meeting of two neighborly scientists to the establishment of "societies," meeting at fixed times, and composed of members living within reasonable travelling distance. There would, perhaps, be the weekly or monthly meetings of men in a limited area; and as the natural outgrowth of these little local societies, with frequent meetings, would come the formation of

larger societies, meeting less often, where members travelled a considerable distance to attend. And, finally, with increased facilities for communication and travel, the great international societies of to-day would be produced--the natural outcome of the neighborly meetings of the primitive mediaeval investigators.

In Italy, at about the time of Galileo, several small societies were formed. One of the most important of these was the Lyncean Society, founded about the year 1611, Galileo himself being a member. This society was succeeded by the Accademia del Cimento, at Florence, in 1657, which for a time flourished, with such a famous scientist as Torricelli as one of its members.

In England an impetus seems to have been given by Sir Francis Bacon's writings in criticism and censure of the systern of teaching in colleges. It is supposed that his suggestions as to what should be the aims of a scientific society led eventually to the establishment of the Royal Society. He pointed out how little had really been accomplished by the existing institutions of learning in advancing science, and asserted that little good could ever come from them while their methods of teaching remained unchanged. He contended that the system which made the lectures and exercises of such a nature that no deviation from the established routine could be thought of was pernicious. But he showed that if any teacher had the temerity to turn from the traditional paths, the daring pioneer was likely to find insurmountable obstacles placed in the way of his advancement. The studies were "imprisoned" within the limits of a certain set of authors, and originality in thought or teaching was to be neither contemplated nor tolerated.

The words of Bacon, given in strong and unsparing terms of censure and condemnation, but nevertheless with perfect justification, soon bore fruit. As early as the year 1645 a small company of scientists had been in the habit of meeting at some place in London to discuss philosophical and scientific subjects for mental advancement. In 1648, owing to the political disturbances of the time, some of the members of these meetings removed to Oxford, among them Boyle, Wallis, and Wren, where the meetings were continued, as were also the meetings of those left in London. In 1662, however, when the political situation bad become more settled, these two bodies of men were united under a charter from Charles II., and Bacon's ideas were practically expressed in that learned body, the Royal Society of London. And it matters little that in some respects Bacon's views were not followed in the practical workings of the society, or that the division of labor in the early stages was somewhat different than at present. The aim of the society has always been one for the advancement of learning; and if Bacon himself could look over its records, he would surely have little fault to find with the aid it has given in carrying out his ideas for the promulgation of useful knowledge.

Ten years after the charter was granted to the Royal Society of London, Lord Bacon's words took practical effect in Germany, with the result that the Academia Naturae Curiosorum was founded, under the leadership of Professor J. C. Sturm. The early labors of this society were devoted to a repetition of the most notable experiments of the time, and the work of the embryo society was published in two volumes, in 1672 and 1685 respectively, which were practically text-books of the physics of the period. It was not until 1700 that Frederick I. founded the Royal Academy of Sciences at Berlin, after the elaborate plan of Leibnitz, who was himself the first president.

Perhaps the nearest realization of Bacon's ideal, however, is in the Royal Academy of Sciences at Paris, which was founded in 1666 under the administration of Colbert, during the reign of Louis XIV. This institution not only recognized independent members, but had besides twenty

pensionnaires who received salaries from the government. In this way a select body of scientists were enabled to pursue their investigations without being obliged to "give thought to the morrow" for their sustenance. In return they were to furnish the meetings with scientific memoirs, and once a year give an account of the work they were engaged upon. Thus a certain number of the brightest minds were encouraged to devote their entire time to scientific research, "delivered alike from the temptations of wealth or the embarrassments of poverty." That such a plan works well is amply attested by the results emanating from the French academy. Pensionnaires in various branches of science, however, either paid by the state or by learned societies, are no longer confined to France.

Among the other early scientific societies was the Imperial Academy of Sciences at St. Petersburg, projected by Peter the Great, and established by his widow, Catharine I., in 1725; and also the Royal Swedish Academy, incorporated in 1781, and counting among its early members such men as the celebrated Linnaeus. But after the first impulse had resulted in a few learned societies, their manifest advantage was so evident that additional numbers increased rapidly, until at present almost every branch of every science is represented by more or less important bodies; and these are, individually and collectively, adding to knowledge and stimulating interest in the many fields of science, thus vindicating Lord Bacon's asseverations that knowledge could be satisfactorily promulgated in this manner.

X. THE SUCCESSORS OF GALILEO IN PHYSICAL SCIENCE

We have now to witness the diversified efforts of a company of men who, working for the most part independently, greatly added to the data of the physical sciences--such men as Boyle, Huygens, Von Gericke, and Hooke. It will be found that the studies of these men covered the whole field of physical sciences as then understood--the field of so-called natural philosophy. We shall best treat these successors of Galileo and precursors of Newton somewhat biographically, pointing out the correspondences and differences between their various accomplishments as we proceed. It will be noted in due course that the work of some of them was anticipatory of great achievements of a later century.

ROBERT BOYLE (1627-1691)

Some of Robert Boyle's views as to the possible structure of atmospheric air will be considered a little farther on in this chapter, but for the moment we will take up the consideration of some of his experiments upon that as well as other gases. Boyle was always much interested in alchemy, and carried on extensive experiments in attempting to accomplish the transmutation of metals; but he did not confine himself to these experiments, devoting himself to researches in all the fields of natural philosophy. He was associated at Oxford with a company of scientists, including Wallis and Wren, who held meetings and made experiments together, these gatherings being the beginning, as mentioned a moment ago, of what finally became the Royal Society. It was during this residence at Oxford that many of his valuable researches upon air were made, and during this time be invented his air-pump, now exhibited in the Royal Society rooms at Burlington House.[1]

His experiments to prove the atmospheric pressure are most interesting and conclusive. "Having three small, round glass bubbles, blown at the flame of a lamp, about the size of hazel-nuts," he says, "each of them with a short, slender stem, by means whereof they were so exactly poised in

water that a very small change of weight would make them either emerge or sink; at a time when the atmosphere was of convenient weight, I put them into a wide-mouthed glass of common water, and leaving them in a quiet place, where they were frequently in my eye, I observed that sometimes they would be at the top of the water, and remain there for several days, or perhaps weeks, together, and sometimes fall to the bottom, and after having continued there for some time rise again. And sometimes they would rise or fall as the air was hot or cold."[2]

It was in the course of these experiments that the observations made by Boyle led to the invention of his "statical barometer," the mercurial barometer having been invented, as we have seen, by Torricelli, in 1643. In describing this invention he says: "Making choice of a large, thin, and light glass bubble, blown at the flame of a lamp, I counterpoised it with a metallic weight, in a pair of scales that were suspended in a frame, that would turn with the thirtieth part of a grain. Both the frame and the balance were then placed near a good barometer, whence I might learn the present weight of the atmosphere; when, though the scales were unable to show all the variations that appeared in the mercurial barometer, yet they gave notice of those that altered the height of the mercury half a quarter of an inch."[3] A fairly sensitive barometer, after all. This statical barometer suggested several useful applications to the fertile imagination of its inventor, among others the measuring of mountain-peaks, as with the mercurial barometer, the rarefication of the air at the top giving a definite ratio to the more condensed air in the valley.

Another of his experiments was made to discover the atmospheric pressure to the square inch. After considerable difficulty he determined that the relative weight of a cubic inch of water and mercury was about one to fourteen, and computing from other known weights he determined that "when a column of quicksilver thirty inches high is sustained in the barometer, as it frequently happens, a column of air that presses upon an inch square near the surface of the earth must weigh about fifteen avoirdupois pounds."[4] As the pressure of air at the sea-level is now estimated at 14.7304 pounds to the square inch, it will be seen that Boyle's calculation was not far wrong.

From his numerous experiments upon the air, Boyle was led to believe that there were many "latent qualities" due to substances contained in it that science had as yet been unable to fathom, believing that there is "not a more heterogeneous body in the world." He believed that contagious diseases were carried by the air, and suggested that eruptions of the earth, such as those made by earthquakes, might send up "venomous exhalations" that produced diseases. He suggested also that the air might play an important part in some processes of calcination, which, as we shall see, was proved to be true by Lavoisier late in the eighteenth century. Boyle's notions of the exact chemical action in these phenomena were of course vague and indefinite, but he had observed that some part was played by the air, and he was right in supposing that the air "may have a great share in varying the salts obtainable from calcined vitriol."[5]

Although he was himself such a painstaking observer of facts, he had the fault of his age of placing too much faith in hear-say evidence of untrained observers. Thus, from the numerous stories he heard concerning the growth of metals in previously exhausted mines, he believed that the air was responsible for producing this growth--in which he undoubtedly believed. The story of a tin-miner that, in his own time, after a lapse of only twenty-five years, a heap, of earth previously exhausted of its ore became again even more richly impregnated than before by lying exposed to the air, seems to have been believed by the philosopher.

As Boyle was an alchemist, and undoubtedly believed in the alchemic theory that metals have "spirits" and various other qualities that do not exist, it is not surprising that he was credulous in the matter of beliefs concerning peculiar phenomena exhibited by them. Furthermore, he undoubtedly fell into the error common to "specialists," or persons working for long periods of time on one subject--the error of over-enthusiasm in his subject. He had discovered so many remarkable qualities in the air that it is not surprising to find that he attributed to it many more that he could not demonstrate.

Boyle's work upon colors, although probably of less importance than his experiments and deductions upon air, show that he was in the van as far as the science of his day was concerned. As he points out, the schools of his time generally taught that "color is a penetrating quality, reaching to the innermost part of the substance," and, as an example of this, sealing-wax was cited, which could be broken into minute bits, each particle retaining the same color as its fellows or the original mass. To refute this theory, and to show instances to the contrary, Boyle, among other things, shows that various colors--blue, red, yellow--may be produced upon tempered steel, and yet the metal within "a hair's-breadth of its surface" have none of these colors. Therefore, he was led to believe that color, in opaque bodies at least, is superficial.

"But before we descend to a more particular consideration of our subject," he says, " 'tis proper to observe that colors may be regarded either as a quality residing in bodies to modify light after a particular manner, or else as light itself so modified as to strike upon the organs of sight, and cause the sensation we call color; and that this latter is the more proper acceptation of the word color will appear hereafter. And indeed it is the light itself, which after a certain manner, either mixed with shades or other-wise, strikes our eyes and immediately produces that motion in the organ which gives us the color of an object."[6]

In examining smooth and rough surfaces to determine the cause of their color, he made use of the microscope, and pointed out the very obvious example of the difference in color of a rough and a polished piece of the same block of stone. He used some striking illustrations of the effect of light and the position of the eye upon colors. "Thus the color of plush or velvet will appear various if you stroke part of it one way and part another, the posture of the particular threads in regard to the light, or the eye, being thereby varied. And 'tis observable that in a field of ripe corn, blown upon by the wind, there will appear waves of a color different from that of the rest of the corn, because the wind, by depressing some of the ears more than others, causes one to reflect more light from the lateral and strawy parts than another."[7] His work upon color, however, as upon light, was entirely overshadowed by the work of his great fellow-countryman Newton.

Boyle's work on electricity was a continuation of Gilbert's, to which he added several new facts. He added several substances to Gilbert's list of "electrics," experimented on smooth and rough surfaces in exciting of electricity, and made the important discovery that amber retained its attractive virtue after the friction that excited it bad ceased. "For the attrition having caused an intestine motion in its parts," he says, "the heat thereby excited ought not to cease as soon as ever the rubbing is over, but to continue capable of emitting effluvia for some time afterwards, longer or shorter according to the goodness of the electric and the degree of the commotion made; all which, joined together, may sometimes make the effect considerable; and by this means, on a warm day, I, with a certain body not bigger than a pea, but very vigorously attractive, moved a

steel needle, freely poised, about three minutes after I had left off rubbing it."[8]

MARIOTTE AND VON GUERICKE

Working contemporaneously with Boyle, and a man whose name is usually associated with his as the propounder of the law of density of gases, was Edme Mariotte (died 1684), a native of Burgundy. Mariotte demonstrated that but for the resistance of the atmosphere, all bodies, whether light or heavy, dense or thin, would fall with equal rapidity, and he proved this by the well-known "guinea-and-feather" experiment. Having exhausted the air from a long glass tube in which a guinea piece and a feather had been placed, he showed that in the vacuum thus formed they fell with equal rapidity as often as the tube was reversed. From his various experiments as to the pressure of the atmosphere he deduced the law that the density and elasticity of the atmosphere are precisely proportional to the compressing force (the law of Boyle and Mariotte). He also ascertained that air existed in a state of mechanical mixture with liquids, "existing between their particles in a state of condensation." He made many other experiments, especially on the collision of bodies, but his most important work was upon the atmosphere.

But meanwhile another contemporary of Boyle and Mariotte was interesting himself in the study of the atmosphere, and had made a wonderful invention and a most striking demonstration. This was Otto von Guericke (1602-1686), Burgomaster of Magdeburg, and councillor to his "most serene and potent Highness" the elector of that place. When not engrossed with the duties of public office, he devoted his time to the study of the sciences, particularly pneumatics and electricity, both then in their infancy. The discoveries of Galileo, Pascal, and Torricelli incited him to solve the problem of the creation of a vacuum--a desideratum since before the days of Aristotle. His first experiments were with a wooden pump and a barrel of water, but he soon found that with such porous material as wood a vacuum could not be created or maintained. He therefore made use of a globe of copper, with pump and stop-cock; and with this he was able to pump out air almost as easily as water. Thus, in 1650, the air-pump was invented. Continuing his experiments upon vacuums and atmospheric pressure with his newly discovered pump, he made some startling discoveries as to the enormous pressure exerted by the air.

It was not his intention, however, to demonstrate his newly acquired knowledge by words or theories alone, nor by mere laboratory experiments; but he chose instead an open field, to which were invited Emperor Ferdinand III., and all the princes of the Diet at Ratisbon. When they were assembled he produced two hollow brass hemispheres about two feet in diameter, and placing their exactly fitting surfaces together, proceeded to pump out the air from their hollow interior, thus causing them to stick together firmly in a most remarkable way, apparently without anything holding them. This of itself was strange enough; but now the worthy burgomaster produced teams of horses, and harnessing them to either side of the hemispheres, attempted to pull the adhering brasses apart. Five, ten, fifteen teams--thirty horses, in all--were attached; but pull and tug as they would they could not separate the firmly clasped hemispheres. The enormous pressure of the atmosphere had been most strikingly demonstrated.

But it is one thing to demonstrate, another to convince; and many of the good people of Magdeburg shook their heads over this "devil's contrivance," and predicted that Heaven would punish the Herr Burgomaster, as indeed it had once by striking his house with lightning and injuring some of his infernal contrivances. They predicted his future punishment, but they did not molest him, for to his fellow-citizens, who talked and laughed, drank and smoked with him, and

knew him for the honest citizen that he was, he did not seem bewitched at all. And so he lived and worked and added other facts to science, and his brass hemispheres were not destroyed by fanatical Inquisitors, but are still preserved in the royal library at Berlin.

In his experiments with his air-pump he discovered many things regarding the action of gases, among others, that animals cannot live in a vacuum. He invented the anemoscope and the air-balance, and being thus enabled to weight the air and note the changes that preceded storms and calms, he was able still further to dumfound his wondering fellow-Magde-burgers by more or less accurate predictions about the weather.

Von Guericke did not accept Gilbert's theory that the earth was a great magnet, but in his experiments along lines similar to those pursued by Gilbert, he not only invented the first electrical machine, but discovered electrical attraction and repulsion. The electrical machine which he invented consisted of a sphere of sulphur mounted on an iron axis to imitate the rotation of the earth, and which, when rubbed, manifested electrical reactions. When this globe was revolved and stroked with the dry hand it was found that it attached to it "all sorts of little fragments, like leaves of gold, silver, paper, etc." "Thus this globe," he says, "when brought rather near drops of water causes them to swell and puff up. It likewise attracts air, smoke, etc."[9] Before the time of Guericke's demonstrations, Cabaeus had noted that chaff leaped back from an "electric," but he did not interpret the phenomenon as electrical repulsion. Von Guericke, however, recognized it as such, and refers to it as what he calls "expulsive virtue." "Even expulsive virtue is seen in this globe," he says, "for it not only attracts, but also REPELS again from itself little bodies of this sort, nor does it receive them until they have touched something else." It will be observed from this that he was very close to discovering the discharge of the electrification of attracted bodies by contact with some other object, after which they are reattracted by the electric.

He performed a most interesting experiment with his sulphur globe and a feather, and in doing so came near anticipating Benjamin Franklin in his discovery of the effects of pointed conductors in drawing off the discharge. Having revolved and stroked his globe until it repelled a bit of down, he removed the globe from its rack and advancing it towards the now repellent down, drove it before him about the room. In this chase he observed that the down preferred to alight against "the points of any object whatsoever." He noticed that should the down chance to be driven within a few inches of a lighted candle, its attitude towards the globe suddenly changed, and instead of running away from it, it now "flew to it for protection" --the charge on the down having been dissipated by the hot air. He also noted that if one face of a feather had been first attracted and then repelled by the sulphur ball, that the surface so affected was always turned towards the globe; so that if the positions of the two were reversed, the sides of the feather reversed also.

Still another important discovery, that of electrical conduction, was made by Von Guericke. Until his discovery no one had observed the transference of electricity from one body to another, although Gilbert had some time before noted that a rod rendered magnetic at one end became so at the other. Von Guericke's experiments were made upon a linen thread with his sulphur globe, which, he says, "having been previously excited by rubbing, can exercise likewise its virtue through a linen thread an ell or more long, and there attract something." But this discovery, and his equally important one that the sulphur ball becomes luminous when rubbed, were practically

forgotten until again brought to notice by the discoveries of Francis Hauksbee and Stephen Gray early in the eighteenth century. From this we may gather that Von Guericke himself did not realize the import of his discoveries, for otherwise he would certainly have carried his investigations still further. But as it was he turned his attention to other fields of research.

ROBERT HOOKE

A slender, crooked, shrivelled-limbed, cantankerous little man, with dishevelled hair and haggard countenance, bad-tempered and irritable, penurious and dishonest, at least in his claims for priority in discoveries--this is the picture usually drawn, alike by friends and enemies, of Robert Hooke (1635-1703), a man with an almost unparalleled genius for scientific discoveries in almost all branches of science. History gives few examples so striking of a man whose really great achievements in science would alone have made his name immortal, and yet who had the pusillanimous spirit of a charlatan--an almost insane mania, as it seems--for claiming the credit of discoveries made by others. This attitude of mind can hardly be explained except as a mania: it is certainly more charitable so to regard it. For his own discoveries and inventions were so numerous that a few more or less would hardly have added to his fame, as his reputation as a philosopher was well established. Admiration for his ability and his philosophical knowledge must always be marred by the recollection of his arrogant claims to the discoveries of other philosophers.

It seems pretty definitely determined that Hooke should be credited with the invention of the balance-spring for regulating watches; but for a long time a heated controversy was waged between Hooke and Huygens as to who was the real inventor. It appears that Hooke conceived the idea of the balance-spring, while to Huygens belongs the credit of having adapted the COILED spring in a working model. He thus made practical Hooke's conception, which is without value except as applied by the coiled spring; but, nevertheless, the inventor, as well as the perfector, should receive credit. In this controversy, unlike many others, the blame cannot be laid at Hooke's door.

Hooke was the first curator of the Royal Society, and when anything was to be investigated, usually invented the mechanical devices for doing so. Astronomical apparatus, instruments for measuring specific weights, clocks and chronometers, methods of measuring the velocity of falling bodies, freezing and boiling points, strength of gunpowder, magnetic instruments--in short, all kinds of ingenious mechanical devices in all branches of science and mechanics. It was he who made the famous air-pump of Robert Boyle, based on Boyle's plans. Incidentally, Hooke claimed to be the inventor of the first air-pump himself, although this claim is now entirely discredited.

Within a period of two years he devised no less than thirty different methods of flying, all of which, of course, came to nothing, but go to show the fertile imagination of the man, and his tireless energy. He experimented with electricity and made some novel suggestions upon the difference between the electric spark and the glow, although on the whole his contributions in this field are unimportant. He also first pointed out that the motions of the heavenly bodies must be looked upon as a mechanical problem, and was almost within grasping distance of the exact theory of gravitation, himself originating the idea of making use of the pendulum in measuring gravity. Likewise, he first proposed the wave theory of light; although it was Huygens who established it on its present foundation.

Hooke published, among other things, a book of plates and descriptions of his Microscopical Observations, which gives an idea of the advance that had already been made in microscopy in his time. Two of these plates are given here, which, even in this age of microscopy, are both interesting and instructive. These plates are made from prints of Hooke's original copper plates, and show that excellent lenses were made even at that time. They illustrate, also, how much might have been accomplished in the field of medicine if more attention had been given to microscopy by physicians. Even a century later, had physicians made better use of their microscopes, they could hardly have overlooked such an easily found parasite as the itch mite, which is quite as easily detected as the cheese mite, pictured in Hooke's book.

In justice to Hooke, and in extenuation of his otherwise inexcusable peculiarities of mind, it should be remembered that for many years he suffered from a painful and wasting disease. This may have affected his mental equilibrium, without appreciably affecting his ingenuity. In his own time this condition would hardly have been considered a disease; but to-day, with our advanced ideas as to mental diseases, we should be more inclined to ascribe his unfortunate attitude of mind to a pathological condition, rather than to any manifestation of normal mentality. From this point of view his mental deformity seems not unlike that of Cavendish's, later, except that in the case of Cavendish it manifested itself as an abnormal sensitiveness instead of an abnormal irritability.

CHRISTIAN HUYGENS

If for nothing else, the world is indebted to the man who invented the pendulum clock, Christian Huygens (1629-1695), of the Hague, inventor, mathematician, mechanician, astronomer, and physicist. Huygens was the descendant of a noble and distinguished family, his father, Sir Constantine Huygens, being a well-known poet and diplomatist. Early in life young Huygens began his career in the legal profession, completing his education in the juridical school at Breda; but his taste for mathematics soon led him to neglect his legal studies, and his aptitude for scientific researches was so marked that Descartes predicted great things of him even while he was a mere tyro in the field of scientific investigation.

One of his first endeavors in science was to attempt an improvement of the telescope. Reflecting upon the process of making lenses then in vogue, young Huygens and his brother Constantine attempted a new method of grinding and polishing, whereby they overcame a great deal of the spherical and chromatic aberration. With this new telescope a much clearer field of vision was obtained, so much so that Huygens was able to detect, among other things, a hitherto unknown satellite of Saturn. It was these astronomical researches that led him to apply the pendulum to regulate the movements of clocks. The need for some more exact method of measuring time in his observations of the stars was keenly felt by the young astronomer, and after several experiments along different lines, Huygens hit upon the use of a swinging weight; and in 1656 made his invention of the pendulum clock. The year following, his clock was presented to the states-general. Accuracy as to time is absolutely essential in astronomy, but until the invention of Huygens's clock there was no precise, nor even approximately precise, means of measuring short intervals.

Huygens was one of the first to adapt the micrometer to the telescope--a mechanical device on which all the nice determination of minute distances depends. He also took up the controversy against Hooke as to the superiority of telescopic over plain sights to quadrants, Hooke

contending in favor of the plain. In this controversy, the subject of which attracted wide attention, Huygens was completely victorious; and Hooke, being unable to refute Huygens's arguments, exhibited such irritability that he increased his already general unpopularity. All of the arguments for and against the telescope sight are too numerous to be given here. In contending in its favor Huygens pointed out that the unaided eye is unable to appreciate an angular space in the sky less than about thirty seconds. Even in the best quadrant with a plain sight, therefore, the altitude must be uncertain by that quantity. If in place of the plain sight a telescope is substituted, even if it magnify only thirty times, it will enable the observer to fix the position to one second, with progressively increased accuracy as the magnifying power of the telescope is increased. This was only one of the many telling arguments advanced by Huygens.

In the field of optics, also, Huygens has added considerably to science, and his work, Dioptrics, is said to have been a favorite book with Newton. During the later part of his life, however, Huygens again devoted himself to inventing and constructing telescopes, grinding the lenses, and devising, if not actually making, the frame for holding them. These telescopes were of enormous lengths, three of his object-glasses, now in possession of the Royal Society, being of 123, 180, and 210 feet focal length respectively. Such instruments, if constructed in the ordinary form of the long tube, were very unmanageable, and to obviate this Huygens adopted the plan of dispensing with the tube altogether, mounting his lenses on long poles manipulated by machinery. Even these were unwieldy enough, but the difficulties of manipulation were fully compensated by the results obtained.

It had been discovered, among other things, that in oblique refraction light is separated into colors. Therefore, any small portion of the convex lens of the telescope, being a prism, the rays proceed to the focus, separated into prismatic colors, which make the image thus formed edged with a fringe of color and indistinct. But, fortunately for the early telescope makers, the degree of this aberration is independent of the focal length of the lens; so that, by increasing this focal length and using the appropriate eye-piece, the image can be greatly magnified, while the fringe of colors remains about the same as when a less powerful lens is used. Hence the advantage of Huygens's long telescope. He did not confine his efforts to simply lengthening the focal length of his telescopes, however, but also added to their efficiency by inventing an almost perfect achromatic eye-piece.

In 1663 he was elected a fellow of the Royal Society of London, and in 1669 he gave to that body a concise statement of the laws governing the collision of elastic bodies. Although the same views had been given by Wallis and Wren a few weeks earlier, there is no doubt that Huygens's views were reached independently; and it is probable that he had arrived at his conclusions several years before. In the Philosophical Transactions for 1669 it is recorded that the society, being interested in the laws of the principles of motion, a request was made that M. Huygens, Dr. Wallis, and Sir Christopher Wren submit their views on the subject. Wallis submitted his paper first, November 15, 1668. A month later, December 17th, Wren imparted to the society his laws as to the nature of the collision of bodies. And a few days later, January 5, 1669, Huygens sent in his "Rules Concerning the Motion of Bodies after Mutual Impulse." Although Huygens's report was received last, he was anticipated by such a brief space of time, and his views are so clearly stated--on the whole rather more so than those of the other two--that we give them in part here:

"1. If a hard body should strike against a body equally hard at rest, after contact the former will

rest and the latter acquire a velocity equal to that of the moving body.

"2. But if that other equal body be likewise in motion, and moving in the same direction, after contact they will move with reciprocal velocities.

"3. A body, however great, is moved by a body however small impelled with any velocity whatsoever.

"5. The quantity of motion of two bodies may be either increased or diminished by their shock; but the same quantity towards the same part remains, after subtracting the quantity of the contrary motion.

"6. The sum of the products arising from multiplying the mass of any hard body into the squares of its velocity is the same both before and after the stroke.

"7. A hard body at rest will receive a greater quantity of motion from another hard body, either greater or less than itself, by the interposition of any third body of a mean quantity, than if it was immediately struck by the body itself; and if the interposing body be a mean proportional between the other two, its action upon the quiescent body will be the greatest of all."[10]

This was only one of several interesting and important communications sent to the Royal Society during his lifetime. One of these was a report on what he calls "Pneumatical Experiments." "Upon including in a vacuum an insect resembling a beetle, but somewhat larger," he says, "when it seemed to be dead, the air was readmitted, and soon after it revived; putting it again in the vacuum, and leaving it for an hour, after which the air was readmitted, it was observed that the insect required a longer time to recover; including it the third time for two days, after which the air was admitted, it was ten hours before it began to stir; but, putting it in a fourth time, for eight days, it never afterwards recovered.... Several birds, rats, mice, rabbits, and cats were killed in a vacuum, but if the air was admitted before the engine was quite exhausted some of them would recover; yet none revived that had been in a perfect vacuum.... Upon putting the weight of eighteen grains of powder with a gauge into a receiver that held several pounds of water, and firing the powder, it raised the mercury an inch and a half; from which it appears that there is one-fifth of air in gunpowder, upon the supposition that air is about one thousand times lighter than water; for in this experiment the mercury rose to the eighteenth part of the height at which the air commonly sustains it, and consequently the weight of eighteen grains of powder yielded air enough to fill the eighteenth part of a receiver that contained seven pounds of water; now this eighteenth part contains forty-nine drachms of water; wherefore the air, that takes up an equal space, being a thousand times lighter, weighs one-thousandth part of forty-nine drachms, which is more than three grains and a half; it follows, therefore, that the weight of eighteen grains of powder contains more than three and a half of air, which is about one-fifth of eighteen grains...."

From 1665 to 1681, accepting the tempting offer made him through Colbert, by Louis XIV., Huygens pursued his studies at the Bibliotheque du Roi as a resident of France. Here he published his Horologium Oscillatorium, dedicated to the king, containing, among other things, his solution of the problem of the "centre of oscillation." This in itself was an important step in the history of mechanics. Assuming as true that the centre of gravity of any number of interdependent bodies cannot rise higher than the point from which it falls, he reached correct conclusions as to the general principle of the conservation of vis viva, although he did not

actually prove his conclusions. This was the first attempt to deal with the dynamics of a system. In this work, also, was the true determination of the relation between the length of a pendulum and the time of its oscillation.

In 1681 he returned to Holland, influenced, it is believed, by the attitude that was being taken in France against his religion. Here he continued his investigations, built his immense telescopes, and, among other things, discovered "polarization," which is recorded in Traite de la Lumiere, published at Leyden in 1690. Five years later he died, bequeathing his manuscripts to the University of Leyden. It is interesting to note that he never accepted Newton's theory of gravitation as a universal property of matter.

XI. NEWTON AND THE COMPOSITION OF LIGHT

Galileo, that giant in physical science of the early seventeenth century, died in 1642. On Christmas day of the same year there was born in England another intellectual giant who was destined to carry forward the work of Copernicus, Kepler, and Galileo to a marvellous consummation through the discovery of the great unifying law in accordance with which the planetary motions are performed. We refer, of course, to the greatest of English physical scientists, Isaac Newton, the Shakespeare of the scientific world. Born thus before the middle of the seventeenth century, Newton lived beyond the first quarter of the eighteenth (1727). For the last forty years of that period his was the dominating scientific personality of the world. With full propriety that time has been spoken of as the "Age of Newton."

Yet the man who was to achieve such distinction gave no early premonition of future greatness. He was a sickly child from birth, and a boy of little seeming promise. He was an indifferent student, yet, on the other hand, he cared little for the common amusements of boyhood. He early exhibited, however, a taste for mechanical contrivances, and spent much time in devising windmills, water-clocks, sun-dials, and kites. While other boys were interested only in having kites that would fly, Newton--at least so the stories of a later time would have us understand--cared more for the investigation of the seeming principles involved, or for testing the best methods of attaching the strings, or the best materials to be used in construction.

Meanwhile the future philosopher was acquiring a taste for reading and study, delving into old volumes whenever he found an opportunity. These habits convinced his relatives that it was useless to attempt to make a farmer of the youth, as had been their intention. He was therefore sent back to school, and in the summer of 1661 he matriculated at Trinity College, Cambridge. Even at college Newton seems to have shown no unusual mental capacity, and in 1664, when examined for a scholarship by Dr. Barrow, that gentleman is said to have formed a poor opinion of the applicant. It is said that the knowledge of the estimate placed upon his abilities by his instructor piqued Newton, and led him to take up in earnest the mathematical studies in which he afterwards attained such distinction. The study of Euclid and Descartes's "Geometry" roused in him a latent interest in mathematics, and from that time forward his investigations were carried on with enthusiasm. In 1667 he was elected Fellow of Trinity College, taking the degree of M.A. the following spring.

It will thus appear that Newton's boyhood and early manhood were passed during that troublous time in British political annals which saw the overthrow of Charles I., the autocracy of Cromwell, and the eventual restoration of the Stuarts. His maturer years witnessed the overthrow

of the last Stuart and the reign of the Dutchman, William of Orange. In his old age he saw the first of the Hanoverians mount the throne of England. Within a decade of his death such scientific path-finders as Cavendish, Black, and Priestley were born--men who lived on to the close of the eighteenth century. In a full sense, then, the age of Newton bridges the gap from that early time of scientific awakening under Kepler and Galileo to the time which we of the twentieth century think of as essentially modern.

THE COMPOSITION OF WHITE LIGHT

In December, 1672, Newton was elected a Fellow of the Royal Society, and at this meeting a paper describing his invention of the refracting telescope was read. A few days later he wrote to the secretary, making some inquiries as to the weekly meetings of the society, and intimating that he had an account of an interesting discovery that he wished to lay before the society. When this communication was made public, it proved to be an explanation of the discovery of the composition of white light. We have seen that the question as to the nature of color had commanded the attention of such investigators as Huygens, but that no very satisfactory solution of the question had been attained. Newton proved by demonstrative experiments that white light is composed of the blending of the rays of diverse colors, and that the color that we ascribe to any object is merely due to the fact that the object in question reflects rays of that color, absorbing the rest. That white light is really made up of many colors blended would seem incredible had not the experiments by which this composition is demonstrated become familiar to every one. The experiments were absolutely novel when Newton brought them forward, and his demonstration of the composition of light was one of the most striking expositions ever brought to the attention of the Royal Society. It is hardly necessary to add that, notwithstanding the conclusive character of Newton's work, his explanations did not for a long time meet with general acceptance.

Newton was led to his discovery by some experiments made with an ordinary glass prism applied to a hole in the shutter of a darkened room, the refracted rays of the sunlight being received upon the opposite wall and forming there the familiar spectrum. "It was a very pleasing diversion," he wrote, "to view the vivid and intense colors produced thereby; and after a time, applying myself to consider them very circumspectly, I became surprised to see them in varying form, which, according to the received laws of refraction, I expected should have been circular. They were terminated at the sides with straight lines, but at the ends the decay of light was so gradual that it was difficult to determine justly what was their figure, yet they seemed semicircular.

"Comparing the length of this colored spectrum with its breadth, I found it almost five times greater; a disproportion so extravagant that it excited me to a more than ordinary curiosity of examining from whence it might proceed. I could scarce think that the various thicknesses of the glass, or the termination with shadow or darkness, could have any influence on light to produce such an effect; yet I thought it not amiss, first, to examine those circumstances, and so tried what would happen by transmitting light through parts of the glass of divers thickness, or through holes in the window of divers bigness, or by setting the prism without so that the light might pass through it and be refracted before it was transmitted through the hole; but I found none of those circumstances material. The fashion of the colors was in all these cases the same.

"Then I suspected whether by any unevenness of the glass or other contingent irregularity these

colors might be thus dilated. And to try this I took another prism like the former, and so placed it that the light, passing through them both, might be refracted contrary ways, and so by the latter returned into that course from which the former diverted it. For, by this means, I thought, the regular effects of the first prism would be destroyed by the second prism, but the irregular ones more augmented by the multiplicity of refractions. The event was that the light, which by the first prism was diffused into an oblong form, was by the second reduced into an orbicular one with as much regularity as when it did not all pass through them. So that, whatever was the cause of that length, 'twas not any contingent irregularity.

"I then proceeded to examine more critically what might be effected by the difference of the incidence of rays coming from divers parts of the sun; and to that end measured the several lines and angles belonging to the image. Its distance from the hole or prism was 22 feet; its utmost length 13 1/4 inches; its breadth 2 5/8; the diameter of the hole 1/4 of an inch; the angle which the rays, tending towards the middle of the image, made with those lines, in which they would have proceeded without refraction, was 44 degrees 56'; and the vertical angle of the prism, 63 degrees 12'. Also the refractions on both sides of the prism--that is, of the incident and emergent rays--were, as near as I could make them, equal, and consequently about 54 degrees 4'; and the rays fell perpendicularly upon the wall. Now, subducting the diameter of the hole from the length and breadth of the image, there remains 13 inches the length, and 2 3/8 the breadth, comprehended by those rays, which, passing through the centre of the said hole, which that breadth subtended, was about 31', answerable to the sun's diameter; but the angle which its length subtended was more than five such diameters, namely 2 degrees 49'.

"Having made these observations, I first computed from them the refractive power of the glass, and found it measured by the ratio of the sines 20 to 31. And then, by that ratio, I computed the refractions of two rays flowing from opposite parts of the sun's discus, so as to differ 31' in their obliquity of incidence, and found that the emergent rays should have comprehended an angle of 31', as they did, before they were incident.

"But because this computation was founded on the hypothesis of the proportionality of the sines of incidence and refraction, which though by my own experience I could not imagine to be so erroneous as to make that angle but 31', which in reality was 2 degrees 49', yet my curiosity caused me again to make my prism. And having placed it at my window, as before, I observed that by turning it a little about its axis to and fro, so as to vary its obliquity to the light more than an angle of 4 degrees or 5 degrees, the colors were not thereby sensibly translated from their place on the wall, and consequently by that variation of incidence the quantity of refraction was not sensibly varied. By this experiment, therefore, as well as by the former computation, it was evident that the difference of the incidence of rays flowing from divers parts of the sun could not make them after decussation diverge at a sensibly greater angle than that at which they before converged; which being, at most, but about 31' or 32', there still remained some other cause to be found out, from whence it could be 2 degrees 49'."

All this caused Newton to suspect that the rays, after their trajection through the prism, moved in curved rather than in straight lines, thus tending to be cast upon the wall at different places according to the amount of this curve. His suspicions were increased, also, by happening to recall that a tennis-ball sometimes describes such a curve when "cut" by a tennis-racket striking the ball obliquely.

"For a circular as well as a progressive motion being communicated to it by the stroke," he says, "its parts on that side where the motions conspire must press and beat the contiguous air more violently than on the other, and there excite a reluctancy and reaction of the air proportionately greater. And for the same reason, if the rays of light should possibly be globular bodies, and by their oblique passage out of one medium into another acquire a circulating motion, they ought to feel the greater resistance from the ambient ether on that side where the motions conspire, and thence be continually bowed to the other. But notwithstanding this plausible ground of suspicion, when I came to examine it I could observe no such curvity in them. And, besides (which was enough for my purpose), I observed that the difference 'twixt the length of the image and diameter of the hole through which the light was transmitted was proportionable to their distance.

"The gradual removal of these suspicions at length led me to the experimentum crucis, which was this: I took two boards, and, placing one of them close behind the prism at the window, so that the light must pass through a small hole, made in it for the purpose, and fall on the other board, which I placed at about twelve feet distance, having first made a small hole in it also, for some of the incident light to pass through. Then I placed another prism behind this second board, so that the light trajected through both the boards might pass through that also, and be again refracted before it arrived at the wall. This done, I took the first prism in my hands and turned it to and fro slowly about its axis, so much as to make the several parts of the image, cast on the second board, successively pass through the hole in it, that I might observe to what places on the wall the second prism would refract them. And I saw by the variation of these places that the light, tending to that end of the image towards which the refraction of the first prism was made, did in the second prism suffer a refraction considerably greater than the light tending to the other end. And so the true cause of the length of that image was detected to be no other than that LIGHT consists of RAYS DIFFERENTLY REFRANGIBLE, which, without any respect to a difference in their incidence, were, according to their degrees of refrangibility, transmitted towards divers parts of the wall."[1]

THE NATURE OF COLOR

Having thus proved the composition of light, Newton took up an exhaustive discussion as to colors, which cannot be entered into at length here. Some of his remarks on the subject of compound colors, however, may be stated in part. Newton's views are of particular interest in this connection, since, as we have already pointed out, the question as to what constituted color could not be agreed upon by the philosophers. Some held that color was an integral part of the substance; others maintained that it was simply a reflection from the surface; and no scientific explanation had been generally accepted. Newton concludes his paper as follows:

"I might add more instances of this nature, but I shall conclude with the general one that the colors of all natural bodies have no other origin than this, that they are variously qualified to reflect one sort of light in greater plenty than another. And this I have experimented in a dark room by illuminating those bodies with uncompounded light of divers colors. For by that means any body may be made to appear of any color. They have there no appropriate color, but ever appear of the color of the light cast upon them, but yet with this difference, that they are most brisk and vivid in the light of their own daylight color. Minium appeareth there of any color indifferently with which 'tis illustrated, but yet most luminous in red; and so Bise appeareth

indifferently of any color with which 'tis illustrated, but yet most luminous in blue. And therefore Minium reflecteth rays of any color, but most copiously those indued with red; and consequently, when illustrated with daylight--that is, with all sorts of rays promiscuously blended--those qualified with red shall abound most in the reflected light, and by their prevalence cause it to appear of that color. And for the same reason, Bise, reflecting blue most copiously, shall appear blue by the excess of those rays in its reflected light; and the like of other bodies. And that this is the entire and adequate cause of their colors is manifest, because they have no power to change or alter the colors of any sort of rays incident apart, but put on all colors indifferently with which they are enlightened."[2]

This epoch-making paper aroused a storm of opposition. Some of Newton's opponents criticised his methods, others even doubted the truth of his experiments. There was one slight mistake in Newton's belief that all prisms would give a spectrum of exactly the same length, and it was some time before he corrected this error. Meanwhile he patiently met and answered the arguments of his opponents until he began to feel that patience was no longer a virtue. At one time he even went so far as to declare that, once he was "free of this business," he would renounce scientific research forever, at least in a public way. Fortunately for the world, however, he did not adhere to this determination, but went on to even greater discoveries--which, it may be added, involved still greater controversies.

In commenting on Newton's discovery of the composition of light, Voltaire said: "Sir Isaac Newton has demonstrated to the eye, by the bare assistance of a prism, that light is a composition of colored rays, which, being united, form white color. A single ray is by him divided into seven, which all fall upon a piece of linen or a sheet of white paper, in their order one above the other, and at equal distances. The first is red, the second orange, the third yellow, the fourth green, the fifth blue, the sixth indigo, the seventh a violet purple. Each of these rays transmitted afterwards by a hundred other prisms will never change the color it bears; in like manner as gold, when completely purged from its dross, will never change afterwards in the crucible."[3]

XII. NEWTON AND THE LAW OF GRAVITATION

We come now to the story of what is by common consent the greatest of scientific achievements. The law of universal gravitation is the most far-reaching principle as yet discovered. It has application equally to the minutest particle of matter and to the most distant suns in the universe, yet it is amazing in its very simplicity. As usually phrased, the law is this: That every particle of matter in the universe attracts every other particle with a force that varies directly with the mass of the particles and inversely as the squares of their mutual distance. Newton did not vault at once to the full expression of this law, though he had formulated it fully before he gave the results of his investigations to the world. We have now to follow the steps by which he reached this culminating achievement.

At the very beginning we must understand that the idea of universal gravitation was not absolutely original with Newton. Away back in the old Greek days, as we have seen, Anaxagoras conceived and clearly expressed the idea that the force which holds the heavenly bodies in their orbits may be the same that operates upon substances at the surface of the earth. With Anaxagoras this was scarcely more than a guess. After his day the idea seems not to have been expressed by any one until the seventeenth century's awakening of science. Then the consideration of Kepler's Third Law of planetary motion suggested to many minds perhaps

independently the probability that the force hitherto mentioned merely as centripetal, through the operation of which the planets are held in their orbits is a force varying inversely as the square of the distance from the sun. This idea had come to Robert Hooke, to Wren, and perhaps to Halley, as well as to Newton; but as yet no one had conceived a method by which the validity of the suggestion might be tested. It was claimed later on by Hooke that he had discovered a method demonstrating the truth of the theory of inverse squares, and after the full announcement of Newton's discovery a heated controversy was precipitated in which Hooke put forward his claims with accustomed acrimony. Hooke, however, never produced his demonstration, and it may well be doubted whether he had found a method which did more than vaguely suggest the law which the observations of Kepler had partially revealed. Newton's great merit lay not so much in conceiving the law of inverse squares as in the demonstration of the law. He was led to this demonstration through considering the orbital motion of the moon. According to the familiar story, which has become one of the classic myths of science, Newton was led to take up the problem through observing the fall of an apple. Voltaire is responsible for the story, which serves as well as another; its truth or falsity need not in the least concern us. Suffice it that through pondering on the familiar fact of terrestrial gravitation, Newton was led to question whether this force which operates so tangibly here at the earth's surface may not extend its influence out into the depths of space, so as to include, for example, the moon. Obviously some force pulls the moon constantly towards the earth; otherwise that body would fly off at a tangent and never return. May not this so-called centripetal force be identical with terrestrial gravitation? Such was Newton's query. Probably many another man since Anaxagoras had asked the same question, but assuredly Newton was the first man to find an answer.

The thought that suggested itself to Newton's mind was this: If we make a diagram illustrating the orbital course of the moon for any given period, say one minute, we shall find that the course of the moon departs from a straight line during that period by a measurable distance--that: is to say, the moon has been virtually pulled towards the earth by an amount that is represented by the difference between its actual position at the end of the minute under observation and the position it would occupy had its course been tangential, as, according to the first law of motion, it must have been had not some force deflected it towards the earth. Measuring the deflection in question--which is equivalent to the so-called versed sine of the arc traversed--we have a basis for determining the strength of the deflecting force. Newton constructed such a diagram, and, measuring the amount of the moon's departure from a tangential rectilinear course in one minute, determined this to be, by his calculation, thirteen feet. Obviously, then, the force acting upon the moon is one that would cause that body to fall towards the earth to the distance of thirteen feet in the first minute of its fall. Would such be the force of gravitation acting at the distance of the moon if the power of gravitation varies inversely as the square of the distance? That was the tangible form in which the problem presented itself to Newton. The mathematical solution of the problem was simple enough. It is based on a comparison of the moon's distance with the length of the earth's radius. On making this calculation, Newton found that the pull of gravitation--if that were really the force that controls the moon--gives that body a fall of slightly over fifteen feet in the first minute, instead of thirteen feet. Here was surely a suggestive approximation, yet, on the other band, the discrepancy seemed to be too great to warrant him in the supposition that he had found the true solution. He therefore dismissed the matter from his mind for the time being, nor did he return to it definitely for some years.

{illustration caption = DIAGRAM TO ILLUSTRATE NEWTON'S LAW OF GRAVITATION

(E represents the earth and A the moon. Were the earth's pull on the moon to cease, the moon's inertia would cause it to take the tangential course, AB. On the other hand, were the moon's motion to be stopped for an instant, the moon would fall directly towards the earth, along the line AD. The moon's actual orbit, resulting from these component forces, is AC. Let AC represent the actual flight of the moon in one minute. Then BC, which is obviously equal to AD, represents the distance which the moon virtually falls towards the earth in one minute. Actual computation, based on measurements of the moon's orbit, showed this distance to be about fifteen feet. Another computation showed that this is the distance that the moon would fall towards the earth under the influence of gravity, on the supposition that the force of gravity decreases inversely with the square of the distance; the basis of comparison being furnished by falling bodies at the surface of the earth. Theory and observations thus coinciding, Newton was justified in declaring that the force that pulls the moon towards the earth and keeps it in its orbit, is the familiar force of gravity, and that this varies inversely as the square of the distance.)}

It was to appear in due time that Newton's hypothesis was perfectly valid and that his method of attempted demonstration was equally so. The difficulty was that the earth's proper dimensions were not at that time known. A wrong estimate of the earth's size vitiated all the other calculations involved, since the measurement of the moon's distance depends upon the observation of the parallax, which cannot lead to a correct computation unless the length of the earth's radius is accurately known. Newton's first calculation was made as early as 1666, and it was not until 1682 that his attention was called to a new and apparently accurate measurement of a degree of the earth's meridian made by the French astronomer Picard. The new measurement made a degree of the earth's surface 69.10 miles, instead of sixty miles.

Learning of this materially altered calculation as to the earth's size, Newton was led to take up again his problem of the falling moon. As he proceeded with his computation, it became more and more certain that this time the result was to harmonize with the observed facts. As the story goes, he was so completely overwhelmed with emotion that he was forced to ask a friend to complete the simple calculation. That story may well be true, for, simple though the computation was, its result was perhaps the most wonderful demonstration hitherto achieved in the entire field of science. Now at last it was known that the force of gravitation operates at the distance of the moon, and holds that body in its elliptical orbit, and it required but a slight effort of the imagination to assume that the force which operates through such a reach of space extends its influence yet more widely. That such is really the case was demonstrated presently through calculations as to the moons of Jupiter and by similar computations regarding the orbital motions of the various planets. All results harmonizing, Newton was justified in reaching the conclusion that gravitation is a universal property of matter. It remained, as we shall see, for nineteenth-century scientists to prove that the same force actually operates upon the stars, though it should be added that this demonstration merely fortified a belief that had already found full acceptance.

Having thus epitomized Newton's discovery, we must now take up the steps of his progress somewhat in detail, and state his theories and their demonstration in his own words. Proposition IV., theorem 4, of his Principia is as follows:

"That the moon gravitates towards the earth and by the force of gravity is continually drawn off from a rectilinear motion and retained in its orbit.

"The mean distance of the moon from the earth, in the syzygies in semi-diameters of the earth, is,

according to Ptolemy and most astronomers, 59; according to Vendelin and Huygens, 60; to Copernicus, 60 1/3; to Street, 60 2/3; and to Tycho, 56 1/2. But Tycho, and all that follow his tables of refractions, making the refractions of the sun and moon (altogether against the nature of light) to exceed the refractions of the fixed stars, and that by four or five minutes NEAR THE HORIZON, did thereby increase the moon's HORIZONTAL parallax by a like number of minutes, that is, by a twelfth or fifteenth part of the whole parallax. Correct this error and the distance will become about 60 1/2 semi-diameters of the earth, near to what others have assigned. Let us assume the mean distance of 60 diameters in the syzygies; and suppose one revolution of the moon, in respect to the fixed stars, to be completed in 27d. 7h. 43', as astronomers have determined; and the circumference of the earth to amount to 123,249,600 Paris feet, as the French have found by mensuration. And now, if we imagine the moon, deprived of all motion, to be let go, so as to descend towards the earth with the impulse of all that force by which (by Cor. Prop. iii.) it is retained in its orb, it will in the space of one minute of time describe in its fall 15 1/12 Paris feet. For the versed sine of that arc which the moon, in the space of one minute of time, would by its mean motion describe at the distance of sixty semi-diameters of the earth, is nearly 15 1/12 Paris feet, or more accurately 15 feet, 1 inch, 1 line 4/9. Wherefore, since that force, in approaching the earth, increases in the reciprocal-duplicate proportion of the distance, and upon that account, at the surface of the earth, is 60 x 60 times greater than at the moon, a body in our regions, falling with that force, ought in the space of one minute of time to describe 60 x 60 x 15 1/12 Paris feet; and in the space of one second of time, to describe 15 1/12 of those feet, or more accurately, 15 feet, 1 inch, 1 line 4/9. And with this very force we actually find that bodies here upon earth do really descend; for a pendulum oscillating seconds in the latitude of Paris will be 3 Paris feet, and 8 lines 1/2 in length, as Mr. Huygens has observed. And the space which a heavy body describes by falling in one second of time is to half the length of the pendulum in the duplicate ratio of the circumference of a circle to its diameter (as Mr. Huygens has also shown), and is therefore 15 Paris feet, 1 inch, 1 line 4/9. And therefore the force by which the moon is retained in its orbit is that very same force which we commonly call gravity; for, were gravity another force different from that, then bodies descending to the earth with the joint impulse of both forces would fall with a double velocity, and in the space of one second of time would describe 30 1/6 Paris feet; altogether against experience."[1]

All this is beautifully clear, and its validity has never in recent generations been called in question; yet it should be explained that the argument does not amount to an actually indisputable demonstration. It is at least possible that the coincidence between the observed and computed motion of the moon may be a mere coincidence and nothing more. This probability, however, is so remote that Newton is fully justified in disregarding it, and, as has been said, all subsequent generations have accepted the computation as demonstrative.

Let us produce now Newton's further computations as to the other planetary bodies, passing on to his final conclusion that gravity is a universal force.

"PROPOSITION V., THEOREM V.

"That the circumjovial planets gravitate towards Jupiter; the circumsaturnal towards Saturn; the circumsolar towards the sun; and by the forces of their gravity are drawn off from rectilinear motions, and retained in curvilinear orbits.

"For the revolutions of the circumjovial planets about Jupiter, of the circumsaturnal about Saturn,

and of Mercury and Venus and the other circumsolar planets about the sun, are appearances of the same sort with the revolution of the moon about the earth; and therefore, by Rule ii., must be owing to the same sort of causes; especially since it has been demonstrated that the forces upon which those revolutions depend tend to the centres of Jupiter, of Saturn, and of the sun; and that those forces, in receding from Jupiter, from Saturn, and from the sun, decrease in the same proportion, and according to the same law, as the force of gravity does in receding from the earth.

"COR. 1.--There is, therefore, a power of gravity tending to all the planets; for doubtless Venus, Mercury, and the rest are bodies of the same sort with Jupiter and Saturn. And since all attraction (by Law iii.) is mutual, Jupiter will therefore gravitate towards all his own satellites, Saturn towards his, the earth towards the moon, and the sun towards all the primary planets.

"COR. 2.--The force of gravity which tends to any one planet is reciprocally as the square of the distance of places from the planet's centre.

"COR. 3.--All the planets do mutually gravitate towards one another, by Cor. 1 and 2, and hence it is that Jupiter and Saturn, when near their conjunction, by their mutual attractions sensibly disturb each other's motions. So the sun disturbs the motions of the moon; and both sun and moon disturb our sea, as we shall hereafter explain.

"SCHOLIUM

"The force which retains the celestial bodies in their orbits has been hitherto called centripetal force; but it being now made plain that it can be no other than a gravitating force, we shall hereafter call it gravity. For the cause of the centripetal force which retains the moon in its orbit will extend itself to all the planets by Rules i., ii., and iii.

"PROPOSITION VI., THEOREM VI.

"That all bodies gravitate towards every planet; and that the weights of the bodies towards any the same planet, at equal distances from the centre of the planet, are proportional to the quantities of matter which they severally contain.

"It has been now a long time observed by others that all sorts of heavy bodies (allowance being made for the inability of retardation which they suffer from a small power of resistance in the air) descend to the earth FROM EQUAL HEIGHTS in equal times; and that equality of times we may distinguish to a great accuracy by help of pendulums. I tried the thing in gold, silver, lead, glass, sand, common salt, wood, water, and wheat. I provided two wooden boxes, round and equal: I filled the one with wood, and suspended an equal weight of gold (as exactly as I could) in the centre of oscillation of the other. The boxes hanging by eleven feet, made a couple of pendulums exactly equal in weight and figure, and equally receiving the resistance of the air. And, placing the one by the other, I observed them to play together forward and backward, for a long time, with equal vibrations. And therefore the quantity of matter in gold was to the quantity of matter in the wood as the action of the motive force (or vis motrix) upon all the gold to the action of the same upon all the wood--that is, as the weight of the one to the weight of the other: and the like happened in the other bodies. By these experiments, in bodies of the same weight, I could manifestly have discovered a difference of matter less than the thousandth part of the

whole, had any such been. But, without all doubt, the nature of gravity towards the planets is the same as towards the earth. For, should we imagine our terrestrial bodies removed to the orb of the moon, and there, together with the moon, deprived of all motion, to be let go, so as to fall together towards the earth, it is certain, from what we have demonstrated before, that, in equal times, they would describe equal spaces with the moon, and of consequence are to the moon, in quantity and matter, as their weights to its weight.

"Moreover, since the satellites of Jupiter perform their revolutions in times which observe the sesquiplicate proportion of their distances from Jupiter's centre, their accelerative gravities towards Jupiter will be reciprocally as the square of their distances from Jupiter's centre--that is, equal, at equal distances. And, therefore, these satellites, if supposed to fall TOWARDS JUPITER from equal heights, would describe equal spaces in equal times, in like manner as heavy bodies do on our earth. And, by the same argument, if the circumsolar planets were supposed to be let fall at equal distances from the sun, they would, in their descent towards the sun, describe equal spaces in equal times. But forces which equally accelerate unequal bodies must be as those bodies--that is to say, the weights of the planets (TOWARDS THE SUN must be as their quantities of matter. Further, that the weights of Jupiter and his satellites towards the sun are proportional to the several quantities of their matter, appears from the exceedingly regular motions of the satellites. For if some of these bodies were more strongly attracted to the sun in proportion to their quantity of matter than others, the motions of the satellites would be disturbed by that inequality of attraction. If at equal distances from the sun any satellite, in proportion to the quantity of its matter, did gravitate towards the sun with a force greater than Jupiter in proportion to his, according to any given proportion, suppose d to e; then the distance between the centres of the sun and of the satellite's orbit would be always greater than the distance between the centres of the sun and of Jupiter nearly in the subduplicate of that proportion: as by some computations I have found. And if the satellite did gravitate towards the sun with a force, lesser in the proportion of e to d, the distance of the centre of the satellite's orb from the sun would be less than the distance of the centre of Jupiter from the sun in the subduplicate of the same proportion. Therefore, if at equal distances from the sun, the accelerative gravity of any satellite towards the sun were greater or less than the accelerative gravity of Jupiter towards the sun by one-one-thousandth part of the whole gravity, the distance of the centre of the satellite's orbit from the sun would be greater or less than the distance of Jupiter from the sun by one one-two-thousandth part of the whole distance--that is, by a fifth part of the distance of the utmost satellite from the centre of Jupiter; an eccentricity of the orbit which would be very sensible. But the orbits of the satellites are concentric to Jupiter, and therefore the accelerative gravities of Jupiter and of all its satellites towards the sun, at equal distances from the sun, are as their several quantities of matter; and the weights of the moon and of the earth towards the sun are either none, or accurately proportional to the masses of matter which they contain.

"COR. 5.--The power of gravity is of a different nature from the power of magnetism; for the magnetic attraction is not as the matter attracted. Some bodies are attracted more by the magnet; others less; most bodies not at all. The power of magnetism in one and the same body may be increased and diminished; and is sometimes far stronger, for the quantity of matter, than the power of gravity; and in receding from the magnet decreases not in the duplicate, but almost in the triplicate proportion of the distance, as nearly as I could judge from some rude observations.

"PROPOSITION VII., THEOREM VII.

"That there is a power of gravity tending to all bodies, proportional to the several quantities of matter which they contain.

That all the planets mutually gravitate one towards another we have proved before; as well as that the force of gravity towards every one of them considered apart, is reciprocally as the square of the distance of places from the centre of the planet. And thence it follows, that the gravity tending towards all the planets is proportional to the matter which they contain.

"Moreover, since all the parts of any planet A gravitates towards any other planet B; and the gravity of every part is to the gravity of the whole as the matter of the part is to the matter of the whole; and to every action corresponds a reaction; therefore the planet B will, on the other hand, gravitate towards all the parts of planet A, and its gravity towards any one part will be to the gravity towards the whole as the matter of the part to the matter of the whole. Q.E.D.

"HENCE IT WOULD APPEAR THAT the force of the whole must arise from the force of the component parts."

Newton closes this remarkable Book iii. with the following words:

"Hitherto we have explained the phenomena of the heavens and of our sea by the power of gravity, but have not yet assigned the cause of this power. This is certain, that it must proceed from a cause that penetrates to the very centre of the sun and planets, without suffering the least diminution of its force; that operates not according to the quantity of the surfaces of the particles upon which it acts (as mechanical causes used to do), but according to the quantity of solid matter which they contain, and propagates its virtue on all sides to immense distances, decreasing always in the duplicate proportions of the distances. Gravitation towards the sun is made up out of the gravitations towards the several particles of which the body of the sun is composed; and in receding from the sun decreases accurately in the duplicate proportion of the distances as far as the orb of Saturn, as evidently appears from the quiescence of the aphelions of the planets; nay, and even to the remotest aphelions of the comets, if those aphelions are also quiescent. But hitherto I have not been able to discover the cause of those properties of gravity from phenomena, and I frame no hypothesis; for whatever is not deduced from the phenomena is to be called an hypothesis; and hypotheses, whether metaphysical or physical, whether of occult qualities or mechanical, have no place in experimental philosophy. . . . And to us it is enough that gravity does really exist, and act according to the laws which we have explained, and abundantly serves to account for all the motions of the celestial bodies and of our sea."[2]

The very magnitude of the importance of the theory of universal gravitation made its general acceptance a matter of considerable time after the actual discovery. This opposition had of course been foreseen by Newton, and, much as be dreaded controversy, he was prepared to face it and combat it to the bitter end. He knew that his theory was right; it remained for him to convince the world of its truth. He knew that some of his contemporary philosophers would accept it at once; others would at first doubt, question, and dispute, but finally accept; while still others would doubt and dispute until the end of their days. This had been the history of other great discoveries; and this will probably be the history of most great discoveries for all time. But in this case the discoverer lived to see his theory accepted by practically all the great minds of his

time.

Delambre is authority for the following estimate of Newton by Lagrange. "The celebrated Lagrange," he says, "who frequently asserted that Newton was the greatest genius that ever existed, used to add--'and the most fortunate, for we cannot find MORE THAN ONCE a system of the world to establish.' " With pardonable exaggeration the admiring followers of the great generalizer pronounced this epitaph:

"Nature and Nature's laws lay hid in night; God said `Let Newton be!' and all was light."

XIII. INSTRUMENTS OF PRECISION IN THE AGE OF NEWTON

During the Newtonian epoch there were numerous important inventions of scientific instruments, as well as many improvements made upon the older ones. Some of these discoveries have been referred to briefly in other places, but their importance in promoting scientific investigation warrants a fuller treatment of some of the more significant.

Many of the errors that had arisen in various scientific calculations before the seventeenth century may be ascribed to the crudeness and inaccuracy in the construction of most scientific instruments. Scientists had not as yet learned that an approach to absolute accuracy was necessary in every investigation in the field of science, and that such accuracy must be extended to the construction of the instruments used in these investigations and observations. In astronomy it is obvious that instruments of delicate exactness are most essential; yet Tycho Brahe, who lived in the sixteenth century, is credited with being the first astronomer whose instruments show extreme care in construction.

It seems practically settled that the first telescope was invented in Holland in 1608; but three men, Hans Lippershey, James Metius, and Zacharias Jansen, have been given the credit of the invention at different times. It would seem from certain papers, now in the library of the University of Leyden, and included in Huygens's papers, that Lippershey was probably the first to invent a telescope and to describe his invention. The story is told that Lippershey, who was a spectacle-maker, stumbled by accident upon the discovery that when two lenses are held at a certain distance apart, objects at a distance appear nearer and larger. Having made this discovery, be fitted two lenses with a tube so as to maintain them at the proper distance, and thus constructed the first telescope.

It was Galileo, however, as referred to in a preceding chapter, who first constructed a telescope based on his knowledge of the laws of refraction. In 1609, having heard that an instrument had been invented, consisting of two lenses fixed in a tube, whereby objects were made to appear larger and nearer, he set about constructing such an instrument that should follow out the known effects of refraction. His first telescope, made of two lenses fixed in a lead pipe, was soon followed by others of improved types, Galileo devoting much time and labor to perfecting lenses and correcting errors. In fact, his work in developing the instrument was so important that the telescope came gradually to be known as the "Galilean telescope."

In the construction of his telescope Galileo made use of a convex and a concave lens; but shortly after this Kepler invented an instrument in which both the lenses used were convex. This telescope gave a much larger field of view than the Galilean telescope, but did not give as clear

an image, and in consequence did not come into general use until the middle of the seventeenth century. The first powerful telescope of this type was made by Huygens and his brother. It was of twelve feet focal length, and enabled Huygens to discover a new satellite of Saturn, and to determine also the true explanation of Saturn's ring.

It was Huygens, together with Malvasia and Auzout, who first applied the micrometer to the telescope, although the inventor of the first micrometer was William Gascoigne, of Yorkshire, about 1636. The micrometer as used in telescopes enables the observer to measure accurately small angular distances. Before the invention of the telescope such measurements were limited to the angle that could be distinguished by the naked eye, and were, of course, only approximately accurate. Even very careful observers, such as Tycho Brahe, were able to obtain only fairly accurate results. But by applying Gascoigne's invention to the telescope almost absolute accuracy became at once possible. The principle of Gascoigne's micrometer was that of two pointers lying parallel, and in this position pointing to zero. These were arranged so that the turning of a single screw separated or approximated them at will, and the angle thus formed could be determined with absolute accuracy.

Huygens's micrometer was a slip of metal of variable breadth inserted at the focus of the telescope. By observing at what point this exactly covered an object under examination, and knowing the focal length of the telescope and the width of the metal, he could then deduce the apparent angular breadth of the object. Huygens discovered also that an object placed in the common focus of the two lenses of a Kepler telescope appears distinct and clearly defined. The micrometers of Malvasia, and later of Auzout and Picard, are the development of this discovery. Malvasia's micrometer, which he described in 1662, consisted of fine silver wires placed at right-angles at the focus of his telescope.

As telescopes increased in power, however, it was found that even the finest wire, or silk filaments, were much too thick for astronomical observations, as they obliterated the image, and so, finally, the spider-web came into use and is still used in micrometers and other similar instruments. Before that time, however, the fine crossed wires had revolutionized astronomical observations. "We may judge how great was the improvement which these contrivances introduced into the art of observing," says Whewell, "by finding that Hevelius refused to adopt them because they would make all the old observations of no value. He had spent a laborious and active life in the exercise of the old methods, and could not bear to think that all the treasures which he had accumulated had lost their worth by the discovery of a new mine of richer ones."[1]

Until the time of Newton, all the telescopes in use were either of the Galilean or Keplerian type, that is, refractors. But about the year 1670 Newton constructed his first reflecting telescope, which was greatly superior to, although much smaller than, the telescopes then in use. He was led to this invention by his experiments with light and colors. In 1671 he presented to the Royal Society a second and somewhat larger telescope, which he had made; and this type of instrument was little improved upon until the introduction of the achromatic telescope, invented by Chester Moor Hall in 1733.

As is generally known, the element of accurate measurements of time plays an important part in the measurements of the movements of the heavenly bodies. In fact, one was scarcely possible without the other, and as it happened it was the same man, Huygens, who perfected Kepler's

telescope and invented the pendulum clock. The general idea had been suggested by Galileo; or, better perhaps, the equal time occupied by the successive oscillations of the pendulum had been noted by him. He had not been able, however, to put this discovery to practical account. But in 1656 Huygens invented the necessary machinery for maintaining the motion of the pendulum and perfected several accurate clocks. These clocks were of invaluable assistance to the astronomers, affording as they did a means of keeping time "more accurate than the sun itself." When Picard had corrected the variation caused by heat and cold acting upon the pendulum rod by combining metals of different degrees of expansibility, a high degree of accuracy was possible.

But while the pendulum clock was an unequalled stationary time-piece, it was useless in such unstable situations as, for example, on shipboard. But here again Huygens played a prominent part by first applying the coiled balance-spring for regulating watches and marine clocks. The idea of applying a spring to the balance-wheel was not original with Huygens, however, as it had been first conceived by Robert Hooke; but Huygens's application made practical Hooke's idea. In England the importance of securing accurate watches or marine clocks was so fully appreciated that a reward of L20,000 sterling was offered by Parliament as a stimulus to the inventor of such a time-piece. The immediate incentive for this offer was the obvious fact that with such an instrument the determination of the longitude of places would be much simplified. Encouraged by these offers, a certain carpenter named Harrison turned his attention to the subject of watch-making, and, after many years of labor, in 1758 produced a spring time-keeper which, during a sea-voyage occupying one hundred and sixty-one days, varied only one minute and five seconds. This gained for Harrison a reward Of L5000 sterling at once, and a little later L10,000 more, from Parliament.

While inventors were busy with the problem of accurate chronometers, however, another instrument for taking longitude at sea had been invented. This was the reflecting quadrant, or sextant, as the improved instrument is now called, invented by John Hadley in 1731, and independently by Thomas Godfrey, a poor glazier of Philadelphia, in 1730. Godfrey's invention, which was constructed on the same principle as that of the Hadley instrument, was not generally recognized until two years after Hadley's discovery, although the instrument was finished and actually in use on a sea-voyage some months before Hadley reported his invention. The principle of the sextant, however, seems to have been known to Newton, who constructed an instrument not very unlike that of Hadley; but this invention was lost sight of until several years after the philosopher's death and some time after Hadley's invention.

The introduction of the sextant greatly simplified taking reckonings at sea as well as facilitating taking the correct longitude of distant places. Before that time the mariner was obliged to depend upon his compass, a cross-staff, or an astrolabe, a table of the sun's declination and a correction for the altitude of the polestar, and very inadequate and incorrect charts. Such were the instruments used by Columbus and Vasco da Gama and their immediate successors.

During the Newtonian period the microscopes generally in use were those constructed of simple lenses, for although compound microscopes were known, the difficulties of correcting aberration had not been surmounted, and a much clearer field was given by the simple instrument. The results obtained by the use of such instruments, however, were very satisfactory in many ways. By referring to certain plates in this volume, which reproduce illustrations from Robert Hooke's

work on the microscope, it will be seen that quite a high degree of effectiveness had been attained. And it should be recalled that Antony von Leeuwenboek, whose death took place shortly before Newton's, had discovered such micro-organisms as bacteria, had seen the blood corpuscles in circulation, and examined and described other microscopic structures of the body.

XIV. PROGRESS IN ELECTRICITY FROM GILBERT AND VON GUERICKE TO FRANKLIN

We have seen how Gilbert, by his experiments with magnets, gave an impetus to the study of magnetism and electricity. Gilbert himself demonstrated some facts and advanced some theories, but the system of general laws was to come later. To this end the discovery of electrical repulsion, as well as attraction, by Von Guericke, with his sulphur ball, was a step forward; but something like a century passed after Gilbert's beginning before anything of much importance was done in the field of electricity.

In 1705, however, Francis Hauksbee began a series of experiments that resulted in some startling demonstrations. For many years it had been observed that a peculiar light was seen sometimes in the mercurial barometer, but Hauksbee and the other scientific investigators supposed the radiance to be due to the mercury in a vacuum, brought about, perhaps, by some agitation. That this light might have any connection with electricity did not, at first, occur to Hauksbee any more than it had to his predecessors. The problem that interested him was whether the vacuum in the tube of the barometer was essential to the light; and in experimenting to determine this, he invented his "mercurial fountain." Having exhausted the air in a receiver containing some mercury, he found that by allowing air to rush through the mercury the metal became a jet thrown in all directions against the sides of the vessel, making a great, flaming shower, "like flashes of lightning," as he said. But it seemed to him that there was a difference between this light and the glow noted in the barometer. This was a bright light, whereas the barometer light was only a glow. Pondering over this, Hauksbee tried various experiments, revolving pieces of amber, flint, steel, and other substances in his exhausted air-pump receiver, with negative, or unsatisfactory, results. Finally, it occurred to him to revolve an exhausted glass tube itself. Mounting such a globe of glass on an axis so that it could be revolved rapidly by a belt running on a large wheel, he found that by holding his fingers against the whirling globe a purplish glow appeared, giving sufficient light so that coarse print could be read, and the walls of a dark room sensibly lightened several feet away. As air was admitted to the globe the light gradually diminished, and it seemed to him that this diminished glow was very similar in appearance to the pale light seen in the mercurial barometer. Could it be that it was the glass, and not the mercury, that caused it? Going to a barometer he proceeded to rub the glass above the column of mercury over the vacuum, without disturbing the mercury, when, to his astonishment, the same faint light, to all appearances identical with the glow seen in the whirling globe, was produced.

Turning these demonstrations over in his mind, he recalled the well-known fact that rubbed glass attracted bits of paper, leaf-brass, and other light substances, and that this phenomenon was supposed to be electrical. This led him finally to determine the hitherto unsuspected fact, that the glow in the barometer was electrical as was also the glow seen in his whirling globe. Continuing his investigations, he soon discovered that solid glass rods when rubbed produced the same effects as the tube. By mere chance, happening to hold a rubbed tube to his cheek, he felt the effect of electricity upon the skin like "a number of fine, limber hairs," and this suggested to him

that, since the mysterious manifestation was so plain, it could be made to show its effects upon various substances. Suspending some woollen threads over the whirling glass cylinder, he found that as soon as he touched the glass with his hands the threads, which were waved about by the wind of the revolution, suddenly straightened themselves in a peculiar manner, and stood in a radical position, pointing to the axis of the cylinder.

Encouraged by these successes, he continued his experiments with breathless expectancy, and soon made another important discovery, that of "induction," although the real significance of this discovery was not appreciated by him or, for that matter, by any one else for several generations following. This discovery was made by placing two revolving cylinders within an inch of each other, one with the air exhausted and the other unexhausted. Placing his hand on the unexhausted tube caused the light to appear not only upon it, but on the other tube as well. A little later he discovered that it is not necessary to whirl the exhausted tube to produce this effect, but simply to place it in close proximity to the other whirling cylinder.

These demonstrations of Hauksbee attracted wide attention and gave an impetus to investigators in the field of electricity; but still no great advance was made for something like a quarter of a century. Possibly the energies of the scientists were exhausted for the moment in exploring the new fields thrown open to investigation by the colossal work of Newton.

THE EXPERIMENTS OF STEPHEN GRAY

In 1729 Stephen Gray (died in 1736), an eccentric and irascible old pensioner of the Charter House in London, undertook some investigations along lines similar to those of Hauksbee. While experimenting with a glass tube for producing electricity, as Hauksbee had done, he noticed that the corks with which he had stopped the ends of the tube to exclude the dust, seemed to attract bits of paper and leaf-brass as well as the glass itself. He surmised at once that this mysterious electricity, or "virtue," as it was called, might be transmitted through other substances as it seemed to be through glass.

"Having by me an ivory ball of about one and three-tenths of an inch in diameter," he writes, "with a hole through it, this I fixed upon a fir-stick about four inches long, thrusting the other end into the cork, and upon rubbing the tube found that the ball attracted and repelled the feather with more vigor than the cork had done, repeating its attractions and repulsions for many times together. I then fixed the ball on longer sticks, first upon one of eight inches, and afterwards upon one of twenty-four inches long, and found the effect the same. Then I made use of iron, and then brass wire, to fix the ball on, inserting the other end of the wire in the cork, as before, and found that the attraction was the same as when the fir-sticks were made use of, and that when the feather was held over against any part of the wire it was attracted by it; but though it was then nearer the tube, yet its attraction was not so strong as that of the ball. When the wire of two or three feet long was used, its vibrations, caused by the rubbing of the tube, made it somewhat troublesome to be managed. This put me to thinking whether, if the ball was hung by a pack-thread and suspended by a loop on the tube, the electricity would not be carried down the line to the ball; I found it to succeed accordingly; for upon suspending the ball on the tube by a pack-thread about three feet long, when the tube had been excited by rubbing, the ivory ball attracted and repelled the leaf-brass over which it was held as freely as it had done when it was suspended on sticks or wire, as did also a ball of cork, and another of lead that weighed one pound and a quarter."

Gray next attempted to determine what other bodies would attract the bits of paper, and for this purpose he tried coins, pieces of metal, and even a tea-kettle, "both empty and filled with hot or cold water"; but he found that the attractive power appeared to be the same regardless of the substance used.

"I next proceeded," he continues, "to try at what greater distances the electric virtues might be carried, and having by me a hollow walking-cane, which I suppose was part of a fishing-rod, two feet seven inches long, I cut the great end of it to fit into the bore of the tube, into which it went about five inches; then when the cane was put into the end of the tube, and this excited, the cane drew the leaf-brass to the height of more than two inches, as did also the ivory ball, when by a cork and stick it had been fixed to the end of the cane.... With several pieces of Spanish cane and fir-sticks I afterwards made a rod, which, together with the tube, was somewhat more than eighteen feet long, which was the greatest length I could conveniently use in my chamber, and found the attraction very nearly, if not altogether, as strong as when the ball was placed on the shorter rods."

This experiment exhausted the capacity of his small room, but on going to the country a little later he was able to continue his experiments. "To a pole of eighteen feet there was tied a line of thirty-four feet in length, so that the pole and line together were fifty-two feet. With the pole and tube I stood in the balcony, the assistant below in the court, where he held the board with the leaf-brass on it. Then the tube being excited, as usual, the electric virtue passed from the tube up the pole and down the line to the ivory ball, which attracted the leaf-brass, and as the ball passed over it in its vibrations the leaf-brass would follow it till it was carried off the board."

Gray next attempted to send the electricity over a line suspended horizontally. To do this he suspended the pack-thread by pieces of string looped over nails driven into beams for that purpose. But when thus suspended he found that the ivory ball no longer excited the leaf-brass, and he guessed correctly that the explanation of this lay in the fact that "when the electric virtue came to the loop that was suspended on the beam it went up the same to the beam," none of it reaching the ball. As we shall see from what follows, however, Gray had not as yet determined that certain substances will conduct electricity while others will not. But by a lucky accident he made the discovery that silk, for example, was a poor conductor, and could be turned to account in insulating the conducting-cord.

A certain Mr. Wheler had become much interested in the old pensioner and his work, and, as a guest at the Wheler house, Gray had been repeating some of his former experiments with the fishing-rod, line, and ivory ball. He had finally exhausted the heights from which these experiments could be made by climbing to the clock-tower and exciting bits of leaf-brass on the ground below.

"As we had no greater heights here," he says, "Mr. Wheler was desirous to try whether we could not carry the electric virtue horizontally. I then told him of the attempt I had made with that design, but without success, telling him the method and materials made use of, as mentioned above. He then proposed a silk line to support the line by which the electric virtue was to pass. I told him it might do better upon account of its smallness; so that there would be less virtue carried from the line of communication.

"The first experiment was made in the matted gallery, July 2, 1729, about ten in the morning.

About four feet from the end of the gallery there was a cross line that was fixed by its ends to each side of the gallery by two nails; the middle part of the line was silk, the rest at each end pack-thread; then the line to which the ivory ball was hung and by which the electric virtue was to be conveyed to it from the tube, being eighty and one-half feet in length, was laid on the cross silk line, so that the ball hung about nine feet below it. Then the other end of the line was by a loop suspended on the glass cane, and the leaf-brass held under the ball on a piece of white paper; when, the tube being rubbed, the ball attracted the leaf-brass, and kept it suspended on it for some time."

This experiment succeeded so well that the string was lengthened until it was some two hundred and ninety-three feet long; and still the attractive force continued, apparently as strong as ever. On lengthening the string still more, however, the extra weight proved too much for the strength of the silk suspending-thread. "Upon this," says Gray, "having brought with me both brass and iron wire, instead of the silk we put up small iron wire; but this was too weak to bear the weight of the line. We then took brass wire of a somewhat larger size than that of iron. This supported our line of communication; but though the tube was well rubbed, yet there was not the least motion or attraction given by the ball, neither with the great tube, which we made use of when we found the small solid cane to be ineffectual; by which we were now convinced that the success we had before depended upon the lines that supported the line of communication being silk, and not upon their being small, as before trial I had imagined it might be; the same effect happening here as it did when the line that is to convey the electric virtue is supported by pack-thread."

Soon after this Gray and his host suspended a pack-thread six hundred and sixty-six feet long on poles across a field, these poles being slightly inclined so that the thread could be suspended from the top by small silk cords, thus securing the necessary insulation. This pack-thread line, suspended upon poles along which Gray was able to transmit the electricity, is very suggestive of the modern telegraph, but the idea of signalling or making use of it for communicating in any way seems not to have occurred to any one at that time. Even the successors of Gray who constructed lines some thousands of feet long made no attempt to use them for anything but experimental purposes--simply to test the distances that the current could be sent. Nevertheless, Gray should probably be credited with the discovery of two of the most important properties of electricity--that it can be conducted and insulated, although, as we have seen, Gilbert and Von Guericke had an inkling of both these properties.

EXPERIMENTS OF CISTERNAY DUFAY

So far England had produced the two foremost workers in electricity. It was now France's turn to take a hand, and, through the efforts of Charles Francois de Cisternay Dufay, to advance the science of electricity very materially. Dufay was a highly educated savant, who had been soldier and diplomat betimes, but whose versatility and ability as a scientist is shown by the fact that he was the only man who had ever contributed to the annals of the academy investigations in every one of the six subjects admitted by that institution as worthy of recognition. Dufay upheld his reputation in this new field of science, making many discoveries and correcting many mistakes of former observers. In this work also he proved himself a great diplomat by remaining on terms of intimate friendship with Dr. Gray--a thing that few people were able to do.

Almost his first step was to overthrow the belief that certain bodies are "electrics" and others

"non-electrics"--that is, that some substances when rubbed show certain peculiarities in attracting pieces of paper and foil which others do not. Dufay proved that all bodies possess this quality in a certain degree.

"I have found that all bodies (metallic, soft, or fluid ones excepted)," he says, "may be made electric by first heating them more or less and then rubbing them on any sort of cloth. So that all kinds of stones, as well precious as common, all kinds of wood, and, in general, everything that I have made trial of, became electric by beating and rubbing, except such bodies as grow soft by beat, as the gums, which dissolve in water, glue, and such like substances. 'Tis also to be remarked that the hardest stones or marbles require more chafing or heating than others, and that the same rule obtains with regard to the woods; so that box, lignum vitae, and such others must be chafed almost to the degree of browning, whereas fir, lime-tree, and cork require but a moderate heat.

"Having read in one of Mr. Gray's letters that water may be made electrical by holding the excited glass tube near it (a dish of water being fixed to a stand and that set on a plate of glass, or on the brim of a drinking-glass, previously chafed, or otherwise warmed), I have found, upon trial, that the same thing happened to all bodies without exception, whether solid or fluid, and that for that purpose 'twas sufficient to set them on a glass stand slightly warmed, or only dried, and then by bringing the tube near them they immediately became electrical. I made this experiment with ice, with a lighted wood-coal, and with everything that came into my mind; and I constantly remarked that such bodies of themselves as were least electrical had the greatest degree of electricity communicated to them at the approval of the glass tube."

His next important discovery was that colors had nothing to do with the conduction of electricity. "Mr. Gray says, towards the end of one of his letters," he writes, "that bodies attract more or less according to their colors. This led me to make several very singular experiments. I took nine silk ribbons of equal size, one white, one black, and the other seven of the seven primitive colors, and having hung them all in order in the same line, and then bringing the tube near them, the black one was first attracted, the white one next, and others in order successively to the red one, which was attracted least, and the last of them all. I afterwards cut out nine square pieces of gauze of the same colors with the ribbons, and having put them one after another on a hoop of wood, with leaf-gold under them, the leaf-gold was attracted through all the colored pieces of gauze, but not through the white or black. This inclined me first to think that colors contribute much to electricity, but three experiments convinced me to the contrary. The first, that by warming the pieces of gauze neither the black nor white pieces obstructed the action of the electrical tube more than those of the other colors. In like manner, the ribbons being warmed, the black and white are not more strongly attracted than the rest. The second is, the gauzes and ribbons being wetted, the ribbons are all attracted equally, and all the pieces of gauze equally intercept the action of electric bodies. The third is, that the colors of a prism being thrown on a white gauze, there appear no differences of attraction. Whence it proceeds that this difference proceeds, not from the color, as a color, but from the substances that are employed in the dyeing. For when I colored ribbons by rubbing them with charcoal, carmine, and such other substances, the differences no longer proved the same."

In connection with his experiments with his thread suspended on glass poles, Dufay noted that a certain amount of the current is lost, being given off to the surrounding air. He recommended,

therefore, that the cords experimented with be wrapped with some non-conductor--that it should be "insulated" ("isolee"), as he said, first making use of this term.

DUFAY DISCOVERS VITREOUS AND RESINOUS ELECTRICITY

It has been shown in an earlier chapter how Von Guericke discovered that light substances like feathers, after being attracted to the sulphur-ball electric-machine, were repelled by it until they touched some object. Von Guericke noted this, but failed to explain it satisfactorily. Dufay, repeating Von Guericke's experiments, found that if, while the excited tube or sulphur ball is driving the repelled feather before it, the ball be touched or rubbed anew, the feather comes to it again, and is repelled alternately, as, the hand touches the ball, or is withdrawn. From this he concluded that electrified bodies first attract bodies not electrified, "charge" them with electricity, and then repel them, the body so charged not being attracted again until it has discharged its electricity by touching something.

"On making the experiment related by Otto von Guericke," he says, "which consists in making a ball of sulphur rendered electrical to repel a down feather, I perceived that the same effects were produced not only by the tube, but by all electric bodies whatsoever, and I discovered that which accounts for a great part of the irregularities and, if I may use the term, of the caprices that seem to accompany most of the experiments on electricity. This principle is that electric bodies attract all that are not so, and repel them as soon as they are become electric by the vicinity or contact of the electric body. Thus gold-leaf is first attracted by the tube, and acquires an electricity by approaching it, and of consequence is immediately repelled by it. Nor is it reattracted while it retains its electric quality. But if while it is thus sustained in the air it chance to light on some other body, it straightway loses its electricity, and in consequence is reattracted by the tube, which, after having given it a new electricity, repels it a second time, which continues as long as the tube keeps its electricity. Upon applying this principle to the various experiments of electricity, one will be surprised at the number of obscure and puzzling facts that it clears up. For Mr. Hauksbee's famous experiment of the glass globe, in which silk threads are put, is a necessary consequence of it. When these threads are arranged in the form of rays by the electricity of the sides of the globe, if the finger be put near the outside of the globe the silk threads within fly from it, as is well known, which happens only because the finger or any other body applied near the glass globe is thereby rendered electrical, and consequently repels the silk threads which are endowed with the same quality. With a little reflection we may in the same manner account for most of the other phenomena, and which seem inexplicable without attending to this principle.

"Chance has thrown in my way another principle, more universal and remarkable than the preceding one, and which throws a new light on the subject of electricity. This principle is that there are two distinct electricities, very different from each other, one of which I call vitreous electricity and the other resinous electricity. The first is that of glass, rock-crystal, precious stones, hair of animals, wool, and many other bodies. The second is that of amber, copal, gumsack, silk thread, paper, and a number of other substances. The characteristic of these two electricities is that a body of the vitreous electricity, for example, repels all such as are of the same electricity, and on the contrary attracts all those of the resinous electricity; so that the tube, made electrical, will repel glass, crystal, hair of animals, etc., when rendered electric, and will attract silk thread, paper, etc., though rendered electrical likewise. Amber, on the contrary, will

attract electric glass and other substances of the same class, and will repel gum-sack, copal, silk thread, etc. Two silk ribbons rendered electrical will repel each other; two woollen threads will do the like; but a woollen thread and a silken thread will mutually attract each other. This principle very naturally explains why the ends of threads of silk or wool recede from each other, in the form of pencil or broom, when they have acquired an electric quality. From this principle one may with the same ease deduce the explanation of a great number of other phenomena; and it is probable that this truth will lead us to the further discovery of many other things.

"In order to know immediately to which of the two classes of electrics belongs any body whatsoever, one need only render electric a silk thread, which is known to be of the resinuous electricity, and see whether that body, rendered electrical, attracts or repels it. If it attracts it, it is certainly of the kind of electricity which I call VITREOUS; if, on the contrary, it repels it, it is of the same kind of electricity with the silk--that is, of the RESINOUS. I have likewise observed that communicated electricity retains the same properties; for if a ball of ivory or wood be set on a glass stand, and this ball be rendered electric by the tube, it will repel such substances as the tube repels; but if it be rendered electric by applying a cylinder of gum-sack near it, it will produce quite contrary effects--namely, precisely the same as gum-sack would produce. In order to succeed in these experiments, it is requisite that the two bodies which are put near each other, to find out the nature of their electricity, be rendered as electrical as possible, for if one of them was not at all or but weakly electrical, it would be attracted by the other, though it be of that sort that should naturally be repelled by it. But the experiment will always succeed perfectly well if both bodies are sufficiently electrical."[1]

As we now know, Dufay was wrong in supposing that there were two different kinds of electricity, vitreous and resinous. A little later the matter was explained by calling one "positive" electricity and the other "negative," and it was believed that certain substances produced only the one kind peculiar to that particular substance. We shall see presently, however, that some twenty years later an English scientist dispelled this illusion by producing both positive (or vitreous) and negative (or resinous) electricity on the same tube of glass at the same time.

After the death of Dufay his work was continued by his fellow-countryman Dr. Joseph Desaguliers, who was the first experimenter to electrify running water, and who was probably the first to suggest that clouds might be electrified bodies. But about, this time--that is, just before the middle of the eighteenth century--the field of greatest experimental activity was transferred to Germany, although both England and France were still active. The two German philosophers who accomplished most at this time were Christian August Hansen and George Matthias Bose, both professors in Leipsic. Both seem to have conceived the idea, simultaneously and independently, of generating electricity by revolving globes run by belt and wheel in much the same manner as the apparatus of Hauksbee.

With such machines it was possible to generate a much greater amount of electricity than Dufay had been able to do with the rubbed tube, and so equipped, the two German professors were able to generate electric sparks and jets of fire in a most startling manner. Bose in particular had a love for the spectacular, which he turned to account with his new electrical machine upon many occasions. On one of these occasions he prepared an elaborate dinner, to which a large number of distinguished guests were invited. Before the arrival of the company, however, Bose insulated the great banquet-table on cakes of pitch, and then connected it with a huge electrical machine

concealed in another room. All being ready, and the guests in their places about to be seated, Bose gave a secret signal for starting this machine, when, to the astonishment of the party, flames of fire shot from flowers, dishes, and viands, giving a most startling but beautiful display.

To add still further to the astonishment of his guests, Bose then presented a beautiful young lady, to whom each of the young men of the party was introduced. In some mysterious manner she was insulated and connected with the concealed electrical machine, so that as each gallant touched her fingertips he received an electric shock that "made him reel." Not content with this, the host invited the young men to kiss the beautiful maid. But those who were bold enough to attempt it received an electric shock that nearly "knocked their teeth out," as the professor tells it.

LUDOLFF'S EXPERIMENT WITH THE ELECTRIC SPARK

But Bose was only one of several German scientists who were making elaborate experiments. While Bose was constructing and experimenting with his huge machine, another German, Christian Friedrich Ludolff, demonstrated that electric sparks are actual fire--a fact long suspected but hitherto unproved. Ludolff's discovery, as it chanced, was made in the lecture-hall of the reorganized Academy of Sciences at Berlin, before an audience of scientists and great personages, at the opening lecture in 1744.

In the course of this lecture on electricity, during which some of the well-known manifestations of electricity were being shown, it occurred to Ludolff to attempt to ignite some inflammable fluid by projecting an electric spark upon its surface with a glass rod. This idea was suggested to him while performing the familiar experiment of producing a spark on the surface of a bowl of water by touching it with a charged glass rod. He announced to his audience the experiment he was about to attempt, and having warmed a spoonful of sulphuric ether, he touched its surface with the glass rod, causing it to burst into flame. This experiment left no room for doubt that the electric spark was actual fire.

As soon as this experiment of Ludolff's was made known to Bose, he immediately claimed that he had previously made similar demonstrations on various inflammable substances, both liquid and solid; and it seems highly probable that he had done so, as he was constantly experimenting with the sparks, and must almost certainly have set certain substances ablaze by accident, if not by intent. At all events, he carried on a series of experiments along this line to good purpose, finally succeeding in exploding gun-powder, and so making the first forerunner of the electric fuses now so universally used in blasting, firing cannon, and other similar purposes. It was Bose also who, observing some of the peculiar manifestations in electrified tubes, and noticing their resemblance to "northern lights," was one of the first, if not the first, to suggest that the aurora borealis is of electric origin.

These spectacular demonstrations had the effect of calling public attention to the fact that electricity is a most wonderful and mysterious thing, to say the least, and kept both scientists and laymen agog with expectancy. Bose himself was aflame with excitement, and so determined in his efforts to produce still stronger electric currents, that he sacrificed the tube of his twenty-foot telescope for the construction of a mammoth electrical machine. With this great machine a discharge of electricity was generated powerful enough to wound the skin when it happened to strike it.

Until this time electricity had been little more than a plaything of the scientists--or, at least, no practical use had been made of it. As it was a practising physician, Gilbert, who first laid the foundation for experimenting with the new substance, so again it was a medical man who first attempted to put it to practical use, and that in the field of his profession. Gottlieb Kruger, a professor of medicine at Halle in 1743, suggested that electricity might be of use in some branches of medicine; and the year following Christian Gottlieb Kratzenstein made a first experiment to determine the effects of electricity upon the body. He found that "the action of the heart was accelerated, the circulation increased, and that muscles were made to contract by the discharge": and he began at once administering electricity in the treatment of certain diseases. He found that it acted beneficially in rheumatic affections, and that it was particularly useful in certain nervous diseases, such as palsies. This was over a century ago, and to-day about the most important use made of the particular kind of electricity with which he experimented (the static, or frictional) is for the treatment of diseases affecting the nervous system.

By the middle of the century a perfect mania for making electrical machines had spread over Europe, and the whirling, hand-rubbed globes were gradually replaced by great cylinders rubbed by woollen cloths or pads, and generating an "enormous power of electricity." These cylinders were run by belts and foot-treadles, and gave a more powerful, constant, and satisfactory current than known heretofore. While making experiments with one of these machines, Johann Heinrichs Winkler attempted to measure the speed at which electricity travels. To do this he extended a cord suspended on silk threads, with the end attached to the machine and the end which was to attract the bits of gold-leaf near enough together so that the operator could watch and measure the interval of time that elapsed between the starting of the current along the cord and its attracting the gold-leaf. The length of the cord used in this experiment was only a little over a hundred feet, and this was, of course, entirely inadequate, the current travelling that space apparently instantaneously.

The improved method of generating electricity that had come into general use made several of the scientists again turn their attention more particularly to attempt putting it to some practical account. They were stimulated to these efforts by the constant reproaches that were beginning to be heard on all sides that electricity was merely a "philosopher's plaything." One of the first to succeed in inventing something that approached a practical mechanical contrivance was Andrew Gordon, a Scotch Benedictine monk. He invented an electric bell which would ring automatically, and a little "motor," if it may be so called. And while neither of these inventions were of any practical importance in themselves, they were attempts in the right direction, and were the first ancestors of modern electric bells and motors, although the principle upon which they worked was entirely different from modern electrical machines. The motor was simply a wheel with several protruding metal points around its rim. These points were arranged to receive an electrical discharge from a frictional machine, the discharge causing the wheel to rotate. There was very little force given to this rotation, however, not enough, in fact, to make it possible to more than barely turn the wheel itself. Two more great discoveries, galvanism and electro-magnetic induction, were necessary before the practical motor became possible.

The sober Gordon had a taste for the spectacular almost equal to that of Bose. It was he who ignited a bowl of alcohol by turning a stream of electrified water upon it, thus presenting the seeming paradox of fire produced by a stream of water. Gordon also demonstrated the power of the electrical discharge by killing small birds and animals at a distance of two hundred ells, the

electricity being conveyed that distance through small wires.

THE LEYDEN JAR DISCOVERED

As yet no one had discovered that electricity could be stored, or generated in any way other than by some friction device. But very soon two experimenters, Dean von Kleist, of Camin, Pomerania, and Pieter van Musschenbroek, the famous teacher of Leyden, apparently independently, made the discovery of what has been known ever since as the Leyden jar. And although Musschenbroek is sometimes credited with being the discoverer, there can be no doubt that Von Kleist's discovery antedated his by a few months at least.

Von Kleist found that by a device made of a narrow-necked bottle containing alcohol or mercury, into which an iron nail was inserted, be was able to retain the charge of electricity, after electrifying this apparatus with the frictional machine. He made also a similar device, more closely resembling the modern Leyden jar, from a thermometer tube partly filled with water and a wire tipped with a ball of lead. With these devices he found that he could retain the charge of electricity for several hours, and could produce the usual electrical manifestations, even to igniting spirits, quite as well as with the frictional machine. These experiments were first made in October, 1745, and after a month of further experimenting, Von Kleist sent the following account of them to several of the leading scientists, among others, Dr. Lieberkuhn, in Berlin, and Dr. Kruger, of Halle.

"When a nail, or a piece of thick brass wire, is put into a small apothecary's phial and electrified, remarkable effects follow; but the phial must be very dry, or warm. I commonly rub it over beforehand with a finger on which I put some pounded chalk. If a little mercury or a few drops of spirit of wine be put into it, the experiment succeeds better. As soon as this phial and nail are removed from the electrifying-glass, or the prime conductor, to which it has been exposed, is taken away, it throws out a pencil of flame so long that, with this burning machine in my hand, I have taken above sixty steps in walking about my room. When it is electrified strongly, I can take it into another room and there fire spirits of wine with it. If while it is electrifying I put my finger, or a piece of gold which I hold in my hand, to the nail, I receive a shock which stuns my arms and shoulders.

"A tin tube, or a man, placed upon electrics, is electrified much stronger by this means than in the common way. When I present this phial and nail to a tin tube, which I have, fifteen feet long, nothing but experience can make a person believe how strongly it is electrified. I am persuaded," he adds, "that in this manner Mr. Bose would not have taken a second electrical kiss. Two thin glasses have been broken by the shock of it. It appears to me very extraordinary, that when this phial and nail are in contact with either conducting or non-conducting matter, the strong shock does not follow. I have cemented it to wood, metal, glass, sealing-wax, etc., when I have electrified without any great effect. The human body, therefore, must contribute something to it. This opinion is confirmed by my observing that unless I hold the phial in my hand I cannot fire spirits of wine with it."[2]

But it seems that none of the men who saw this account were able to repeat the experiment and produce the effects claimed by Von Kleist, and probably for this reason the discovery of the obscure Pomeranian was for a time lost sight of.

Musschenbroek's discovery was made within a short time after Von Kleist's--in fact, only a matter of about two months later. But the difference in the reputations of the two discoverers insured a very different reception for their discoveries. Musschenbroek was one of the foremost teachers of Europe, and so widely known that the great universities vied with each other, and kings were bidding, for his services. Naturally, any discovery made by such a famous person would soon be heralded from one end of Europe to the other. And so when this professor of Leyden made his discovery, the apparatus came to be called the "Leyden jar," for want of a better name. There can be little doubt that Musschenbroek made his discovery entirely independently of any knowledge of Von Kleist's, or, for that matter, without ever having heard of the Pomeranian, and his actions in the matter are entirely honorable.

His discovery was the result of an accident. While experimenting to determine the strength of electricity he suspended a gun-barrel, which he charged with electricity from a revolving glass globe. From the end of the gun-barrel opposite the globe was a brass wire, which extended into a glass jar partly filled with water. Musschenbroek held in one hand this jar, while with the other he attempted to draw sparks from the barrel. Suddenly he received a shock in the hand holding the jar, that "shook him like a stroke of lightning," and for a moment made him believe that "he was done for." Continuing his experiments, nevertheless, he found that if the jar were placed on a piece of metal on the table, a shock would be received by touching this piece of metal with one hand and touching the wire with the other--that is, a path was made for the electrical discharge through the body. This was practically the same experiment as made by Von Kleist with his bottle and nail, but carried one step farther, as it showed that the "jar" need not necessarily be held in the hand, as believed by Von Kleist. Further experiments, continued by many philosophers at the time, revealed what Von Kleist had already pointed out, that the electrified jar remained charged for some time.

Soon after this Daniel Gralath, wishing to obtain stronger discharges than could be had from a single Leyden jar, conceived the idea of combining several jars, thus for the first time grouping the generators in a "battery" which produced a discharge strong enough to kill birds and small animals. He also attempted to measure the strength of the discharges, but soon gave it up in despair, and the solution of this problem was left for late nineteenth-century scientists.

The advent of the Leyden jar, which made it possible to produce strong electrical discharges from a small and comparatively simple device, was followed by more spectacular demonstrations of various kinds all over Europe. These exhibitions aroused the interest of the kings and noblemen, so that electricity no longer remained a "plaything of the philosophers" alone, but of kings as well. A favorite demonstration was that of sending the electrical discharge through long lines of soldiers linked together by pieces of wire, the discharge causing them to "spring into the air simultaneously" in a most astonishing manner. A certain monk in Paris prepared a most elaborate series of demonstrations for the amusement of the king, among other things linking together an entire regiment of nine hundred men, causing them to perform simultaneous springs and contortions in a manner most amusing to the royal guests. But not all the experiments being made were of a purely spectacular character, although most of them accomplished little except in a negative way. The famous Abbe Nollet, for example, combined useful experiments with spectacular demonstrations, thus keeping up popular interest while aiding the cause of scientific electricity.

WILLIAM WATSON

Naturally, the new discoveries made necessary a new nomenclature, new words and electrical terms being constantly employed by the various writers of that day. Among these writers was the English scientist William Watson, who was not only a most prolific writer but a tireless investigator. Many of the words coined by him are now obsolete, but one at least, "circuit," still remains in use.

In 1746, a French scientist, Louis Guillaume le Monnier, bad made a circuit including metal and water by laying a chain half-way around the edge of a pond, a man at either end holding it. One of these men dipped his free hand in the water, the other presenting a Leyden jar to a rod suspended on a cork float on the water, both men receiving a shock simultaneously. Watson, a year later, attempted the same experiment on a larger scale. He laid a wire about twelve hundred feet long across Westminster Bridge over the Thames, bringing the ends to the water's edge on the opposite banks, a man at one end holding the wire and touching the water. A second man on the opposite side held the wire and a Leyden jar; and a third touched the jar with one hand, while with the other he grasped a wire that extended into the river. In this way they not only received the shock, but fired alcohol as readily across the stream as could be done in the laboratory. In this experiment Watson discovered the superiority of wire over chain as a conductor, rightly ascribing this superiority to the continuity of the metal.

Watson continued making similar experiments over longer watercourses, some of them as long as eight thousand feet, and while engaged in making one of these he made the discovery so essential to later inventions, that the earth could be used as part of the circuit in the same manner as bodies of water. Lengthening his wires he continued his experiments until a circuit of four miles was made, and still the electricity seemed to traverse the course instantaneously, and with apparently undiminished force, if the insulation was perfect.

BENJAMIN FRANKLIN

Watson's writings now carried the field of active discovery across the Atlantic, and for the first time an American scientist appeared--a scientist who not only rivalled, but excelled, his European contemporaries. Benjamin Franklin, of Philadelphia, coming into possession of some of Watson's books, became so interested in the experiments described in them that he began at once experimenting with electricity. In Watson's book were given directions for making various experiments, and these assisted Franklin in repeating the old experiments, and eventually adding new ones. Associated with Franklin, and equally interested and enthusiastic, if not equally successful in making discoveries, were three other men, Thomas Hopkinson, Philip Sing, and Ebenezer Kinnersley. These men worked together constantly, although it appears to have been Franklin who made independently the important discoveries, and formulated the famous Franklinian theory.

Working steadily, and keeping constantly in touch with the progress of the European investigators, Franklin soon made some experiments which he thought demonstrated some hitherto unknown phases of electrical manifestation. This was the effect of pointed bodies "in DRAWING OFF and THROWING OFF the electrical fire." In his description of this phenomenon, Franklin writes:

"Place an iron shot of three or four inches diameter on the mouth of a clean, dry, glass bottle. By a fine silken thread from the ceiling right over the mouth of the bottle, suspend a small cork ball, about the bigness of a marble; the thread of such a length that the cork ball may rest against the side of the shot. Electrify the shot, and the ball will be repelled to the distance of four or five inches, more or less, according to the quantity of electricity. When in this state, if you present to the shot the point of a long, slender shaft-bodkin, at six or eight inches distance, the repellency is instantly destroyed, and the cork flies to the shot. A blunt body must be brought within an inch, and draw a spark, to produce the same effect.

"To prove that the electrical fire is DRAWN OFF by the point, if you take the blade of the bodkin out of the wooden handle and fix it in a stick of sealing-wax, and then present it at the distance aforesaid, or if you bring it very near, no such effect follows; but sliding one finger along the wax till you touch the blade, and the ball flies to the shot immediately. If you present the point in the dark you will see, sometimes at a foot distance, and more, a light gather upon it like that of a fire-fly or glow-worm; the less sharp the point, the nearer you must bring it to observe the light; and at whatever distance you see the light, you may draw off the electrical fire and destroy the repellency. If a cork ball so suspended be repelled by the tube, and a point be presented quick to it, though at a considerable distance, 'tis surprising to see how suddenly it flies back to the tube. Points of wood will do as well as those of iron, provided the wood is not dry; for perfectly dry wood will no more conduct electricity than sealing-wax.

"To show that points will THROW OFF as well as DRAW OFF the electrical fire, lay a long, sharp needle upon the shot, and you cannot electrify the shot so as to make it repel the cork ball. Or fix a needle to the end of a suspended gun-barrel or iron rod, so as to point beyond it like a little bayonet, and while it remains there, the gun-barrel or rod cannot, by applying the tube to the other end, be electrified so as to give a spark, the fire continually running out silently at the point. In the dark you may see it make the same appearance as it does in the case before mentioned."[3]

Von Guericke, Hauksbee, and Gray had noticed that pointed bodies attracted electricity in a peculiar manner, but this demonstration of the "drawing off" of "electrical fire" was original with Franklin. Original also was the theory that he now suggested, which had at least the merit of being thinkable even by non-philosophical minds. It assumes that electricity is like a fluid, that will flow along conductors and accumulate in proper receptacles, very much as ordinary fluids do. This conception is probably entirely incorrect, but nevertheless it is likely to remain a popular one, at least outside of scientific circles, or until something equally tangible is substituted.

FRANKLIN'S THEORY OF ELECTRICITY

According to Franklin's theory, electricity exists in all bodies as a "common stock," and tends to seek and remain in a state of equilibrium, just as fluids naturally tend to seek a level. But it may, nevertheless, be raised or lowered, and this equilibrium be thus disturbed. If a body has more electricity than its normal amount it is said to be POSITIVELY electrified; but if it has less, it is NEGATIVELY electrified. An over-electrified or "plus" body tends to give its surplus stock to a body containing the normal amount; while the "minus" or under-electrified body will draw electricity from one containing the normal amount.

Working along lines suggested by this theory, Franklin attempted to show that electricity is not created by friction, but simply collected from its diversified state, the rubbed glass globe attracting a certain quantity of "electrical fire," but ever ready to give it up to any body that has less. He explained the charged Leyden jar by showing that the inner coating of tin-foil received more than the ordinary quantity of electricity, and in consequence is POSITIVELY electrified, while the outer coating, having the ordinary quantity of electricity diminished, is electrified NEGATIVELY.

These studies of the Leyden jar, and the studies of pieces of glass coated with sheet metal, led Franklin to invent his battery, constructed of eleven large glass plates coated with sheets of lead. With this machine, after overcoming some defects, he was able to produce electrical manifestations of great force--a force that "knew no bounds," as he declared ("except in the matter of expense and of labor"), and which could be made to exceed "the greatest know effects of common lightning."

This reference to lightning would seem to show Franklin's belief, even at that time, that lightning is electricity. Many eminent observers, such as Hauksbee, Wall, Gray, and Nollet, had noticed the resemblance between electric sparks and lightning, but none of these had more than surmised that the two might be identical. In 1746, the surgeon, John Freke, also asserted his belief in this identity. Winkler, shortly after this time, expressed the same belief, and, assuming that they were the same, declared that "there is no proof that they are of different natures"; and still he did not prove that they were the same nature.

FRANKLIN INVENTS THE LIGHTNING-ROD

Even before Franklin proved conclusively the nature of lightning, his experiments in drawing off the electric charge with points led to some practical suggestions which resulted in the invention of the lightning-rod. In the letter of July, 1750, which he wrote on the subject, he gave careful instructions as to the way in which these rods might be constructed. In part Franklin wrote: "May not the knowledge of this power of points be of use to mankind in preserving houses, churches, ships, etc., from the stroke of lightning by directing us to fix on the highest parts of the edifices upright rods of iron made sharp as a needle, and gilt to prevent rusting, and from the foot of these rods a wire down the outside of the building into the grounds, or down round one of the shrouds of a ship and down her side till it reaches the water? Would not these pointed rods probably draw the electrical fire silently out of a cloud before it came nigh enough to strike, and thereby secure us from that most sudden and terrible mischief?

"To determine this question, whether the clouds that contain the lightning are electrified or not, I propose an experiment to be tried where it may be done conveniently. On the top of some high tower or steeple, place a kind of sentry-box, big enough to contain a man and an electrical stand. From the middle of the stand let an iron rod rise and pass, bending out of the door, and then upright twenty or thirty feet, pointed very sharp at the end. If the electrical stand be kept clean and dry, a man standing on it when such clouds are passing low might be electrified and afford sparks, the rod drawing fire to him from a cloud. If any danger to the man be apprehended (though I think there would be none), let him stand on the floor of his box and now and then bring near to the rod the loop of a wire that has one end fastened to the leads, he holding it by a wax handle; so the sparks, if the rod is electrified, will strike from the rod to the wire and not effect him."[4]

Not satisfied with all the evidence that he had collected pointing to the identity of lightning and electricity, he adds one more striking and very suggestive piece of evidence. Lightning was known sometimes to strike persons blind without killing them. In experimenting on pigeons and pullets with his electrical machine, Franklin found that a fowl, when not killed outright, was sometimes rendered blind. The report of these experiments were incorporated in this famous letter of the Philadelphia philosopher.

The attitude of the Royal Society towards this clearly stated letter, with its useful suggestions, must always remain as a blot on the record of this usually very receptive and liberal-minded body. Far from publishing it or receiving it at all, they derided the whole matter as too visionary for discussion by the society. How was it possible that any great scientific discovery could be made by a self-educated colonial newspaper editor, who knew nothing of European science except by hearsay, when all the great scientific minds of Europe had failed to make the discovery? How indeed! And yet it would seem that if any of the influential members of the learned society had taken the trouble to read over Franklin's clearly stated letter, they could hardly have failed to see that his suggestions were worthy of consideration. But at all events, whether they did or did not matters little. The fact remains that they refused to consider the paper seriously at the time; and later on, when its true value became known, were obliged to acknowledge their error by a tardy report on the already well-known document.

But if English scientists were cold in their reception of Franklin's theory and suggestions, the French scientists were not. Buffon, perceiving at once the importance of some of Franklin's experiments, took steps to have the famous letter translated into French, and soon not only the savants, but members of the court and the king himself were intensely interested. Two scientists, De Lor and D'Alibard, undertook to test the truth of Franklin's suggestions as to pointed rods "drawing off lightning." In a garden near Paris, the latter erected a pointed iron rod fifty feet high and an inch in diameter. As no thunder-clouds appeared for several days, a guard was stationed, armed with an insulated brass wire, who was directed to test the iron rods with it in case a storm came on during D'Alibard's absence. The storm did come on, and the guard, not waiting for his employer's arrival, seized the wire and touched the rod. Instantly there was a report. Sparks flew and the guard received such a shock that he thought his time had come. Believing from his outcry that he was mortally hurt, his friends rushed for a spiritual adviser, who came running through rain and hail to administer the last rites; but when he found the guard still alive and uninjured, he turned his visit to account by testing the rod himself several times, and later writing a report of his experiments to M. d'Alibard. This scientist at once reported the affair to the French Academy, remarking that "Franklin's idea was no longer a conjecture, but a reality."

FRANKLIN PROVES THAT LIGHTNING IS ELECTRICITY

Europe, hitherto somewhat sceptical of Franklin's views, was by this time convinced of the identity of lightning and electricity. It was now Franklin's turn to be sceptical. To him the fact that a rod, one hundred feet high, became electrified during a storm did not necessarily prove that the storm-clouds were electrified. A rod of that length was not really projected into the cloud, for even a very low thunder-cloud was more than a hundred feet above the ground. Irrefutable proof could only be had, as he saw it, by "extracting" the lightning with something actually sent up into the storm-cloud; and to accomplish this Franklin made his silk kite, with which he finally demonstrated to his own and the world's satisfaction that his theory was correct.

Taking his kite out into an open common on the approach of a thunder-storm, he flew it well up into the threatening clouds, and then, touching, the suspended key with his knuckle, received the electric spark; and a little later he charged a Leyden jar from the electricity drawn from the clouds with his kite.

In a brief but direct letter, he sent an account of his kite and his experiment to England:

"Make a small cross of two light strips of cedar," he wrote, "the arms so long as to reach to the four corners of a large, thin, silk handkerchief when extended; tie the corners of the handkerchief to the extremities of the cross so you have the body of a kite; which being properly accommodated with a tail, loop, and string, will rise in the air like those made of paper; but this being of silk is fitter to bear the wind and wet of a thunder-gust without tearing. To the top of the upright stick of the cross is to be fixed a very sharp-pointed wire, rising a foot or more above the wood. To the end of the twine, next the hand, is to be tied a silk ribbon; where the silk and twine join a key may be fastened. This kite is to be raised when a thunder-gust appears to be coming on, and the person who holds the string must stand within a door or window or under some cover, so that the silk ribbon may not be wet; and care must be taken that the twine does not touch the frame of the door or window. As soon as any of the thunder-clouds come over the kite, the pointed wire will draw the electric fire from them, and the kite, with all the twine, will be electrified and the loose filaments will stand out everywhere and be attracted by the approaching finger, and when the rain has wet the kite and twine so that it can conduct the electric fire freely, you will find it stream out plentifully from the key on the approach of your knuckle, and with this key the phial may be charged; and from electric fire thus obtained spirits may be kindled and all other electric experiments performed which are usually done by the help of a rubbed glass globe or tube, and thereby the sameness of the electric matter with that of lightning completely demonstrated."[5]

In experimenting with lightning and Franklin's pointed rods in Europe, several scientists received severe shocks, in one case with a fatal result. Professor Richman, of St. Petersburg, while experimenting during a thunder-storm, with an iron rod which he had erected on his house, received a shock that killed him instantly.

About 1733, as we have seen, Dufay had demonstrated that there were two apparently different kinds of electricity; one called VITREOUS because produced by rubbing glass, and the other RESINOUS because produced by rubbed resinous bodies. Dufay supposed that these two apparently different electricities could only be produced by their respective substances; but twenty years later, John Canton (1715-1772), an Englishman, demonstrated that under certain conditions both might be produced by rubbing the same substance. Canton's experiment, made upon a glass tube with a roughened surface, proved that if the surface of the tube were rubbed with oiled silk, vitreous or positive electricity was produced, but if rubbed with flannel, resinous electricity was produced. He discovered still further that both kinds could be excited on the same tube simultaneously with a single rubber. To demonstrate this he used a tube, one-half of which had a roughened the other a glazed surface. With a single stroke of the rubber he was able to excite both kinds of electricity on this tube. He found also that certain substances, such as glass and amber, were electrified positively when taken out of mercury, and this led to his important discovery that an amalgam of mercury and tin, when used on the surface of the rubber, was very effective in exciting glass.

XV. NATURAL HISTORY TO THE TIME OF LINNAeUS

Modern systematic botany and zoology are usually held to have their beginnings with Linnaeus. But there were certain precursors of the famous Swedish naturalist, some of them antedating him by more than a century, whose work must not be altogether ignored--such men as Konrad Gesner (1516-1565), Andreas Caesalpinus (1579-1603), Francisco Redi (1618-1676), Giovanni Alfonso Borelli (1608-1679), John Ray (1628-1705), Robert Hooke (1635-1703), John Swammerdam (1637-1680), Marcello Malpighi (1628-1694), Nehemiah Grew (1628-1711), Joseph Tournefort (1656-1708), Rudolf Jacob Camerarius (1665-1721), and Stephen Hales (1677-1761). The last named of these was, to be sure, a contemporary of Linnaeus himself, but Gesner and Caesalpinus belong, it will be observed, to so remote an epoch as that of Copernicus.

Reference has been made in an earlier chapter to the microscopic investigations of Marcello Malpighi, who, as there related, was the first observer who actually saw blood corpuscles pass through the capillaries. Another feat of this earliest of great microscopists was to dissect muscular tissue, and thus become the father of microscopic anatomy. But Malpighi did not confine his observations to animal tissues. He dissected plants as well, and he is almost as fully entitled to be called the father of vegetable anatomy, though here his honors are shared by the Englishman Grew. In 1681, while Malpighi's work, Anatomia plantarum, was on its way to the Royal Society for publication, Grew's Anatomy of Vegetables was in the hands of the publishers, making its appearance a few months earlier than the work of the great Italian. Grew's book was epoch-marking in pointing out the sex-differences in plants.

Robert Hooke developed the microscope, and took the first steps towards studying vegetable anatomy, publishing in 1667, among other results, the discovery of the cellular structure of cork. Hooke applied the name "cell" for the first time in this connection. These discoveries of Hooke, Malpighi, and Grew, and the discovery of the circulation of the blood by William Harvey shortly before, had called attention to the similarity of animal and vegetable structures. Hales made a series of investigations upon animals to determine the force of the blood pressure; and similarly he made numerous statical experiments to determine the pressure of the flow of sap in vegetables. His Vegetable Statics, published in 1727, was the first important work on the subject of vegetable physiology, and for this reason Hales has been called the father of this branch of science.

In botany, as well as in zoology, the classifications of Linnaeus of course supplanted all preceding classifications, for the obvious reason that they were much more satisfactory; but his work was a culmination of many similar and more or less satisfactory attempts of his predecessors. About the year 1670 Dr. Robert Morison (1620-1683), of Aberdeen, published a classification of plants, his system taking into account the woody or herbaceous structure, as well as the flowers and fruit. This classification was supplanted twelve years later by the classification of Ray, who arranged all known vegetables into thirty-three classes, the basis of this classification being the fruit. A few years later Rivinus, a professor of botany in the University of Leipzig, made still another classification, determining the distinguishing character chiefly from the flower, and Camerarius and Tournefort also made elaborate classifications. On the Continent Tournefort's classification was the most popular until the time of Linnaeus, his systematic arrangement including about eight thousand species of plants, arranged chiefly according to the form of the corolla.

Most of these early workers gave attention to both vegetable and animal kingdoms. They were called naturalists, and the field of their investigations was spoken of as "natural history." The specialization of knowledge had not reached that later stage in which botanist, zoologist, and physiologist felt their labors to be sharply divided. Such a division was becoming more and more necessary as the field of knowledge extended; but it did not become imperative until long after the time of Linnaeus. That naturalist himself, as we shall see, was equally distinguished as botanist and as zoologist. His great task of organizing knowledge was applied to the entire range of living things.

Carolus Linnaeus was born in the town of Rashult, in Sweden, on May 13, 1707. As a child he showed great aptitude in learning botanical names, and remembering facts about various plants as told him by his father. His eagerness for knowledge did not extend to the ordinary primary studies, however, and, aside from the single exception of the study of physiology, he proved himself an indifferent pupil. His backwardness was a sore trial to his father, who was desirous that his son should enter the ministry; but as the young Linnaeus showed no liking for that calling, and as he had acquitted himself well in his study of physiology, his father at last decided to allow him to take up the study of medicine. Here at last was a field more to the liking of the boy, who soon vied with the best of his fellow-students for first honors. Meanwhile he kept steadily at work in his study of natural history, acquiring considerable knowledge of ornithology, entomology, and botany, and adding continually to his collection of botanical specimens. In 1729 his botanical knowledge was brought to the attention of Olaf Rudbeck, professor of botany in the University of Upsala, by a short paper on the sexes of plants which Linnaeus had prepared. Rudbeck was so impressed by some of the ideas expressed in this paper that he appointed the author as his assistant the following year.

This was the beginning of Linnaes's career as a botanist. The academic gardens were thus thrown open to him, and he found time at his disposal for pursuing his studies between lecture hours and in the evenings. It was at this time that he began the preparation of his work the Systema naturae, the first of his great works, containing a comprehensive sketch of the whole field of natural history. When this work was published, the clearness of the views expressed and the systematic arrangement of the various classifications excited great astonishment and admiration, and placed Linaeus at once in the foremost rank of naturalists. This work was followed shortly by other publications, mostly on botanical subjects, in which, among other things, he worked out in detail his famous "system."

This system is founded on the sexes of plants, and is usually referred to as an "artificial method" of classification because it takes into account only a few marked characters of plants, without uniting them by more general natural affinities. At the present time it is considered only as a stepping-stone to the "natural" system; but at the time of its promulgation it was epoch-marking in its directness and simplicity, and therefore superiority, over any existing systems.

One of the great reforms effected by Linnaeus was in the matter of scientific terminology. Technical terms are absolutely necessary to scientific progress, and particularly so in botany, where obscurity, ambiguity, or prolixity in descriptions are fatally misleading. Linnaeus's work contains something like a thousand terms, whose meanings and uses are carefully explained. Such an array seems at first glance arbitrary and unnecessary, but the fact that it has remained in use for something like two centuries is indisputable evidence of its practicality. The descriptive

language of botany, as employed by Linnaeus, still stands as a model for all other subjects.

Closely allied to botanical terminology is the subject of botanical nomenclature. The old method of using a number of Latin words to describe each different plant is obviously too cumbersome, and several attempts had been made prior to the time of Linnaeus to substitute simpler methods. Linnaeus himself made several unsatisfactory attempts before he finally hit upon his system of "trivial names," which was developed in his Species plantarum, and which, with some, minor alterations, remains in use to this day. The essence of the system is the introduction of binomial nomenclature--that is to say, the use of two names and no more to designate any single species of animal or plant. The principle is quite the same as that according to which in modern society a man has two names, let us say, John Doe, the one designating his family, the other being individual. Similarly each species of animal or plant, according to the Linnaeean system, received a specific or "trivial" name; while various species, associated according to their seeming natural affinities into groups called genera, were given the same generic name. Thus the generic name given all members of the cat tribe being Felis, the name Felis leo designates the lion; Felis pardus, the leopard; Felis domestica, the house cat, and so on. This seems perfectly simple and natural now, but to understand how great a reform the binomial nomenclature introduced we have but to consult the work of Linnaeus's predecessors. A single illustration will suffice. There is, for example, a kind of grass, in referring to which the naturalist anterior to Linnaeus, if he would be absolutely unambiguous, was obliged to use the following descriptive formula: Gramen Xerampelino, Miliacea, praetenuis ramosaque sparsa panicula, sive Xerampelino congener, arvense, aestivum; gramen minutissimo semine. Linnaeus gave to this plant the name Poa bulbosa--a name that sufficed, according to the new system, to distinguish this from every other species of vegetable. It does not require any special knowledge to appreciate the advantage of such a simplification.

While visiting Paris in 1738 Linnaeus met and botanized with the two botanists whose "natural method" of classification was later to supplant his own "artificial system." These were Bernard and Antoine Laurent de Jussieu. The efforts of these two scientists were directed towards obtaining a system which should aim at clearness, simplicity, and precision, and at the same time be governed by the natural affinities of plants. The natural system, as finally propounded by them, is based on the number of cotyledons, the structure of the seed, and the insertion of the stamens. Succeeding writers on botany have made various modifications of this system, but nevertheless it stands as the foundation-stone of modern botanical classification.

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[For notes and bibliography to vol. II. see vol. V.]

A HISTORY OF SCIENCE

BY

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IN FIVE VOLUMES

VOLUME III. MODERN DEVELOPMENT OF THE PHYSICAL SCIENCES

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A HISTORY OF SCIENCE- BOOK III MODERN DEVELOPMENT OF THE PHYSICAL SCIENCES

With the present book we enter the field of the distinctively modern. There is no precise date at which we take up each of the successive stories, but the main sweep of development has to do in each case with the nineteenth century. We shall see at once that this is a time both of rapid progress and of great differentiation. We have heard almost nothing hitherto of such sciences as paleontology, geology, and meteorology, each of which now demands full attention. Meantime, astronomy and what the workers of the elder day called natural philosophy become wonderfully diversified and present numerous phases that would have been startling enough to the star-gazers and philosophers of the earlier epoch.

Thus, for example, in the field of astronomy, Herschel is able, thanks to his perfected telescope, to discover a new planet and then to reach out into the depths of space and gain such knowledge of stars and nebulae as hitherto no one had more than dreamed of. Then, in rapid sequence, a whole coterie of hitherto unsuspected minor planets is discovered, stellar distances are measured, some members of the starry galaxy are timed in their flight, the direction of movement of the solar system itself is investigated, the spectroscope reveals the chemical composition even of suns that

are unthinkably distant, and a tangible theory is grasped of the universal cycle which includes the birth and death of worlds.

Similarly the new studies of the earth's surface reveal secrets of planetary formation hitherto quite inscrutable. It becomes known that the strata of the earth's surface have been forming throughout untold ages, and that successive populations differing utterly from one another have peopled the earth in different geological epochs. The entire point of view of thoughtful men becomes changed in contemplating the history of the world in which we live--albeit the newest thought harks back to some extent to those days when the inspired thinkers of early Greece dreamed out the wonderful theories with which our earlier chapters have made our readers familiar.

In the region of natural philosophy progress is no less pronounced and no less striking. It suffices here, however, by way of anticipation, simply to name the greatest generalization of the century in physical science--the doctrine of the conservation of energy.

I THE SUCCESSORS OF NEWTON IN ASTRONOMY

HEVELIUS AND HALLEY

STRANGELY enough, the decade immediately following Newton was one of comparative barrenness in scientific progress, the early years of the eighteenth century not being as productive of great astronomers as the later years of the seventeenth, or, for that matter, as the later years of the eighteenth century itself. Several of the prominent astronomers of the later seventeenth century lived on into the opening years of the following century, however, and the younger generation soon developed a coterie of astronomers, among whom Euler, Lagrange, Laplace, and Herschel, as we shall see, were to accomplish great things in this field before the century closed.

One of the great seventeenth-century astronomers, who died just before the close of the century, was Johannes Hevelius (1611-1687), of Dantzig, who advanced astronomy by his accurate description of the face and the spots of the moon. But he is remembered also for having retarded progress by his influence in refusing to use telescopic sights in his observations, preferring until his death the plain sights long before discarded by most other astronomers. The advantages of these telescope sights have been discussed under the article treating of Robert Hooke, but no such advantages were ever recognized by Hevelius. So great was Hevelius's reputation as an astronomer that his refusal to recognize the advantage of the telescope sights caused many astronomers to hesitate before accepting them as superior to the plain; and even the famous Halley, of whom we shall speak further in a moment, was sufficiently in doubt over the matter to pay the aged astronomer a visit to test his skill in using the old-style sights. Side by side, Hevelius and Halley made their observations, Hevelius with his old instrument and Halley with the new. The results showed slightly in the younger man's favor, but not enough to make it an entirely convincing demonstration. The explanation of this, however, did not lie in the lack of superiority of the telescopic instrument, but rather in the marvellous skill of the aged Hevelius, whose dexterity almost compensated for the defect of his instrument. What he might have accomplished could he have been induced to adopt the telescope can only be surmised.

Halley himself was by no means a tyro in matters astronomical at that time. As the only son of a wealthy soap-boiler living near London, he had been given a liberal education, and even before leaving college made such novel scientific observations as that of the change in the variation of the compass. At nineteen years of age he discovered a new method of determining the elements of the planetary orbits which was a distinct improvement over the old. The year following he sailed for the Island of St, Helena to make observations of the heavens in the southern hemisphere.

It was while in St. Helena that Halley made his famous observation of the transit of Mercury over the sun's disk, this observation being connected, indirectly at least, with his discovery of a method of determining the parallax of the planets. By parallax is meant the apparent change in the position of an object, due really to a change in the position of the observer. Thus, if we imagine two astronomers making observations of the sun from opposite sides of the earth at the same time, it is obvious that to these observers the sun will appear to be at two different points in the sky. Half the angle measuring this difference would be known as the sun's parallax. This would depend, then, upon the distance of the earth from the sun and the length of the earth's radius. Since the actual length of this radius has been determined, the parallax of any heavenly body enables the astronomer to determine its exact distance.

The parallaxes can be determined equally well, however, if two observers are separated by exactly known distances, several hundreds or thousands of miles apart. In the case of a transit of Venus across the sun's disk, for example, an observer at New York notes the image of the planet moving across the sun's disk, and notes also the exact time of this observation. In the same manner an observer at London makes similar observations. Knowing the distance between New York and London, and the different time of the passage, it is thus possible to calculate the difference of the parallaxes of the sun and a planet crossing its disk. The idea of thus determining the parallax of the planets originated, or at least was developed, by Halley, and from this phenomenon he thought it possible to conclude the dimensions of all the planetary orbits. As we shall see further on, his views were found to be correct by later astronomers.

In 1721 Halley succeeded Flamsteed as astronomer royal at the Greenwich Observatory. Although sixty- four years of age at that time his activity in astronomy continued unabated for another score of years. At Greenwich he undertook some tedious observations of the moon, and during those observations was first to detect the acceleration of mean motion. He was unable to explain this, however, and it remained for Laplace in the closing years of the century to do so, as we shall see later.

Halley's book, the Synopsis Astronomiae Cometicae, is one of the most valuable additions to astronomical literature since the time of Kepler. He was first to attempt the calculation of the orbit of a comet, having revived the ancient opinion that comets belong to the solar system, moving in eccentric orbits round the sun, and his calculation of the orbit of the comet of 1682 led him to predict correctly the return of that comet in 1758. Halley's Study of Meteors.

Like other astronomers of his time be was greatly puzzled over the well-known phenomena of shooting- stars, or meteors, making many observations himself, and examining carefully the observations of other astronomers. In 1714 he gave his views as to the origin and composition of

these mysterious visitors in the earth's atmosphere. As this subject will be again referred to in a later chapter, Halley's views, representing the most advanced views of his age, are of interest.

"The theory of the air seemeth at present," he says, "to be perfectly well understood, and the differing densities thereof at all altitudes; for supposing the same air to occupy spaces reciprocally proportional to the quantity of the superior or incumbent air, I have elsewhere proved that at forty miles high the air is rarer than at the surface of the earth at three thousand times; and that the utmost height of the atmosphere, which reflects light in the Crepusculum, is not fully forty-five miles, notwithstanding which 'tis still manifest that some sort of vapors, and those in no small quantity, arise nearly to that height. An instance of this may be given in the great light the society had an account of (vide Transact. Sep., 1676) from Dr. Wallis, which was seen in very distant counties almost over all the south part of England. Of which though the doctor could not get so particular a relation as was requisite to determine the height thereof, yet from the distant places it was seen in, it could not but be very many miles high.

"So likewise that meteor which was seen in 1708, on the 31st of July, between nine and ten o'clock at night, was evidently between forty and fifty miles perpendicularly high, and as near as I can gather, over Shereness and the buoy on the Nore. For it was seen at London moving horizontally from east by north to east by south at least fifty degrees high, and at Redgrove, in Suffolk, on the Yarmouth road, about twenty miles from the east coast of England, and at least forty miles to the eastward of London, it appeared a little to the westward of the south, suppose south by west, and was seen about thirty degrees high, sliding obliquely downward. I was shown in both places the situation thereof, which was as described, but could wish some person skilled in astronomical matters bad seen it, that we might pronounce concerning its height with more certainty. Yet, as it is, we may securely conclude that it was not many more miles westerly than Redgrove, which, as I said before, is about forty miles more easterly than London. Suppose it, therefore, where perpendicular, to have been thirty-five miles east from London, and by the altitude it appeared at in London-- viz., fifty degrees, its tangent will be forty-two miles, for the height of the meteor above the surface of the earth; which also is rather of the least, because the altitude of the place shown me is rather more than less than fifty degrees; and the like may be concluded from the altitude it appeared in at Redgrove, near seventy miles distant. Though at this very great distance, it appeared to move with an incredible velocity, darting, in a very few seconds of time, for about twelve degrees of a great circle from north to south, being very bright at its first appearance; and it died away at the east of its course, leaving for some time a pale whiteness in the place, with some remains of it in the track where it had gone; but no hissing sound as it passed, or bounce of an explosion were heard.

"It may deserve the honorable society's thoughts, how so great a quantity of vapor should be raised to the top of the atmosphere, and there collected, so as upon its ascension or otherwise illumination, to give a light to a circle of above one hundred miles diameter, not much inferior to the light of the moon; so as one might see to take a pin from the ground in the otherwise dark night. 'Tis hard to conceive what sort of exhalations should rise from the earth, either by the action of the sun or subterranean heat, so as to surmount the extreme cold and rareness of the air in those upper regions: but the fact is indisputable, and therefore requires a solution."

From this much of the paper it appears that there was a general belief that this burning mass was heated vapor thrown off from the earth in some mysterious manner, yet this is unsatisfactory to Halley, for after citing various other meteors that have appeared within his knowledge, he goes on to say:

"What sort of substance it must be, that could be so impelled and ignited at the same time; there being no Vulcano or other Spiraculum of subterraneous fire in the northeast parts of the world, that we ever yet heard of, from whence it might be projected.

"I have much considered this appearance, and think it one of the hardest things to account for that I have yet met with in the phenomena of meteors, and I am induced to think that it must be some collection of matter formed in the aether, as it were, by some fortuitous concourse of atoms, and that the earth met with it as it passed along in its orb, then but newly formed, and before it had conceived any great impetus of descent towards the sun. For the direction of it was exactly opposite to that of the earth, which made an angle with the meridian at that time of sixty-seven gr., that is, its course was from west southwest to east northeast, wherefore the meteor seemed to move the contrary way. And besides falling into the power of the earth's gravity, and losing its motion from the opposition of the medium, it seems that it descended towards the earth, and was extinguished in the Tyrrhene Sea, to the west southwest of Leghorn. The great blow being heard upon its first immersion into the water, and the rattling like the driving of a cart over stones being what succeeded upon its quenching; something like this is always heard upon quenching a very hot iron in water. These facts being past dispute, I would be glad to have the opinion of the learned thereon, and what objection can be reasonably made against the above hypothesis, which I humbly submit to their censure."[1]

These few paragraphs, coming as they do from a leading eighteenth-century astronomer, convey more clearly than any comment the actual state of the meteorological learning at that time. That this ball of fire, rushing "at a greater velocity than the swiftest cannon-ball," was simply a mass of heated rock passing through our atmosphere, did not occur to him, or at least was not credited. Nor is this surprising when we reflect that at that time universal gravitation had been but recently discovered; heat had not as yet been recognized as simply a form of motion; and thunder and lightning were unexplained mysteries, not to be explained for another three-quarters of a century. In the chapter on meteorology we shall see how the solution of this mystery that puzzled Halley and his associates all their lives was finally attained.

BRADLEY AND THE ABERRATION OF LIGHT

Halley was succeeded as astronomer royal by a man whose useful additions to the science were not to be recognized or appreciated fully until brought to light by the Prussian astronomer Bessel early in the nineteenth century. This was Dr. James Bradley, an ecclesiastic, who ranks as one of the most eminent astronomers of the eighteenth century. His most remarkable discovery was the explanation of a peculiar motion of the pole-star, first observed, but not explained, by Picard a century before. For many years a satisfactory explanation was sought unsuccessfully by Bradley and his fellow-astronomers, but at last he was able to demonstrate that the stary Draconis, on which he was making his observations, described, or appeared to describe, a small ellipse. If this observation was correct, it afforded a means of computing the aberration of any star at all times.

The explanation of the physical cause of this aberration, as Bradley thought, and afterwards demonstrated, was the result of the combination of the motion of light with the annual motion of the earth. Bradley first formulated this theory in 1728, but it was not until 1748--twenty years of continuous struggle and observation by him--that he was prepared to communicate the results of his efforts to the Royal Society. This remarkable paper is thought by the Frenchman, Delambre, to entitle its author to a place in science beside such astronomers as Hipparcbus and Kepler.

Bradley's studies led him to discover also the libratory motion of the earth's axis. "As this appearance of g Draconis. indicated a diminution of the inclination of the earth's axis to the plane of the ecliptic," he says; "and as several astronomers have supposed THAT inclination to diminish regularly; if this phenomenon depended upon such a cause, and amounted to 18" in nine years, the obliquity of the ecliptic would, at that rate, alter a whole minute in thirty years; which is much faster than any observations, before made, would allow. I had reason, therefore, to think that some part of this motion at the least, if not the whole, was owing to the moon's action upon the equatorial parts of the earth; which, I conceived, might cause a libratory motion of the earth's axis. But as I was unable to judge, from only nine years observations, whether the axis would entirely recover the same position that it had in the year 1727, I found it necessary to continue my observations through a whole period of the moon's nodes; at the end of which I had the satisfaction to see, that the stars, returned into the same position again; as if there had been no alteration at all in the inclination of the earth's axis; which fully convinced me that I had guessed rightly as to the cause of the phenomena. This circumstance proves likewise, that if there be a gradual diminution of the obliquity of the ecliptic, it does not arise only from an alteration in the position of the earth's axis, but rather from some change in the plane of the ecliptic itself; because the stars, at the end of the period of the moon's nodes, appeared in the same places, with respect to the equator, as they ought to have done, if the earth's axis had retained the same inclination to an invariable plane."[2]

FRENCH ASTRONOMERS

Meanwhile, astronomers across the channel were by no means idle. In France several successful observers were making many additions to the already long list of observations of the first astronomer of the Royal Observatory of Paris, Dominic Cassini (1625-1712), whose reputation among his contemporaries was much greater than among succeeding generations of astronomers. Perhaps the most deserving of these successors was Nicolas Louis de Lacaille (1713-1762), a theologian who had been educated at the expense of the Duke of Bourbon, and who, soon after completing his clerical studies, came under the patronage of Cassini, whose attention had been called to the young man's interest in the sciences. One of Lacaille's first under-takings was the remeasuring of the French are of the meridian, which had been incorrectly measured by his patron in 1684. This was begun in 1739, and occupied him for two years before successfully completed. As a reward, however, he was admitted to the academy and appointed mathematical professor in Mazarin College.

In 1751 he went to the Cape of Good Hope for the purpose of determining the sun's parallax by observations of the parallaxes of Mars and Venus, and incidentally to make observations on the other southern hemisphere stars. The results of this undertaking were most successful, and were given in his Coelum australe stelligerum, etc., published in 1763. In this he shows that in the course of a single year he had observed some ten thousand stars, and computed the places of one thousand

nine hundred and forty-two of them, measured a degree of the meridian, and made many observations of the moon--productive industry seldom equalled in a single year in any field. These observations were of great service to the astronomers, as they afforded the opportunity of comparing the stars of the southern hemisphere with those of the northern, which were being observed simultaneously by Lelande at Berlin.

Lacaille's observations followed closely upon the determination of an absorbing question which occupied the attention of the astronomers in the early part of the century. This question was as to the shape of the earth--whether it was actually flattened at the poles. To settle this question once for all the Academy of Sciences decided to make the actual measurement of the length of two degrees, one as near the pole as possible, the other at the equator. Accordingly, three astronomers, Godin, Bouguer, and La Condamine, made the journey to a spot on the equator in Peru, while four astronomers, Camus, Clairaut, Maupertuis, and Lemonnier, made a voyage to a place selected in Lapland. The result of these expeditions was the determination that the globe is oblately spheroidal.

A great contemporary and fellow-countryman of Lacaille was Jean Le Rond d'Alembert (1717-1783), who, although not primarily an astronomer, did so much with his mathematical calculations to aid that science that his name is closely connected with its progress during the eighteenth century. D'Alembert, who became one of the best-known men of science of his day, and whose services were eagerly sought by the rulers of Europe, began life as a foundling, having been exposed in one of the markets of Paris. The sickly infant was adopted and cared for in the family of a poor glazier, and treated as a member of the family. In later years, however, after the foundling had become famous throughout Europe, his mother, Madame Tencin, sent for him, and acknowledged her relationship. It is more than likely that the great philosopher believed her story, but if so he did not allow her the satisfaction of knowing his belief, declaring always that Madame Tencin could "not be nearer than a step-mother to him, since his mother was the wife of the glazier."

D'Alembert did much for the cause of science by his example as well as by his discoveries. By living a plain but honest life, declining magnificent offers of positions from royal patrons, at the same time refusing to grovel before nobility, he set a worthy example to other philosophers whose cringing and pusillanimous attitude towards persons of wealth or position had hitherto earned them the contempt of the upper classes.

His direct additions to astronomy are several, among others the determination of the mutation of the axis of the earth. He also determined the ratio of the attractive forces of the sun and moon, which he found to be about as seven to three. From this he reached the conclusion that the earth must be seventy times greater than the moon. The first two volumes of his Researches on the Systems of the World, published in 1754, are largely devoted to mathematical and astronomical problems, many of them of little importance now, but of great interest to astronomers at that time.

Another great contemporary of D'Alembert, whose name is closely associated and frequently confounded with his, was Jean Baptiste Joseph Delambre (1749- 1822). More fortunate in birth as also in his educational advantages, Delambre as a youth began his studies under the celebrated poet Delille. Later he was obliged to struggle against poverty, supporting himself for a time by making translations from Latin, Greek, Italian, and English, and acting as tutor in private families. The

turning-point of his fortune came when the attention of Lalande was called to the young man by his remarkable memory, and Lalande soon showed his admiration by giving Delambre certain difficult astronomical problems to solve. By performing these tasks successfully his future as an astronomer became assured. At that time the planet Uranus had just been discovered by Herschel, and the Academy of Sciences offered as the subject for one of its prizes the determination of the planet's orbit. Delambre made this determination and won the prize--a feat that brought him at once into prominence.

By his writings he probably did as much towards perfecting modern astronomy as any one man. His History of Astronomy is not merely a narrative of progress of astronomy but a complete abstract of all the celebrated works written on the subject. Thus he became famous as an historian as well as an astronomer.

LEONARD EULER

Still another contemporary of D'Alembert and Delambre, and somewhat older than either of them, was Leonard Euler (1707-1783), of Basel, whose fame as a philosopher equals that of either of the great Frenchmen. He is of particular interest here in his capacity of astronomer, but astronomy was only one of the many fields of science in which he shone. Surely something out of the ordinary was to be expected of the man who could "repeat the AEneid of Virgil from the beginning to the end without hesitation, and indicate the first and last line of every page of the edition which he used." Something was expected, and he fulfilled these expectations.

In early life he devoted himself to the study of theology and the Oriental languages, at the request of his father, but his love of mathematics proved too strong, and, with his father's consent, he finally gave up his classical studies and turned to his favorite study, geometry. In 1727 he was invited by Catharine I. to reside in St. Petersburg, and on accepting this invitation he was made an associate of the Academy of Sciences. A little later he was made professor of physics, and in 1733 professor of mathematics. In 1735 he solved a problem in three days which some of the eminent mathematicians would not undertake under several months. In 1741 Frederick the Great invited him to Berlin, where he soon became a member of the Academy of Sciences and professor of mathematics; but in 1766 he returned to St. Petersburg. Towards the close of his life be became virtually blind, being obliged to dictate his thoughts, sometimes to persons entirely ignorant of the subject in hand. Nevertheless, his remarkable memory, still further heightened by his blindness, enabled him to carry out the elaborate computations frequently involved.

Euler's first memoir, transmitted to the Academy of Sciences of Paris in 1747, was on the planetary perturbations. This memoir carried off the prize that had been offered for the analytical theory of the motions of Jupiter and Saturn. Other memoirs followed, one in 1749 and another in 1750, with further expansions of the same subject. As some slight errors were found in these, such as a mistake in some of the formulae expressing the secular and periodic inequalities, the academy proposed the same subject for the prize of 1752. Euler again competed, and won this prize also. The contents of this memoir laid the foundation for the subsequent demonstration of the permanent stability of the planetary system by Laplace and Lagrange.

It was Euler also who demonstrated that within certain fixed limits the eccentricities and places of the aphelia of Saturn and Jupiter are subject to constant variation, and he calculated that after a lapse of about thirty thousand years the elements of the orbits of these two planets recover their original values.

II THE PROGRESS OF MODERN ASTRONOMY

A NEW epoch in astronomy begins with the work of William Herschel, the Hanoverian, whom England made hers by adoption. He was a man with a positive genius for sidereal discovery. At first a mere amateur in astronomy, he snatched time from his duties as music-teacher to grind him a telescopic mirror, and began gazing at the stars. Not content with his first telescope, he made another and another, and he had such genius for the work that he soon possessed a better instrument than was ever made before. His patience in grinding the curved reflective surface was monumental. Sometimes for sixteen hours together he must walk steadily about the mirror, polishing it, without once removing his hands. Meantime his sister, always his chief lieutenant, cheered him with her presence, and from time to time put food into his mouth. The telescope completed, the astronomer turned night into day, and from sunset to sunrise, year in and year out, swept the heavens unceasingly, unless prevented by clouds or the brightness of the moon. His sister sat always at his side, recording his observations. They were in the open air, perched high at the mouth of the reflector, and sometimes it was so cold that the ink froze in the bottle in Caroline Herschel's hand; but the two enthusiasts hardly noticed a thing so common-place as terrestrial weather. They were living in distant worlds.

The results? What could they be? Such enthusiasm would move mountains. But, after all, the moving of mountains seems a liliputian task compared with what Herschel really did with those wonderful telescopes. He moved worlds, stars, a universe-- even, if you please, a galaxy of universes; at least he proved that they move, which seems scarcely less wonderful; and he expanded the cosmos, as man conceives it, to thousands of times the dimensions it had before. As a mere beginning, he doubled the diameter of the solar system by observing the great outlying planet which we now call Uranus, but which he christened Georgium Sidus, in honor of his sovereign, and which his French contemporaries, not relishing that name, preferred to call Herschel.

This discovery was but a trifle compared with what Herschel did later on, but it gave him world-wide reputation none the less. Comets and moons aside, this was the first addition to the solar system that had been made within historic times, and it created a veritable furor of popular interest and enthusiasm. Incidentally King George was flattered at having a world named after him, and he smiled on the astronomer, and came with his court to have a look at his namesake. The inspection was highly satisfactory; and presently the royal favor enabled the astronomer to escape the thraldom of teaching music and to devote his entire time to the more congenial task of star-gazing.

Thus relieved from the burden of mundane embarrassments, he turned with fresh enthusiasm to the skies, and his discoveries followed one another in bewildering profusion. He found various hitherto unseen moons of our sister planets; be made special studies of Saturn, and proved that this planet, with its rings, revolves on its axis; he scanned the spots on the sun, and suggested that they

influence the weather of our earth; in short, he extended the entire field of solar astronomy. But very soon this field became too small for him, and his most important researches carried him out into the regions of space compared with which the span of our solar system is a mere point. With his perfected telescopes he entered abysmal vistas which no human eve ever penetrated before, which no human mind had hitherto more than vaguely imagined. He tells us that his forty-foot reflector will bring him light from a distance of "at least eleven and three-fourths millions of millions of millions of miles"--light which left its source two million years ago. The smallest stars visible to the unaided eye are those of the sixth magnitude; this telescope, he thinks, has power to reveal stars of the 1342d magnitude.

But what did Herschel learn regarding these awful depths of space and the stars that people them? That was what the world wished to know. Copernicus, Galileo, Kepler, had given us a solar system, but the stars had been a mystery. What says the great reflector--are the stars points of light, as the ancients taught, and as more than one philosopher of the eighteenth century has still contended, or are they suns, as others hold? Herschel answers, they are suns, each and every one of all the millions--suns, many of them, larger than the one that is the centre of our tiny system. Not only so, but they are moving suns. Instead of being fixed in space, as has been thought, they are whirling in gigantic orbits about some common centre. Is our sun that centre? Far from it. Our sun is only a star like all the rest, circling on with its attendant satellites--our giant sun a star, no different from myriad other stars, not even so large as some; a mere insignificant spark of matter in an infinite shower of sparks.

Nor is this all. Looking beyond the few thousand stars that are visible to the naked eye, Herschel sees series after series of more distant stars, marshalled in galaxies of millions; but at last he reaches a distance beyond which the galaxies no longer increase. And yet--so he thinks--he has not reached the limits of his vision. What then? He has come to the bounds of the sidereal system--seen to the confines of the universe. He believes that he can outline this system, this universe, and prove that it has the shape of an irregular globe, oblately flattened to almost disklike proportions, and divided at one edge--a bifurcation that is revealed even to the naked eye in the forking of the Milky Way.

This, then, is our universe as Herschel conceives it-- a vast galaxy of suns, held to one centre, revolving, poised in space. But even here those marvellous telescopes do not pause. Far, far out beyond the confines of our universe, so far that the awful span of our own system might serve as a unit of measure, are revealed other systems, other universes, like our own, each composed, as he thinks, of myriads of suns, clustered like our galaxy into an isolated system--mere islands of matter in an infinite ocean of space. So distant from our universe are these now universes of Herschel's discovery that their light reaches us only as a dim, nebulous glow, in most cases invisible to the unaided eye. About a hundred of these nebulae were known when Herschel began his studies. Before the close of the century he had discovered about two thousand more of them, and many of these had been resolved by his largest telescopes into clusters of stars. He believed that the farthest of these nebulae that he could see was at least three hundred thousand times as distant from us as the nearest fixed star. Yet that nearest star--so more recent studies prove--is so remote that its light, travelling one hundred and eighty thousand miles a second, requires three and one-half years to reach our planet.

As if to give the finishing touches to this novel scheme of cosmology, Herschel, though in the main very little given to unsustained theorizing, allows himself the privilege of one belief that he cannot call upon his telescope to substantiate. He thinks that all the myriad suns of his numberless systems are instinct with life in the human sense. Giordano Bruno and a long line of his followers had held that some of our sister planets may be inhabited, but Herschel extends the thought to include the moon, the sun, the stars--all the heavenly bodies. He believes that he can demonstrate the habitability of our own sun, and, reasoning from analogy, he is firmly convinced that all the suns of all the systems are "well supplied with inhabitants." In this, as in some other inferences, Herschel is misled by the faulty physics of his time. Future generations, working with perfected instruments, may not sustain him all along the line of his observations, even, let alone his inferences. But how one's egotism shrivels and shrinks as one grasps the import of his sweeping thoughts!

Continuing his observations of the innumerable nebulae, Herschel is led presently to another curious speculative inference. He notes that some star groups are much more thickly clustered than others, and he is led to infer that such varied clustering tells of varying ages of the different nebulae. He thinks that at first all space may have been evenly sprinkled with the stars and that the grouping has resulted from the action of gravitation.

"That the Milky Way is a most extensive stratum of stars of various sizes admits no longer of lasting doubt," he declares, "and that our sun is actually one of the heavenly bodies belonging to it is as evident. I have now viewed and gauged this shining zone in almost every direction and find it composed of stars whose number ... constantly increases and decreases in proportion to its apparent brightness to the naked eye.

"Let us suppose numberless stars of various sizes, scattered over an indefinite portion of space in such a manner as to be almost equally distributed throughout the whole. The laws of attraction which no doubt extend to the remotest regions of the fixed stars will operate in such a manner as most probably to produce the following effects:

"In the first case, since we have supposed the stars to be of various sizes, it will happen that a star, being considerably larger than its neighboring ones, will attract them more than they will be attracted by others that are immediately around them; by which means they will be, in time, as it were, condensed about a centre, or, in other words, form themselves into a cluster of stars of almost a globular figure, more or less regular according to the size and distance of the surrounding stars....

"The next case, which will also happen almost as frequently as the former, is where a few stars, though not superior in size to the rest, may chance to be rather nearer one another than the surrounding ones,... and this construction admits of the utmost variety of shapes. . . .

"From the composition and repeated conjunction of both the foregoing formations, a third may be derived when many large stars, or combined small ones, are spread in long, extended, regular, or crooked rows, streaks, or branches; for they will also draw the surrounding stars, so as to produce figures of condensed stars curiously similar to the former which gave rise to these condensations.

"We may likewise admit still more extensive combinations; when, at the same time that a cluster of stars is forming at the one part of space, there may be another collection in a different but perhaps not far- distant quarter, which may occasion a mutual approach towards their own centre of gravity.

"In the last place, as a natural conclusion of the former cases, there will be formed great cavities or vacancies by the retreating of the stars towards the various centres which attract them."[1]

Looking forward, it appears that the time must come when all the suns of a system will be drawn together and destroyed by impact at a common centre. Already, it seems to Herschel, the thickest clusters have "outlived their usefulness" and are verging towards their doom.

But again, other nebulae present an appearance suggestive of an opposite condition. They are not resolvable into stars, but present an almost uniform appearance throughout, and are hence believed to be composed of a shining fluid, which in some instances is seen to be condensed at the centre into a glowing mass. In such a nebula Herschel thinks he sees a sun in process of formation.

THE NEBULAR HYPOTHESIS OF KANT

Taken together, these two conceptions outline a majestic cycle of world formation and world destruction-- a broad scheme of cosmogony, such as had been vaguely adumbrated two centuries before by Kepler and in more recent times by Wright and Swedenborg. This so-called "nebular hypothesis" assumes that in the beginning all space was uniformly filled with cosmic matter in a state of nebular or "fire-mist" diffusion, "formless and void." It pictures the condensation--coagulation, if you will--of portions of this mass to form segregated masses, and the ultimate development out of these masses of the sidereal bodies that we see.

Perhaps the first elaborate exposition of this idea was that given by the great German philosopher Immanuel Kant (born at Konigsberg in 1724, died in 1804), known to every one as the author of the Critique of Pure Reason. Let us learn from his own words how the imaginative philosopher conceived the world to have come into existence.

"I assume," says Kant, "that all the material of which the globes belonging to our solar system--all the planets and comets--consist, at the beginning of all things was decomposed into its primary elements, and filled the whole space of the universe in which the bodies formed out of it now revolve. This state of nature, when viewed in and by itself without any reference to a system, seems to be the very simplest that can follow upon nothing. At that time nothing has yet been formed. The construction of heavenly bodies at a distance from one another, their distances regulated by their attraction, their form arising out of the equilibrium of their collected matter, exhibit a later state.... In a region of space filled in this manner, a universal repose could last only a moment. The elements have essential forces with which to put each other in motion, and thus are themselves a source of life. Matter immediately begins to strive to fashion itself. The scattered elements of a denser kind, by means of their attraction, gather from a sphere around them all the matter of less specific gravity; again, these elements themselves, together with the material which they have united with them, collect in those points where the particles of a still denser kind are found; these in like manner join still denser particles, and so on. If we follow in imagination this process by which nature fashions itself into form through the whole extent of chaos, we easily perceive that all the

results of the process would consist in the formation of divers masses which, when their formation was complete, would by the equality of their attraction be at rest and be forever unmoved.

"But nature has other forces in store which are specially exerted when matter is decomposed into fine particles. They are those forces by which these particles repel one another, and which, by their conflict with attractions, bring forth that movement which is, as it were, the lasting life of nature. This force of repulsion is manifested in the elasticity of vapors, the effluences of strong-smelling bodies, and the diffusion of all spirituous matters. This force is an uncontestable phenomenon of matter. It is by it that the elements, which may be falling to the point attracting them, are turned sideways promiscuously from their movement in a straight line; and their perpendicular fall thereby issues in circular movements, which encompass the centre towards which they were falling. In order to make the formation of the world more distinctly conceivable, we will limit our view by withdrawing it from the infinite universe of nature and directing it to a particular system, as the one which belongs to our sun. Having considered the generation of this system, we shall be able to advance to a similar consideration of the origin of the great world-systems, and thus to embrace the infinitude of the whole creation in one conception.

"From what has been said, it will appear that if a point is situated in a very large space where the attraction of the elements there situated acts more strongly than elsewhere, then the matter of the elementary particles scattered throughout the whole region will fall to that point. The first effect of this general fall is the formation of a body at this centre of attraction, which, so to speak, grows from an infinitely small nucleus by rapid strides; and in the proportion in which this mass increases, it also draws with greater force the surrounding particles to unite with it. When the mass of this central body has grown so great that the velocity with which it draws the particles to itself with great distances is bent sideways by the feeble degree of repulsion with which they impede one another, and when it issues in lateral movements which are capable by means of the centrifugal force of encompassing the central body in an orbit, then there are produced whirls or vortices of particles, each of which by itself describes a curved line by the composition of the attracting force and the force of revolution that had been bent sideways. These kinds of orbits all intersect one another, for which their great dispersion in this space gives place. Yet these movements are in many ways in conflict with one another, and they naturally tend to bring one another to a uniformity--that is, into a state in which one movement is as little obstructive to the other as possible. This happens in two ways: first by the particles limiting one another's movement till they all advance in one direction; and, secondly, in this way, that the particles limit their vertical movements in virtue of which they are approaching the centre of attraction, till they all move horizontally--i. e., in parallel circles round the sun as their centre, no longer intercept one another, and by the centrifugal force becoming equal with the falling force they keep themselves constantly in free circular orbits at the distance at which they move. The result, finally, is that only those particles continue to move in this region of space which have acquired by their fall a velocity, and through the resistance of the other particles a direction, by which they can continue to maintain a FREE CIRCULAR MOVEMENT....

"The view of the formation of the planets in this system has the advantage over every other possible theory in holding that the origin of the movements, and the position of the orbits in arising at that same point of time--nay, more, in showing that even the deviations from the greatest possible exactness in their determinations, as well as the accordances themselves, become clear at a glance.

The planets are formed out of particles which, at the distance at which they move, have exact movements in circular orbits; and therefore the masses composed out of them will continue the same movements and at the same rate and in the same direction."[2]

It must be admitted that this explanation leaves a good deal to be desired. It is the explanation of a metaphysician rather than that of an experimental scientist. Such phrases as "matter immediately begins to strive to fashion itself," for example, have no place in the reasoning of inductive science. Nevertheless, the hypothesis of Kant is a remarkable conception; it attempts to explain along rational lines something which hitherto had for the most part been considered altogether inexplicable.

But there are various questions that at once suggest themselves which the Kantian theory leaves unanswered. How happens it, for example, that the cosmic mass which gave birth to our solar system was divided into several planetary bodies instead of remaining a single mass? Were the planets struck from the sun by the chance impact of comets, as Buffon has suggested? or thrown out by explosive volcanic action, in accordance with the theory of Dr. Darwin? or do they owe their origin to some unknown law? In any event, how chanced it that all were projected in nearly the same plane as we now find them?

LAPLACE AND THE NEBULAR HYPOTHESIS

It remained for a mathematical astronomer to solve these puzzles. The man of all others competent to take the subject in hand was the French astronomer Laplace. For a quarter of a century he had devoted his transcendent mathematical abilities to the solution of problems of motion of the heavenly bodies. Working in friendly rivalry with his countryman Lagrange, his only peer among the mathematicians of the age, he had taken up and solved one by one the problems that Newton left obscure. Largely through the efforts of these two men the last lingering doubts as to the solidarity of the Newtonian hypothesis of universal gravitation had been removed. The share of Lagrange was hardly less than that of his co-worker; but Laplace will longer be remembered, because he ultimately brought his completed labors into a system, and, incorporating with them the labors of his contemporaries, produced in the Mecanique Celeste the undisputed mathematical monument of the century, a fitting complement to the Principia of Newton, which it supplements and in a sense completes.

In the closing years of the eighteenth century Laplace took up the nebular hypothesis of cosmogony, to which we have just referred, and gave it definite proportions; in fact, made it so thoroughly his own that posterity will always link it with his name. Discarding the crude notions of cometary impact and volcanic eruption, Laplace filled up the gaps in the hypothesis with the aid of well-known laws of gravitation and motion. He assumed that the primitive mass of cosmic matter which was destined to form our solar system was revolving on its axis even at a time when it was still nebular in character, and filled all space to a distance far beyond the present limits of the system. As this vaporous mass contracted through loss of heat, it revolved more and more swiftly, and from time to time, through balance of forces at its periphery, rings of its substance were whirled off and left revolving there, subsequently to become condensed into planets, and in their turn whirl off minor rings that became moons. The main body of the original mass remains in the present as the still contracting and rotating body which we call the sun.

Let us allow Laplace to explain all this in detail:

"In order to explain the prime movements of the planetary system," he says, "there are the five following phenomena: The movement of the planets in the same direction and very nearly in the same plane; the movement of the satellites in the same direction as that of the planets; the rotation of these different bodies and the sun in the same direction as their revolution, and in nearly the same plane; the slight eccentricity of the orbits of the planets and of the satellites; and, finally, the great eccentricity of the orbits of the comets, as if their inclinations had been left to chance.

"Buffon is the only man I know who, since the discovery of the true system of the world, has endeavored to show the origin of the planets and their satellites. He supposes that a comet, in falling into the sun, drove from it a mass of matter which was reassembled at a distance in the form of various globes more or less large, and more or less removed from the sun, and that these globes, becoming opaque and solid, are now the planets and their satellites.

"This hypothesis satisfies the first of the five preceding phenomena; for it is clear that all the bodies thus formed would move very nearly in the plane which passed through the centre of the sun, and in the direction of the torrent of matter which was produced; but the four other phenomena appear to be inexplicable to me by this means. Indeed, the absolute movement of the molecules of a planet ought then to be in the direction of the movement of its centre of gravity; but it does not at all follow that the motion of the rotation of the planets should be in the same direction. Thus the earth should rotate from east to west, but nevertheless the absolute movement of its molecules should be from east to west; and this ought also to apply to the movement of the revolution of the satellites, in which the direction, according to the hypothesis which he offers, is not necessarily the same as that of the progressive movement of the planets.

"A phenomenon not only very difficult to explain under this hypothesis, but one which is even contrary to it, is the slight eccentricity of the planetary orbits. We know, by the theory of central forces, that if a body moves in a closed orbit around the sun and touches it, it also always comes back to that point at every revolution; whence it follows that if the planets were originally detached from the sun, they would touch it at each return towards it, and their orbits, far from being circular, would be very eccentric. It is true that a mass of matter driven from the sun cannot be exactly compared to a globe which touches its surface, for the impulse which the particles of this mass receive from one another and the reciprocal attractions which they exert among themselves, could, in changing the direction of their movements, remove their perihelions from the sun; but their orbits would be always most eccentric, or at least they would not have slight eccentricities except by the most extraordinary chance. Thus we cannot see, according to the hypothesis of Buffon, why the orbits of more than a hundred comets already observed are so elliptical. This hypothesis is therefore very far from satisfying the preceding phenomena. Let us see if it is possible to trace them back to their true cause.

"Whatever may be its ultimate nature, seeing that it has caused or modified the movements of the planets, it is necessary that this cause should embrace every body, and, in view of the enormous distances which separate them, it could only have been a fluid of immense extent. In order to have given them an almost circular movement in the same direction around the sun, it is necessary that

this fluid should have enveloped the sun as in an atmosphere. The consideration of the planetary movements leads us then to think that, on account of excessive heat, the atmosphere of the sun originally extended beyond the orbits of all the planets, and that it was successively contracted to its present limits.

"In the primitive condition in which we suppose the sun to have been, it resembled a nebula such as the telescope shows is composed of a nucleus more or less brilliant, surrounded by a nebulosity which, on condensing itself towards the centre, forms a star. If it is conceived by analogy that all the stars were formed in this manner, it is possible to imagine their previous condition of nebulosity, itself preceded by other states in which the nebulous matter was still more diffused, the nucleus being less and less luminous. By going back as far as possible, we thus arrive at a nebulosity so diffused that its existence could hardly be suspected.

"For a long time the peculiar disposition of certain stars, visible to the unaided eye, has struck philosophical observers. Mitchell has already remarked how little probable it is that the stars in the Pleiades, for example, could have been contracted into the small space which encloses them by the fortuity of chance alone, and he has concluded that this group of stars, and similar groups which the skies present to us, are the necessary result of the condensation of a nebula, with several nuclei, and it is evident that a nebula, by continually contracting, towards these various nuclei, at length would form a group of stars similar to the Pleiades. The condensation of a nebula with two nuclei would form a system of stars close together, turning one upon the other, such as those double stars of which we already know the respective movements.

"But how did the solar atmosphere determine the movements of the rotation and revolution of the planets and satellites? If these bodies had penetrated very deeply into this atmosphere, its resistance would have caused them to fall into the sun. We can therefore conjecture that the planets were formed at their successive limits by the condensation of a zone of vapors which the sun, on cooling, left behind, in the plane of his equator.

"Let us recall the results which we have given in a preceding chapter. The atmosphere of the sun could not have extended indefinitely. Its limit was the point where the centrifugal force due to its movement of rotation balanced its weight. But in proportion as the cooling contracted the atmosphere, and those molecules which were near to them condensed upon the surface of the body, the movement of the rotation increased; for, on account of the Law of Areas, the sum of the areas described by the vector of each molecule of the sun and its atmosphere and projected in the plane of the equator being always the same, the rotation should increase when these molecules approach the centre of the sun. The centrifugal force due to this movement becoming thus larger, the point where the weight is equal to it is nearer the sun. Supposing, then, as it is natural to admit, that the atmosphere extended at some period to its very limits, it should, on cooling, leave molecules behind at this limit and at limits successively occasioned by the increased rotation of the sun. The abandoned molecules would continue to revolve around this body, since their centrifugal force was balanced by their weight. But this equilibrium not arising in regard to the atmospheric molecules parallel to the solar equator, the latter, on account of their weight, approached the atmosphere as they condensed, and did not cease to belong to it until by this motion they came upon the equator.

"Let us consider now the zones of vapor successively left behind. These zones ought, according to appearance, by the condensation and mutual attraction of their molecules, to form various concentric rings of vapor revolving around the sun. The mutual gravitational friction of each ring would accelerate some and retard others, until they had all acquired the same angular velocity. Thus the actual velocity of the molecules most removed from the sun would be the greatest. The following cause would also operate to bring about this difference of speed. The molecules farthest from the sun, and which by the effects of cooling and condensation approached one another to form the outer part of the ring, would have always described areas proportional to the time since the central force by which they were controlled has been constantly directed towards this body. But this constancy of areas necessitates an increase of velocity proportional to the distance. It is thus seen that the same cause would diminish the velocity of the molecules which form the inner part of the ring.

"If all the molecules of the ring of vapor continued to condense without disuniting, they would at length form a ring either solid or fluid. But this formation would necessitate such a regularity in every part of the ring, and in its cooling, that this phenomenon is extremely rare; and the solar system affords us, indeed, but one example--namely, in the ring of Saturn. In nearly every case the ring of vapor was broken into several masses, each moving at similar velocities, and continuing to rotate at the same distance around the sun. These masses would take a spheroid form with a rotatory movement in the direction of the revolution, because their inner molecules had less velocity than the outer. Thus were formed so many planets in a condition of vapor. But if one of them were powerful enough to reunite successively by its attraction all the others around its centre of gravity, the ring of vapor would be thus transformed into a single spheroidical mass of vapor revolving around the sun with a rotation in the direction of its revolution. The latter case has been that which is the most common, but nevertheless the solar system affords us an instance of the first case in the four small planets which move between Jupiter and Mars; at least, if we do not suppose, as does M. Olbers, that they originally formed a single planet which a mighty explosion broke up into several portions each moving at different velocities.

"According to our hypothesis, the comets are strangers to our planetary system. In considering them, as we have done, as minute nebulosities, wandering from solar system to solar system, and formed by the condensation of the nebulous matter everywhere existent in profusion in the universe, we see that when they come into that part of the heavens where the sun is all-powerful, he forces them to describe orbits either elliptical or hyperbolic, their paths being equally possible in all directions, and at all inclinations of the ecliptic, conformably to what has been observed. Thus the condensation of nebulous matter, by which we have at first explained the motions of the rotation and revolution of the planets and their satellites in the same direction, and in nearly approximate planes, explains also why the movements of the comets escape this general law."[3]

The nebular hypothesis thus given detailed completion by Laplace is a worthy complement of the grand cosmologic scheme of Herschel. Whether true or false, the two conceptions stand as the final contributions of the eighteenth century to the history of man's ceaseless efforts to solve the mysteries of cosmic origin and cosmic structure. The world listened eagerly and without prejudice to the new doctrines; and that attitude tells of a marvellous intellectual growth of our race. Mark the transition. In the year 1600, Bruno was burned at the stake for teaching that our earth is not the centre of the universe. In 1700, Newton was pronounced "impious and heretical" by a large school

of philosophers for declaring that the force which holds the planets in their orbits is universal gravitation. In 1800, Laplace and Herschel are honored for teaching that gravitation built up the system which it still controls; that our universe is but a minor nebula, our sun but a minor star, our earth a mere atom of matter, our race only one of myriad races peopling an infinity of worlds. Doctrines which but the span of two human lives before would have brought their enunciators to the stake were now pronounced not impious, but sublime.

ASTEROIDS AND SATELLITES

The first day of the nineteenth century was fittingly signalized by the discovery of a new world. On the evening of January 1, 1801, an Italian astronomer, Piazzi, observed an apparent star of about the eighth magnitude (hence, of course, quite invisible to the unaided eye), which later on was seen to have moved, and was thus shown to be vastly nearer the earth than any true star. He at first supposed, as Herschel had done when he first saw Uranus, that the unfamiliar body was a comet; but later observation proved it a tiny planet, occupying a position in space between Mars and Jupiter. It was christened Ceres, after the tutelary goddess of Sicily.

Though unpremeditated, this discovery was not unexpected, for astronomers had long surmised the existence of a planet in the wide gap between Mars and Jupiter. Indeed, they were even preparing to make concerted search for it, despite the protests of philosophers, who argued that the planets could not possibly exceed the magic number seven, when Piazzi forestalled their efforts. But a surprise came with the sequel; for the very next year Dr. Olbers, the wonderful physician-astronomer of Bremen, while following up the course of Ceres, happened on another tiny moving star, similarly located, which soon revealed itself as planetary. Thus two planets were found where only one was expected.

The existence of the supernumerary was a puzzle, but Olbers solved it for the moment by suggesting that Ceres and Pallas, as he called his captive, might be fragments of a quondam planet, shattered by internal explosion or by the impact of a comet. Other similar fragments, he ventured to predict, would be found when searched for. William Herschel sanctioned this theory, and suggested the name asteroids for the tiny planets. The explosion theory was supported by the discovery of another asteroid, by Harding, of Lilienthal, in 1804, and it seemed clinched when Olbers himself found a fourth in 1807. The new-comers were named Juno and Vesta respectively.

There the case rested till 1845, when a Prussian amateur astronomer named Hencke found another asteroid, after long searching, and opened a new epoch of discovery. From then on the finding of asteroids became a commonplace. Latterly, with the aid of photography, the list has been extended to above four hundred, and as yet there seems no dearth in the supply, though doubtless all the larger members have been revealed. Even these are but a few hundreds of miles in diameter, while the smaller ones are too tiny for measurement. The combined bulk of these minor planets is believed to be but a fraction of that of the earth.

Olbers's explosion theory, long accepted by astronomers, has been proven open to fatal objections. The minor planets are now believed to represent a ring of cosmical matter, cast off from the solar nebula like the rings that went to form the major planets, but prevented from becoming aggregated into a single body by the perturbing mass of Jupiter.

The Discovery of Neptune

As we have seen, the discovery of the first asteroid confirmed a conjecture; the other important planetary discovery of the nineteenth century fulfilled a prediction. Neptune was found through scientific prophecy. No one suspected the existence of a trans-Uranian planet till Uranus itself, by hair-breadth departures from its predicted orbit, gave out the secret. No one saw the disturbing planet till the pencil of the mathematician, with almost occult divination, had pointed out its place in the heavens. The general predication of a trans-Uranian planet was made by Bessel, the great Konigsberg astronomer, in 1840; the analysis that revealed its exact location was undertaken, half a decade later, by two independent workers--John Couch Adams, just graduated senior wrangler at Cambridge, England, and U. J. J. Leverrier, the leading French mathematician of his generation.

Adams's calculation was first begun and first completed. But it had one radical defect--it was the work of a young and untried man. So it found lodgment in a pigeon-hole of the desk of England's Astronomer Royal, and an opportunity was lost which English astronomers have never ceased to mourn. Had the search been made, an actual planet would have been seen shining there, close to the spot where the pencil of the mathematician had placed its hypothetical counterpart. But the search was not made, and while the prophecy of Adams gathered dust in that regrettable pigeon-hole, Leverrier's calculation was coming on, his tentative results meeting full encouragement from Arago and other French savants. At last the laborious calculations proved satisfactory, and, confident of the result, Leverrier sent to the Berlin observatory, requesting that search be made for the disturber of Uranus in a particular spot of the heavens. Dr. Galle received the request September 23, 1846. That very night he turned his telescope to the indicated region, and there, within a single degree of the suggested spot, he saw a seeming star, invisible to the unaided eye, which proved to be the long-sought planet, henceforth to be known as Neptune. To the average mind, which finds something altogether mystifying about abstract mathematics, this was a feat savoring of the miraculous.

Stimulated by this success, Leverrier calculated an orbit for an interior planet from perturbations of Mercury, but though prematurely christened Vulcan, this hypothetical nursling of the sun still haunts the realm of the undiscovered, along with certain equally hypothetical trans-Neptunian planets whose existence has been suggested by "residual perturbations" of Uranus, and by the movements of comets. No other veritable additions of the sun's planetary family have been made in our century, beyond the finding of seven small moons, which chiefly attest the advance in telescopic powers. Of these, the tiny attendants of our Martian neighbor, discovered by Professor Hall with the great Washington refractor, are of greatest interest, because of their small size and extremely rapid flight. One of them is poised only six thousand miles from Mars, and whirls about him almost four times as fast as he revolves, seeming thus, as viewed by the Martian, to rise in the west and set in the east, and making the month only one-fourth as long as the day.

The Rings of Saturn

The discovery of the inner or crape ring of Saturn, made simultaneously in 1850 by William C. Bond, at the Harvard observatory, in America, and the Rev. W. R. Dawes in England, was another interesting optical achievement; but our most important advances in knowledge of Saturn's unique

system are due to the mathematician. Laplace, like his predecessors, supposed these rings to be solid, and explained their stability as due to certain irregularities of contour which Herschel bad pointed out. But about 1851 Professor Peirce, of Harvard, showed the untenability of this conclusion, proving that were the rings such as Laplace thought them they must fall of their own weight. Then Professor J. Clerk-Maxwell, of Cambridge, took the matter in hand, and his analysis reduced the puzzling rings to a cloud of meteoric particles--a "shower of brickbats"--each fragment of which circulates exactly as if it were an independent planet, though of course perturbed and jostled more or less by its fellows. Mutual perturbations, and the disturbing pulls of Saturn's orthodox satellites, as investigated by Maxwell, explain nearly all the phenomena of the rings in a manner highly satisfactory.

After elaborate mathematical calculations covering many pages of his paper entitled "On the Stability of Saturn's Rings," he summarizes his deductions as follows:

"Let us now gather together the conclusions we have been able to draw from the mathematical theory of various kinds of conceivable rings.

"We found that the stability of the motion of a solid ring depended on so delicate an adjustment, and at the same time so unsymmetrical a distribution of mass, that even if the exact conditions were fulfilled, it could scarcely last long, and, if it did, the immense preponderance of one side of the ring would be easily observed, contrary to experience. These considerations, with others derived from the mechanical structure of so vast a body, compel us to abandon any theory of solid rings.

"We next examined the motion of a ring of equal satellites, and found that if the mass of the planet is sufficient, any disturbances produced in the arrangement of the ring will be propagated around it in the form of waves, and will not introduce dangerous confusion. If the satellites are unequal, the propagations of the waves will no longer be regular, but disturbances of the ring will in this, as in the former case, produce only waves, and not growing confusion. Supposing the ring to consist, not of a single row of large satellites, but a cloud of evenly distributed unconnected particles, we found that such a cloud must have a very small density in order to be permanent, and that this is inconsistent with its outer and inner parts moving with the same angular velocity. Supposing the ring to be fluid and continuous, we found that it will be necessarily broken up into small portions.

"We conclude, therefore, that the rings must consist of disconnected particles; these must be either solid or liquid, but they must be independent. The entire system of rings must, therefore, consist either of a series of many concentric rings each moving with its own velocity and having its own system of waves, or else of a confused multitude of revolving particles not arranged in rings and continually coming into collision with one another.

"Taking the first case, we found that in an indefinite number of possible cases the mutual perturbations of two rings, stable in themselves, might mount up in time to a destructive magnitude, and that such cases must continually occur in an extensive system like that of Saturn, the only retarding cause being the irregularity of the rings.

"The result of long-continued disturbance was found to be the spreading-out of the rings in breadth, the outer rings pressing outward, while the inner rings press inward.

"The final result, therefore, of the mechanical theory is that the only system of rings which can exist is one composed of an indefinite number of unconnected particles, revolving around the planet with different velocities, according to their respective distances. These particles may be arranged in series of narrow rings, or they may move through one another irregularly. In the first case the destruction of the system will be very slow, in the second case it will be more rapid, but there may be a tendency towards arrangement in narrow rings which may retard the process.

"We are not able to ascertain by observation the constitution of the two outer divisions of the system of rings, but the inner ring is certainly transparent, for the limb of Saturn has been observed through it. It is also certain that though the space occupied by the ring is transparent, it is not through the material parts of it that the limb of Saturn is seen, for his limb was observed without distortion; which shows that there was no refraction, and, therefore, that the rays did not pass through a medium at all, but between the solar or liquid particles of which the ring is composed. Here, then, we have an optical argument in favor of the theory of independent particles as the material of the rings. The two outer rings may be of the same nature, but not so exceedingly rare that a ray of light can pass through their whole thickness without encountering one of the particles.

"Finally, the two outer rings have been observed for two hundred years, and it appears, from the careful analysis of all the observations of M. Struve, that the second ring is broader than when first observed, and that its inner edge is nearer the planet than formerly. The inner ring also is suspected to be approaching the planet ever since its discovery in 1850. These appearances seem to indicate the same slow progress of the rings towards separation which we found to be the result of theory, and the remark that the inner edge of the inner ring is more distinct seems to indicate that the approach towards the planet is less rapid near the edge, as we had reason to conjecture. As to the apparent unchangeableness of the exterior diameter of the outer ring, we must remember that the outer rings are certainly far more dense than the inner one, and that a small change in the outer rings must balance a great change in the inner one. It is possible, however, that some of the observed changes may be due to the existence of a resisting medium. If the changes already suspected should be confirmed by repeated observations with the same instruments, it will be worth while to investigate more carefully whether Saturn's rings are permanent or transitory elements of the solar system, and whether in that part of the heavens we see celestial immutability or terrestrial corruption and generation, and the old order giving place to the new before our eyes."[4]

Studies of the Moon

But perhaps the most interesting accomplishments of mathematical astronomy--from a mundane standpoint, at any rate--are those that refer to the earth's own satellite. That seemingly staid body was long ago discovered to have a propensity to gain a little on the earth, appearing at eclipses an infinitesimal moment ahead of time. Astronomers were sorely puzzled by this act of insubordination; but at last Laplace and Lagrange explained it as due to an oscillatory change in the earth's orbit, thus fully exonerating the moon, and seeming to demonstrate the absolute stability of our planetary system, which the moon's misbehavior had appeared to threaten.

This highly satisfactory conclusion was an orthodox belief of celestial mechanics until 1853, when Professor Adams of Neptunian fame, with whom complex analyses were a pastime, reviewed

Laplace's calculation, and discovered an error which, when corrected, left about half the moon's acceleration unaccounted for. This was a momentous discrepancy, which at first no one could explain. But presently Professor Helmholtz, the great German physicist, suggested that a key might be found in tidal friction, which, acting as a perpetual brake on the earth's rotation, and affecting not merely the waters but the entire substance of our planet, must in the long sweep of time have changed its rate of rotation. Thus the seeming acceleration of the moon might be accounted for as actual retardation of the earth's rotation--a lengthening of the day instead of a shortening of the month.

Again the earth was shown to be at fault, but this time the moon could not be exonerated, while the estimated stability of our system, instead of being re-established, was quite upset. For the tidal retardation is not an oscillatory change which will presently correct itself, like the orbital wobble, but a perpetual change, acting always in one direction. Unless fully counteracted by some opposing reaction, therefore (as it seems not to be), the effect must be cumulative, the ultimate consequences disastrous. The exact character of these consequences was first estimated by Professor G. H. Darwin in 1879. He showed that tidal friction, in retarding the earth, must also push the moon out from the parent planet on a spiral orbit. Plainly, then, the moon must formerly have been nearer the earth than at present. At some very remote period it must have actually touched the earth; must, in other words, have been thrown off from the then plastic mass of the earth, as a polyp buds out from its parent polyp. At that time the earth was spinning about in a day of from two to four hours.

Now the day has been lengthened to twenty-four hours, and the moon has been thrust out to a distance of a quarter-million miles; but the end is not yet. The same progress of events must continue, till, at some remote period in the future, the day has come to equal the month, lunar tidal action has ceased, and one face of the earth looks out always at the moon with that same fixed stare which even now the moon has been brought to assume towards her parent orb. Should we choose to take even greater liberties with the future, it may be made to appear (though some astronomers dissent from this prediction) that, as solar tidal action still continues, the day must finally exceed the month, and lengthen out little by little towards coincidence with the year; and that the moon meantime must pause in its outward flight, and come swinging back on a descending spiral, until finally, after the lapse of untold aeons, it ploughs and ricochets along the surface of the earth, and plunges to catastrophic destruction.

But even though imagination pause far short of this direful culmination, it still is clear that modern calculations, based on inexorable tidal friction, suffice to revolutionize the views formerly current as to the stability of the planetary system. The eighteenth-century mathematician looked upon this system as a vast celestial machine which had been in existence about six thousand years, and which was destined to run on forever. The analyst of to-day computes both the past and the future of this system in millions instead of thousands of years, yet feels well assured that the solar system offers no contradiction to those laws of growth and decay which seem everywhere to represent the immutable order of nature.

COMETS AND METEORS

Until the mathematician ferreted out the secret, it surely never could have been suspected by any one that the earth's serene attendant,

"That orbed maiden, with white fire laden, Whom mortals call the moon,"

could be plotting injury to her parent orb. But there is another inhabitant of the skies whose purposes have not been similarly free from popular suspicion. Needless to say I refer to the black sheep of the sidereal family, that "celestial vagabond" the comet.

Time out of mind these wanderers have been supposed to presage war, famine, pestilence, perhaps the destruction of the world. And little wonder. Here is a body which comes flashing out of boundless space into our system, shooting out a pyrotechnic tail some hundreds of millions of miles in length; whirling, perhaps, through the very atmosphere of the sun at a speed of three or four hundred miles a second; then darting off on a hyperbolic orbit that forbids it ever to return, or an elliptical one that cannot be closed for hundreds or thousands of years; the tail meantime pointing always away from the sun, and fading to nothingness as the weird voyager recedes into the spatial void whence it came. Not many times need the advent of such an apparition coincide with the outbreak of a pestilence or the death of a Caesar to stamp the race of comets as an ominous clan in the minds of all superstitious generations.

It is true, a hard blow was struck at the prestige of these alleged supernatural agents when Newton proved that the great comet of 1680 obeyed Kepler's laws in its flight about the sun; and an even harder one when the same visitant came back in 1758, obedient to Halley's prediction, after its three-quarters of a century of voyaging but in the abyss of space. Proved thus to bow to natural law, the celestial messenger could no longer fully, sustain its role. But long-standing notoriety cannot be lived down in a day, and the comet, though proved a "natural" object, was still regarded as a very menacing one for another hundred years or so. It remained for the nineteenth century to completely unmask the pretender and show how egregiously our forebears had been deceived.

The unmasking began early in the century, when Dr. Olbers, then the highest authority on the subject, expressed the opinion that the spectacular tail, which had all along been the comet's chief stock-in-trade as an earth-threatener, is in reality composed of the most filmy vapors, repelled from the cometary body by the sun, presumably through electrical action, with a velocity comparable to that of light. This luminous suggestion was held more or less in abeyance for half a century. Then it was elaborated by Zollner, and particularly by Bredichin, of the Moscow observatory, into what has since been regarded as the most plausible of cometary theories. It is held that comets and the sun are similarly electrified, and hence mutually repulsive. Gravitation vastly outmatches this repulsion in the body of the comet, but yields to it in the case of gases, because electrical force varies with the surface, while gravitation varies only with the mass. From study of atomic weights and estimates of the velocity of thrust of cometary tails, Bredichin concluded that the chief components of the various kinds of tails are hydrogen, hydrocarbons, and the vapor of iron; and spectroscopic analysis goes far towards sustaining these assumptions.

But, theories aside, the unsubstantialness of the comet's tail has been put to a conclusive test. Twice during the nineteenth century the earth has actually plunged directly through one of these threatening appendages--in 1819, and again in 1861, once being immersed to a depth of some three hundred thousand miles in its substance. Yet nothing dreadful happened to us. There was a peculiar

glow in the atmosphere, so the more imaginative observers thought, and that was all. After such fiascos the cometary train could never again pose as a world-destroyer.

But the full measure of the comet's humiliation is not yet told. The pyrotechnic tail, composed as it is of portions of the comet's actual substance, is tribute paid the sun, and can never be recovered. Should the obeisance to the sun be many times repeated, the train-forming material will be exhausted, and the comet's chiefest glory will have departed. Such a fate has actually befallen a multitude of comets which Jupiter and the other outlying planets have dragged into our system and helped the sun to hold captive here. Many of these tailless comets were known to the eighteenth-century astronomers, but no one at that time suspected the true meaning of their condition. It was not even known how closely some of them are enchained until the German astronomer Encke, in 1822, showed that one which he had rediscovered, and which has since borne his name, was moving in an orbit so contracted that it must complete its circuit in about three and a half years. Shortly afterwards another comet, revolving in a period of about six years, was discovered by Biela, and given his name. Only two more of these short-period comets were discovered during the first half of last century, but latterly they have been shown to be a numerous family. Nearly twenty are known which the giant Jupiter holds so close that the utmost reach of their elliptical tether does not let them go beyond the orbit of Saturn. These aforetime wanderers have adapted themselves wonderfully to planetary customs, for all of them revolve in the same direction with the planets, and in planes not wide of the ecliptic.

Checked in their proud hyperbolic sweep, made captive in a planetary net, deprived of their trains, these quondam free-lances of the heavens are now mere shadows of their former selves. Considered as to mere bulk, they are very substantial shadows, their extent being measured in hundreds of thousands of miles; but their actual mass is so slight that they are quite at the mercy of the gravitation pulls of their captors. And worse is in store for them. So persistently do sun and planets tug at them that they are doomed presently to be torn into shreds.

Such a fate has already overtaken one of them, under the very eyes of the astronomers, within the relatively short period during which these ill-fated comets have. been observed. In 1832 Biela's comet passed quite near the earth, as astronomers measure distance, and in doing so created a panic on our planet. It did no greater harm than that, of course, and passed on its way as usual. The very next time it came within telescopic hail it was seen to have broken into two fragments. Six years later these fragments were separated by many millions of miles; and in 1852, when the comet was due again, astronomers looked for it in vain. It had been completely shattered.

What had become of the fragments? At that time no one positively knew. But the question was to be answered presently. It chanced that just at this period astronomers were paying much attention to a class of bodies which they had hitherto somewhat neglected, the familiar shooting-stars, or meteors. The studies of Professor Newton, of Yale, and Professor Adams, of Cambridge, with particular reference to the great meteor-shower of November, 1866, which Professor Newton had predicted and shown to be recurrent at intervals of thirty-three years, showed that meteors are not mere sporadic swarms of matter flying at random, but exist in isolated swarms, and sweep about the sun in regular elliptical orbits.

Presently it was shown by the Italian astronomer Schiaparelli that one of these meteor swarms moves in the orbit of a previously observed comet, and other coincidences of the kind were soon forthcoming. The conviction grew that meteor swarms are really the debris of comets; and this conviction became a practical certainty when, in November, 1872, the earth crossed the orbit of the ill-starred Biela, and a shower of meteors came whizzing into our atmosphere in lieu of the lost comet.

And so at last the full secret was out. The awe- inspiring comet, instead of being the planetary body it had all along been regarded, is really nothing more nor less than a great aggregation of meteoric particles, which have become clustered together out in space somewhere, and which by jostling one another or through electrical action become luminous. So widely are the individual particles separated that the cometary body as a whole has been estimated to be thousands of times less dense than the earth's atmosphere at sea- level. Hence the ease with which the comet may be dismembered and its particles strung out into streaming swarms.

So thickly is the space we traverse strewn with this cometary dust that the earth sweeps up, according to Professor Newcomb's estimate, a million tons of it each day. Each individual particle, perhaps no larger than a millet seed, becomes a shooting-star, or meteor, as it burns to vapor in the earth's upper atmosphere. And if one tiny planet sweeps up such masses of this cosmic matter, the amount of it in the entire stretch of our system must be beyond all estimate. What a story it tells of the myriads of cometary victims that have fallen prey to the sun since first he stretched his planetary net across the heavens!

THE FIXED STARS

When Biela's comet gave the inhabitants of the earth such a fright in 1832, it really did not come within fifty millions of miles of us. Even the great comet through whose filmy tail the earth passed in 1861 was itself fourteen millions of miles away. The ordinary mind, schooled to measure space by the tiny stretches of a pygmy planet, cannot grasp the import of such distances; yet these are mere units of measure compared with the vast stretches of sidereal space. Were the comet which hurtles past us at a speed of, say, a hundred miles a second to continue its mad flight unchecked straight into the void of space, it must fly on its frigid way eight thousand years before it could reach the very nearest of our neighbor stars; and even then it would have penetrated but a mere arm's-length into the vistas where lie the dozen or so of sidereal residents that are next beyond. Even to the trained mind such distances are only vaguely imaginable. Yet the astronomer of our century has reached out across this unthinkable void and brought back many a secret which our predecessors thought forever beyond human grasp.

A tentative assault upon this stronghold of the stars was being made by Herschel at the beginning of the century. In 1802 that greatest of observing astronomers announced to the Royal Society his discovery that certain double stars had changed their relative positions towards one another since he first carefully charted them twenty years before. Hitherto it had been supposed that double stars were mere optical effects. Now it became clear that some of them, at any rate, are true "binary systems," linked together presumably by gravitation and revolving about one another. Halley had shown, three-quarters of a century before, that the stars have an actual or "proper" motion in space; Herschel himself had proved that the sun shares this motion with the other stars. Here was another

shift of place, hitherto quite unsuspected, to be reckoned with by the astronomer in fathoming sidereal secrets.

Double Stars

When John Herschel, the only son and the worthy successor of the great astronomer, began star-gazing in earnest, after graduating senior wrangler at Cambridge, and making two or three tentative professional starts in other directions to which his versatile genius impelled him, his first extended work was the observation of his father's double stars. His studies, in which at first he had the collaboration of Mr. James South, brought to light scores of hitherto unrecognized pairs, and gave fresh data for the calculation of the orbits of those longer known. So also did the independent researches of F. G. W. Struve, the enthusiastic observer of the famous Russian observatory at the university of Dorpat, and subsequently at Pulkowa. Utilizing data gathered by these observers, M. Savary, of Paris, showed, in 1827, that the observed elliptical orbits of the double stars are explicable by the ordinary laws of gravitation, thus confirming the assumption that Newton's laws apply to these sidereal bodies. Henceforth there could be no reason to doubt that the same force which holds terrestrial objects on our globe pulls at each and every particle of matter throughout the visible universe.

The pioneer explorers of the double stars early found that the systems into which the stars are linked are by no means confined to single pairs. Often three or four stars are found thus closely connected into gravitation systems; indeed, there are all gradations between binary systems and great clusters containing hundreds or even thousands of members. It is known, for example, that the familiar cluster of the Pleiades is not merely an optical grouping, as was formerly supposed, but an actual federation of associated stars, some two thousand five hundred in number, only a few of which are visible to the unaided eve. And the more carefully the motions of the stars are studied, the more evident it becomes that widely separated stars are linked together into infinitely complex systems, as yet but little understood. At the same time, all instrumental advances tend to resolve more and more seemingly single stars into close pairs and minor clusters. The two Herschels between them discovered some thousands of these close multiple systems; Struve and others increased the list to above ten thousand; and Mr. S. W. Burnham, of late years the most enthusiastic and successful of double-star pursuers, added a thousand new discoveries while he was still an amateur in astronomy, and by profession the stenographer of a Chicago court. Clearly the actual number of multiple stars is beyond all present estimate.

The elder Herschel's early studies of double stars were undertaken in the hope that these objects might aid him in ascertaining the actual distance of a star, through measurement of its annual parallax--that is to say, of the angle which the diameter of the earth's orbit would subtend as seen from the star. The expectation was not fulfilled. The apparent shift of position of a star as viewed from opposite sides of the earth's orbit, from which the parallax might be estimated, is so extremely minute that it proved utterly inappreciable, even to the almost preternaturally acute vision of Herschel, with the aid of any instrumental means then at command. So the problem of star distance allured and eluded him to the end, and he died in 1822 without seeing it even in prospect of solution. His estimate of the minimum distance of the nearest star, based though it was on the fallacious test of apparent brilliancy, was a singularly sagacious one, but it was at best a scientific guess, not a scientific measurement.

The Distance of the Stars

Just about this time, however, a great optician came to the aid of the astronomers. Joseph Fraunhofer perfected the refracting telescope, as Herschel had perfected the reflector, and invented a wonderfully accurate "heliometer," or sun-measurer. With the aid of these instruments the old and almost infinitely difficult problem of star distance was solved. In 1838 Bessel announced from the Konigsberg observatory that he had succeeded, after months of effort, in detecting and measuring the parallax of a star. Similar claims had been made often enough before, always to prove fallacious when put to further test; but this time the announcement carried the authority of one of the greatest astronomers of the age, and scepticism was silenced.

Nor did Bessel's achievement long await corroboration. Indeed, as so often happens in fields of discovery, two other workers had almost simultaneously solved the same problem--Struve at Pulkowa, where the great Russian observatory, which so long held the palm over all others, had now been established; and Thomas Henderson, then working at the Cape of Good Hope, but afterwards the Astronomer Royal of Scotland. Henderson's observations had actual precedence in point of time, but Bessel's measurements were so much more numerous and authoritative that he has been uniformly considered as deserving the chief credit of the discovery, which priority of publication secured him.

By an odd chance, the star on which Henderson's observations were made, and consequently the first star the parallax of which was ever measured, is our nearest neighbor in sidereal space, being, indeed, some ten billions of miles nearer than the one next beyond. Yet even this nearest star is more than two hundred thousand times as remote from us as the sun. The sun's light flashes to the earth in eight minutes, and to Neptune in about three and a half hours, but it requires three and a half years to signal Alpha Centauri. And as for the great majority of the stars, had they been blotted out of existence before the Christian era, we of to-day should still receive their light and seem to see them just as we do. When we look up to the sky, we study ancient history; we do not see the stars as they ARE, but as they WERE years, centuries, even millennia ago.

The information derived from the parallax of a star by no means halts with the disclosure of the distance of that body. Distance known, the proper motion of the star, hitherto only to be reckoned as so many seconds of arc, may readily be translated into actual speed of progress; relative brightness becomes absolute lustre, as compared with the sun; and in the case of the double stars the absolute mass of the components may be computed from the laws of gravitation. It is found that stars differ enormously among themselves in all these regards. As to speed, some, like our sun, barely creep through space--compassing ten or twenty miles a second, it is true, yet even at that rate only passing through the equivalent of their own diameter in a day. At the other extreme, among measured stars, is one that moves two hundred miles a second; yet even this "flying star," as seen from the earth, seems to change its place by only about three and a half lunar diameters in a thousand years. In brightness, some stars yield to the sun, while others surpass him as the arc-light surpasses a candle. Arcturus, the brightest measured star, shines like two hundred suns; and even this giant orb is dim beside those other stars which are so distant that their parallax cannot be measured, yet which greet our eyes at first magnitude. As to actual bulk, of which apparent lustre furnishes no adequate test, some stars are smaller than the sun, while others exceed him hundreds

or perhaps thousands of times. Yet one and all, so distant are they, remain mere disklike points of light before the utmost powers of the modern telescope.

Revelations of the Spectroscope

All this seems wonderful enough, but even greater things were in store. In 1859 the spectroscope came upon the scene, perfected by Kirchhoff and Bunsen, along lines pointed out by Fraunhofer almost half a century before. That marvellous instrument, by revealing the telltale lines sprinkled across a prismatic spectrum, discloses the chemical nature and physical condition of any substance whose light is submitted to it, telling its story equally well, provided the light be strong enough, whether the luminous substance be near or far--in the same room or at the confines of space. Clearly such an instrument must prove a veritable magic wand in the hands of the astronomer.

Very soon eager astronomers all over the world were putting the spectroscope to the test. Kirchhoff himself led the way, and Donati and Father Secchi in Italy, Huggins and Miller in England, and Rutherfurd in America, were the chief of his immediate followers. The results exceeded the dreams of the most visionary. At the very outset, in 1860, it was shown that such common terrestrial substances as sodium, iron, calcium, magnesium, nickel, barium, copper, and zinc exist in the form of glowing vapors in the sun, and very soon the stars gave up a corresponding secret. Since then the work of solar and sidereal analysis has gone on steadily in the hands of a multitude of workers (prominent among whom, in this country, are Professor Young of Princeton, Professor Langley of Washington, and Professor Pickering of Harvard), and more than half the known terrestrial elements have been definitely located in the sun, while fresh discoveries are in prospect.

It is true the sun also contains some seeming elements that are unknown on the earth, but this is no matter for surprise. The modern chemist makes no claim for his elements except that they have thus far resisted all human efforts to dissociate them; it would be nothing strange if some of them, when subjected to the crucible of the sun, which is seen to vaporize iron, nickel, silicon, should fail to withstand the test. But again, chemistry has by no means exhausted the resources of the earth's supply of raw material, and the substance which sends its message from a star may exist undiscovered in the dust we tread or in the air we breathe. In the year 1895 two new terrestrial elements were discovered; but one of these had for years been known to the astronomer as a solar and suspected as a stellar element, and named helium because of its abundance in the sun. The spectroscope had reached out millions of miles into space and brought back this new element, and it took the chemist a score of years to discover that he had all along had samples of the same substance unrecognized in his sublunary laboratory. There is hardly a more picturesque fact than that in the entire history of science.

But the identity in substance of earth and sun and stars was not more clearly shown than the diversity of their existing physical conditions. It was seen that sun and stars, far from being the cool, earthlike, habitable bodies that Herschel thought them (surrounded by glowing clouds, and protected from undue heat by other clouds), are in truth seething caldrons of fiery liquid, or gas made viscid by condensation, with lurid envelopes of belching flames. It was soon made clear, also, particularly by the studies of Rutherfurd and of Secchi, that stars differ among themselves in exact constitution or condition. There are white or Sirian stars, whose spectrum revels in the lines of hydrogen; yellow or solar stars (our sun being the type), showing various metallic vapors; and

sundry red stars, with banded spectra indicative of carbon compounds; besides the purely gaseous stars of more recent discovery, which Professor Pickering had specially studied. Zollner's famous interpretation of these diversities, as indicative of varying stages of cooling, has been called in question as to the exact sequence it postulates, but the general proposition that stars exist under widely varying conditions of temperature is hardly in dispute.

The assumption that different star types mark varying stages of cooling has the further support of modern physics, which has been unable to demonstrate any way in which the sun's radiated energy may be restored, or otherwise made perpetual, since meteoric impact has been shown to be--under existing conditions, at any rate--inadequate. In accordance with the theory of Helmholtz, the chief supply of solar energy is held to be contraction of the solar mass itself; and plainly this must have its limits. Therefore, unless some means as yet unrecognized is restoring the lost energy to the stellar bodies, each of them must gradually lose its lustre, and come to a condition of solidification, seeming sterility, and frigid darkness. In the case of our own particular star, according to the estimate of Lord Kelvin, such a culmination appears likely to occur within a period of five or six million years.

The Astronomy of the Invisible

But by far the strongest support of such a forecast as this is furnished by those stellar bodies which even now appear to have cooled to the final stage of star development and ceased to shine. Of this class examples in miniature are furnished by the earth and the smaller of its companion planets. But there are larger bodies of the same type out in stellar space--veritable "dark stars"--invisible, of course, yet nowadays clearly recognized.

The opening up of this "astronomy of the invisible" is another of the great achievements of the nineteenth century, and again it is Bessel to whom the honor of discovery is due. While testing his stars for parallax; that astute observer was led to infer, from certain unexplained aberrations of motion, that various stars, Sirius himself among the number, are accompanied by invisible companions, and in 1840 he definitely predicated the existence of such "dark stars." The correctness of the inference was shown twenty years later, when Alvan Clark, Jr., the American optician, while testing a new lens, discovered the companion of Sirius, which proved thus to be faintly luminous. Since then the existence of other and quite invisible star companions has been proved incontestably, not merely by renewed telescopic observations, but by the curious testimony of the ubiquitous spectroscope.

One of the most surprising accomplishments of that instrument is the power to record the flight of a luminous object directly in the line of vision. If the luminous body approaches swiftly, its Fraunhofer lines are shifted from their normal position towards the violet end of the spectrum; if it recedes, the lines shift in the opposite direction. The actual motion of stars whose distance is unknown may be measured in this way. But in certain cases the light lines are seen to oscillate on the spectrum at regular intervals. Obviously the star sending such light is alternately approaching and receding, and the inference that it is revolving about a companion is unavoidable. From this extraordinary test the orbital distance, relative mass, and actual speed of revolution of the absolutely invisible body may be determined. Thus the spectroscope, which deals only with light,

makes paradoxical excursions into the realm of the invisible. What secrets may the stars hope to conceal when questioned by an instrument of such necromantic power?

But the spectroscope is not alone in this audacious assault upon the strongholds of nature. It has a worthy companion and assistant in the photographic film, whose efficient aid has been invoked by the astronomer even more recently. Pioneer work in celestial photography was, indeed, done by Arago in France and by the elder Draper in America in 1839, but the results then achieved were only tentative, and it was not till forty years later that the method assumed really important proportions. In 1880, Dr. Henry Draper, at Hastings-on-the-Hudson, made the first successful photograph of a nebula. Soon after, Dr. David Gill, at the Cape observatory, made fine photographs of a comet, and the flecks of starlight on his plates first suggested the possibilities of this method in charting the heavens.

Since then star-charting with the film has come virtually to supersede the old method. A concerted effort is being made by astronomers in various parts of the world to make a complete chart of the heavens, and before the close of our century this work will be accomplished, some fifty or sixty millions of visible stars being placed on record with a degree of accuracy hitherto unapproachable. Moreover, other millions of stars are brought to light by the negative, which are too distant or dim to be visible with any telescopic powers yet attained--a fact which wholly discredits all previous inferences as to the limits of our sidereal system. Hence, notwithstanding the wonderful instrumental advances of the nineteenth century, knowledge of the exact form and extent of our universe seems more unattainable than it seemed a century ago.

The Structure of Nebulae

Yet the new instruments, while leaving so much untold, have revealed some vastly important secrets of cosmic structure. In particular, they have set at rest the long-standing doubts as to the real structure and position of the mysterious nebulae--those lazy masses, only two or three of them visible to the unaided eye, which the telescope reveals in almost limitless abundance, scattered everywhere among the stars, but grouped in particular about the poles of the stellar stream or disk which we call the Milky Way.

Herschel's later view, which held that some at least of the nebulae are composed of a "shining fluid," in process of condensation to form stars, was generally accepted for almost half a century. But in 1844, when Lord Rosse's great six-foot reflector--the largest telescope ever yet constructed--was turned on the nebulae, it made this hypothesis seem very doubtful. Just as Galileo's first lens had resolved the Milky Way into stars, just as Herschel had resolved nebulae that resisted all instruments but his own, so Lord Rosse's even greater reflector resolved others that would not yield to Herschel's largest mirror. It seemed a fair inference that with sufficient power, perhaps some day to be attained, all nebulae would yield, hence that all are in reality what Herschel had at first thought them-- vastly distant "island universes," composed of aggregations of stars, comparable to our own galactic system.

But the inference was wrong; for when the spectroscope was first applied to a nebula in 1864, by Dr. Huggins, it clearly showed the spectrum not of discrete stars, but of a great mass of glowing gases, hydrogen among others. More extended studies showed, it is true, that some nebulae give the

continuous spectrum of solids or liquids, but the different types intermingle and grade into one another. Also, the closest affinity is shown between nebulae and stars. Some nebulae are found to contain stars, singly or in groups, in their actual midst; certain condensed "planetary" nebulae are scarcely to be distinguished from stars of the gaseous type; and recently the photographic film has shown the presence of nebulous matter about stars that to telescopic vision differ in no respect from the generality of their fellows in the galaxy. The familiar stars of the Pleiades cluster, for example, appear on the negative immersed in a hazy blur of light. All in all, the accumulated impressions of the photographic film reveal a prodigality of nebulous matter in the stellar system not hitherto even conjectured.

And so, of course, all question of "island universes" vanishes, and the nebulae are relegated to their true position as component parts of the one stellar system--the one universe--that is open to present human inspection. And these vast clouds of world-stuff have been found by Professor Keeler, of the Lick observatory, to be floating through space at the starlike speed of from ten to thirty-eight miles per second.

The linking of nebulae with stars, so clearly evidenced by all these modern observations, is, after all, only the scientific corroboration of what the elder Herschel's later theories affirmed. But the nebulae have other affinities not until recently suspected; for the spectra of some of them are practically identical with the spectra of certain comets. The conclusion seems warranted that comets are in point of fact minor nebulae that are drawn into our system; or, putting it otherwise, that the telescopic nebulae are simply gigantic distant comets.

Lockyer's Meteoric Hypothesis

Following up the surprising clews thus suggested, Sir Norman Lockyer, of London, has in recent years elaborated what is perhaps the most comprehensive cosmogonic guess that has ever been attempted. His theory, known as the "meteoric hypothesis," probably bears the same relation to the speculative thought of our time that the nebular hypothesis of Laplace bore to that of the eighteenth century. Outlined in a few words, it is an attempt to explain all the major phenomena of the universe as due, directly or indirectly, to the gravitational impact of such meteoric particles, or specks of cosmic dust, as comets are composed of. Nebulae are vast cometary clouds, with particles more or less widely separated, giving off gases through meteoric collisions, internal or external, and perhaps glowing also with electrical or phosphorescent light. Gravity eventually brings the nebular particles into closer aggregations, and increased collisions finally vaporize the entire mass, forming planetary nebulae and gaseous stars. Continued condensation may make the stellar mass hotter and more luminous for a time, but eventually leads to its liquefaction, and ultimate consolidation-- the aforetime nebulae becoming in the end a dark or planetary star.

The exact correlation which Lockyer attempts to point out between successive stages of meteoric condensation and the various types of observed stellar bodies does not meet with unanimous acceptance. Mr. Ranyard, for example, suggests that the visible nebulae may not be nascent stars, but emanations from stars, and that the true pre-stellar nebulae are invisible until condensed to stellar proportions. But such details aside, the broad general hypothesis that all the bodies of the universe are, so to speak, of a single species-- that nebulae (including comets), stars of all types, and planets, are but varying stages in the life history of a single race or type of cosmic organisms--

is accepted by the dominant thought of our time as having the highest warrant of scientific probability.

All this, clearly, is but an amplification of that nebular hypothesis which, long before the spectroscope gave us warrant to accurately judge our sidereal neighbors, had boldly imagined the development of stars out of nebulae and of planets out of stars. But Lockyer's hypothesis does not stop with this. Having traced the developmental process from the nebular to the dark star, it sees no cause to abandon this dark star to its fate by assuming, as the original speculation assumed, that this is a culminating and final stage of cosmic existence. For the dark star, though its molecular activities have come to relative stability and impotence, still retains the enormous potentialities of molar motion; and clearly, where motion is, stasis is not. Sooner or later, in its ceaseless flight through space, the dark star must collide with some other stellar body, as Dr. Croll imagines of the dark bodies which his "pre-nebular theory" postulates. Such collision may be long delayed; the dark star may be drawn in comet-like circuit about thousands of other stellar masses, and be hurtled on thousands of diverse parabolic or elliptical orbits, before it chances to collide--but that matters not: "billions are the units in the arithmetic of eternity," and sooner or later, we can hardly doubt, a collision must occur. Then without question the mutual impact must shatter both colliding bodies into vapor, or vapor combined with meteoric fragments; in short, into a veritable nebula, the matrix of future worlds. Thus the dark star, which is the last term of one series of cosmic changes, becomes the first term of another series--at once a post-nebular and a pre-nebular condition; and the nebular hypothesis, thus amplified, ceases to be a mere linear scale, and is rounded out to connote an unending series of cosmic cycles, more nearly satisfying the imagination.

In this extended view, nebulae and luminous stars are but the infantile and adolescent stages of the life history of the cosmic individual; the dark star, its adult stage, or time of true virility. Or we may think of the shrunken dark star as the germ-cell, the pollen-grain, of the cosmic organism. Reduced in size, as becomes a germ-cell, to a mere fraction of the nebular body from which it sprang, it yet retains within its seemingly non- vital body all the potentialities of the original organism, and requires only to blend with a fellow-cell to bring a new generation into being. Thus may the cosmic race, whose aggregate census makes up the stellar universe, be perpetuated--individual solar systems, such as ours, being born, and growing old, and dying to live again in their descendants, while the universe as a whole maintains its unified integrity throughout all these internal mutations--passing on, it may be, by infinitesimal stages, to a culmination hopelessly beyond human comprehension.

III. THE NEW SCIENCE OF PALEONTOLOGY

WILLIAM SMITH AND FOSSIL SHELLS

Ever since Leonardo da Vinci first recognized the true character of fossils, there had been here and there a man who realized that the earth's rocky crust is one gigantic mausoleum. Here and there a dilettante had filled his cabinets with relics from this monster crypt; here and there a philosopher had pondered over them--questioning whether perchance they had once been alive, or whether they

were not mere abortive souvenirs of that time when the fertile matrix of the earth was supposed to have

> "teemed at a birth Innumerous living creatures, perfect forms, Limbed and full grown."

Some few of these philosophers--as Robert Hooke and Steno in the seventeenth century, and Moro, Leibnitz, Buffon, Whitehurst, Werner, Hutton, and others in the eighteenth--had vaguely conceived the importance of fossils as records of the earth's ancient history, but the wisest of them no more suspected the full import of the story written in the rocks than the average stroller in a modern museum suspects the meaning of the hieroglyphs on the case of a mummy.

It was not that the rudiments of this story are so very hard to decipher--though in truth they are hard enough--but rather that the men who made the attempt had all along viewed the subject through an atmosphere of preconception, which gave a distorted image. Before this image could be corrected it was necessary that a man should appear who could see without prejudice, and apply sound common-sense to what he saw. And such a man did appear towards the close of the century, in the person of William Smith, the English surveyor. He was a self-taught man, and perhaps the more independent for that, and he had the gift, besides his sharp eyes and receptive mind, of a most tenacious memory. By exercising these faculties, rare as they are homely, he led the way to a science which was destined, in its later developments, to shake the structure of established thought to its foundations.

Little enough did William Smith suspect, however, that any such dire consequences were to come of his act when he first began noticing the fossil shells that here and there are to be found in the stratified rocks and soils of the regions over which his surveyor's duties led him. Nor, indeed, was there anything of such apparent revolutionary character in the facts which he unearthed; yet in their implications these facts were the most disconcerting of any that had been revealed since the days of Copernicus and Galileo. In its bald essence, Smith's discovery was simply this: that the fossils in the rocks, instead of being scattered haphazard, are arranged in regular systems, so that any given stratum of rock is labelled by its fossil population; and that the order of succession of such groups of fossils is always the same in any vertical series of strata in which they occur. That is to say, if fossil A underlies fossil B in any given region, it never overlies it in any other series; though a kind of fossils found in one set of strata may be quite omitted in another. Moreover, a fossil once having disappeared never reappears in any later stratum.

From these novel facts Smith drew the commonsense inference that the earth had had successive populations of creatures, each of which in its turn had become extinct. He partially verified this inference by comparing the fossil shells with existing species of similar orders, and found that such as occur in older strata of the rocks had no counterparts among living species. But, on the whole, being eminently a practical man, Smith troubled himself but little about the inferences that might be drawn from his facts. He was chiefly concerned in using the key he had discovered as an aid to the construction of the first geological map of England ever attempted, and he left to others the untangling of any snarls of thought that might seem to arise from his discovery of the succession of varying forms of life on the globe.

He disseminated his views far and wide, however, in the course of his journeyings--quite disregarding the fact that peripatetics went out of fashion when the printing-press came in--and by the beginning of the nineteenth century he had begun to have a following among the geologists of England. It must not for a moment be supposed, however, that his contention regarding the succession of strata met with immediate or general acceptance. On the contrary, it was most bitterly antagonized. For a long generation after the discovery was made, the generality of men, prone as always to strain at gnats and swallow camels, preferred to believe that the fossils, instead of being deposited in successive ages, had been swept all at once into their present positions by the current of a mighty flood--and that flood, needless to say, the Noachian deluge. Just how the numberless successive strata could have been laid down in orderly sequence to the depth of several miles in one such fell cataclysm was indeed puzzling, especially after it came to be admitted that the heaviest fossils were not found always at the bottom; but to doubt that this had been done in some way was rank heresy in the early days of the nineteenth century.

CUVIER AND FOSSIL VERTEBRATES

But once discovered, William Smith's unique facts as to the succession of forms in the rocks would not down. There was one most vital point, however, regarding which the inferences that seem to follow from these facts needed verification--the question, namely, whether the disappearance of a fauna from the register in the rocks really implies the extinction of that fauna. Everything really depended upon the answer to that question, and none but an accomplished naturalist could answer it with authority. Fortunately, the most authoritative naturalist of the time, George Cuvier, took the question in hand--not, indeed, with the idea of verifying any suggestion of Smith's, but in the course of his own original studies--at the very beginning of the century, when Smith's views were attracting general attention.

Cuvier and Smith were exact contemporaries, both men having been born in 1769, that "fertile year" which gave the world also Chateaubriand, Von Humboldt, Wellington, and Napoleon. But the French naturalist was of very different antecedents from the English surveyor. He was brilliantly educated, had early gained recognition as a scientist, and while yet a young man had come to be known as the foremost comparative anatomist of his time. It was the anatomical studies that led him into the realm of fossils. Some bones dug out of the rocks by workmen in a quarry were brought to his notice, and at once his trained eye told him that they were different from anything he had seen before. Hitherto such bones, when not entirely ignored, had been for the most part ascribed to giants of former days, or even to fallen angels. Cuvier soon showed that neither giants nor angels were in question, but elephants of an unrecognized species. Continuing his studies, particularly with material gathered from gypsum beds near Paris, he had accumulated, by the beginning of the nineteenth century, bones of about twenty-five species of animals that he believed to be different from any now living on the globe.

The fame of these studies went abroad, and presently fossil bones poured in from all sides, and Cuvier's conviction that extinct forms of animals are represented among the fossils was sustained by the evidence of many strange and anomalous forms, some of them of gigantic size. In 1816 the famous Ossements Fossiles, describing these novel objects, was published, and vertebrate paleontology became a science. Among other things of great popular interest the book contained the first authoritative description of the hairy elephant, named by Cuvier the mammoth, the remains

of which bad been found embedded in a mass of ice in Siberia in 1802, so wonderfully preserved that the dogs of the Tungusian fishermen actually ate its flesh. Bones of the same species had been found in Siberia several years before by the naturalist Pallas, who had also found the carcass of a rhinoceros there, frozen in a mud-bank; but no one then suspected that these were members of an extinct population--they were supposed to be merely transported relics of the flood.

Cuvier, on the other hand, asserted that these and the other creatures he described had lived and died in the region where their remains were found, and that most of them have no living representatives upon the globe. This, to be sure, was nothing more than William Smith had tried all along to establish regarding lower forms of life; but flesh and blood monsters appeal to the imagination in a way quite beyond the power of mere shells; so the announcement of Cuvier's discoveries aroused the interest of the entire world, and the Ossements Fossiles was accorded a popular reception seldom given a work of technical science--a reception in which the enthusiastic approval of progressive geologists was mingled with the bitter protests of the conservatives.

"Naturalists certainly have neither explored all the continents," said Cuvier, "nor do they as yet even know all the quadrupeds of those parts which have been explored. New species of this class are discovered from time to time; and those who have not examined with attention all the circumstances belonging to these discoveries may allege also that the unknown quadrupeds, whose fossil bones have been found in the strata of the earth, have hitherto remained concealed in some islands not yet discovered by navigators, or in some of the vast deserts which occupy the middle of Africa, Asia, the two Americas, and New Holland.

"But if we carefully attend to the kind of quadrupeds that have been recently discovered, and to the circumstances of their discovery, we shall easily perceive that there is very little chance indeed of our ever finding alive those which have only been seen in a fossil state.

"Islands of moderate size, and at a considerable distance from the large continents, have very few quadrupeds. These must have been carried to them from other countries. Cook and Bougainville found no other quadrupeds besides hogs and dogs in the South Sea Islands; and the largest quadruped of the West India Islands, when first discovered, was the agouti, a species of the cavy, an animal apparently between the rat and the rabbit.

"It is true that the great continents, as Asia, Africa, the two Americas, and New Holland, have large quadrupeds, and, generally speaking, contain species common to each; insomuch, that upon discovering countries which are isolated from the rest of the world, the animals they contain of the class of quadruped were found entirely different from those which existed in other countries. Thus, when the Spaniards first penetrated into South America, they did not find it to contain a single quadruped exactly the same with those of Europe, Asia, and Africa. The puma, the jaguar, the tapir, the capybara, the llama, or glama, and vicuna, and the whole tribe of sapajous, were to them entirely new animals, of which they had not the smallest idea....

"If there still remained any great continent to be discovered, we might perhaps expect to be made acquainted with new species of large quadrupeds, among which some might be found more or less similar to those of which we find the exuviae in the bowels of the earth. But it is merely sufficient to glance the eye over the maps of the world and observe the innumerable directions in which

navigators have traversed the ocean, in order to be satisfied that there does not remain any large land to be discovered, unless it may be situated towards the Antarctic Pole, where eternal ice necessarily forbids the existence of animal life."[1]

Cuvier then points out that the ancients were well acquainted with practically all the animals on the continents of Europe, Asia, and Africa now known to scientists. He finds little grounds, therefore, for belief in the theory that at one time there were monstrous animals on the earth which it was necessary to destroy in order that the present fauna and men might flourish. After reviewing these theories and beliefs in detail, he takes up his Inquiry Respecting the Fabulous Animals of the Ancients. "It is easy," he says, "to reply to the foregoing objections, by examining the descriptions that are left us by the ancients of those unknown animals, and by inquiring into their origins. Now that the greater number of these animals have an origin, the descriptions given of them bear the most unequivocal marks; as in almost all of them we see merely the different parts of known animals united by an unbridled imagination, and in contradiction to every established law of nature."[2]

Having shown how the fabulous monsters of ancient times and of foreign nations, such as the Chinese, were simply products of the imagination, having no prototypes in nature, Cuvier takes up the consideration of the difficulty of distinguishing the fossil bones of quadrupeds.

We shall have occasion to revert to this part of Cuvier's paper in another connection. Here it suffices to pass at once to the final conclusion that the fossil bones in question are the remains of an extinct fauna, the like of which has no present-day representation on the earth. Whatever its implications, this conclusion now seemed to Cuvier to be fully established.

In England the interest thus aroused was sent to fever-heat in 1821 by the discovery of abundant beds of fossil bones in the stalagmite-covered floor of a cave at Kirkdale, Yorkshire which went to show that England, too, had once had her share of gigantic beasts. Dr. Buckland, the incumbent of the chair of geology at Oxford, and the most authoritative English geologist of his day, took these finds in hand and showed that the bones belonged to a number of species, including such alien forms as elephants, rhinoceroses, hippopotami, and hyenas. He maintained that all of these creatures had actually lived in Britain, and that the caves in which their bones were found had been the dens of hyenas.

The claim was hotly disputed, as a matter of course. As late as 1827 books were published denouncing Buckland, doctor of divinity though he was, as one who had joined in an "unhallowed cause," and reiterating the old cry that the fossils were only remains of tropical species washed thither by the deluge. That they were found in solid rocks or in caves offered no difficulty, at least not to the fertile imagination of Granville Penn, the leader of the conservatives, who clung to the old idea of Woodward and Cattcut that the deluge had dissolved the entire crust of the earth to a paste, into which the relics now called fossils had settled. The caves, said Mr. Penn, are merely the result of gases given off by the carcasses during decomposition-- great air-bubbles, so to speak, in the pasty mass, becoming caverns when the waters receded and the paste hardened to rocky consistency.

But these and such-like fanciful views were doomed even in the day of their utterance. Already in 1823 other gigantic creatures, christened ichthyosaurus and plesiosaurus by Conybeare, had been found in deeper strata of British rocks; and these, as well as other monsters whose remains were unearthed in various parts of the world, bore such strange forms that even the most sceptical could scarcely hope to find their counterparts among living creatures. Cuvier's contention that all the larger vertebrates of the existing age are known to naturalists was borne out by recent explorations, and there seemed no refuge from the conclusion that the fossil records tell of populations actually extinct. But if this were admitted, then Smith's view that there have been successive rotations of population could no longer be denied. Nor could it be in doubt that the successive faunas, whose individual remains have been preserved in myriads, representing extinct species by thousands and tens of thousands, must have required vast periods of time for the production and growth of their countless generations.

As these facts came to be generally known, and as it came to be understood in addition that the very matrix of the rock in which fossils are imbedded is in many cases one gigantic fossil, composed of the remains of microscopic forms of life, common-sense, which, after all, is the final tribunal, came to the aid of belabored science. It was conceded that the only tenable interpretation of the record in the rocks is that numerous populations of creatures, distinct from one another and from present forms, have risen and passed away; and that the geologic ages in which these creatures lived were of inconceivable length. The rank and file came thus, with the aid of fossil records, to realize the import of an idea which James Hutton, and here and there another thinker, had conceived with the swift intuition of genius long before the science of paleontology came into existence. The Huttonian proposition that time is long had been abundantly established, and by about the close of the first third of the last century geologists had begun to speak of "ages" and "untold aeons of time" with a familiarity which their predecessors had reserved for days and decades.

CHARLES LYELL COMBATS CATASTROPHISM

And now a new question pressed for solution. If the earth has been inhabited by successive populations of beings now extinct, how have all these creatures been destroyed? That question, however, seemed to present no difficulties. It was answered out of hand by the application of an old idea. All down the centuries, whatever their varying phases of cosmogonic thought, there had been ever present the idea that past times were not as recent times; that in remote epochs the earth had been the scene of awful catastrophes that have no parallel in "these degenerate days." Naturally enough, this thought, embalmed in every cosmogonic speculation of whatever origin, was appealed to in explanation of the destruction of these hitherto unimagined hosts, which now, thanks to science, rose from their abysmal slumber as incontestable, but also as silent and as thought-provocative, as Sphinx or pyramid. These ancient hosts, it was said, have been exterminated at intervals of odd millions of years by the recurrence of catastrophes of which the Mosaic deluge is the latest, but perhaps not the last.

This explanation had fullest warrant of scientific authority. Cuvier had prefaced his classical work with a speculative disquisition whose very title (Discours sur les Revolutions du Globe) is ominous of catastrophism, and whose text fully sustains the augury. And Buckland, Cuvier's foremost follower across the Channel, had gone even beyond the master, naming the work in which he described the Kirkdale fossils, Reliquiae Diluvianae, or Proofs of a Universal Deluge.

Both these authorities supposed the creatures whose remains they studied to have perished suddenly in the mighty flood whose awful current, as they supposed, gouged out the modern valleys and hurled great blocks of granite broadcast over the land. And they invoked similar floods for the extermination of previous populations.

It is true these scientific citations had met with only qualified approval at the time of their utterance, because then the conservative majority of mankind did not concede that there had been a plurality of populations or revolutions; but now that the belief in past geologic ages had ceased to be a heresy, the recurring catastrophes of the great paleontologists were accepted with acclaim. For the moment science and tradition were at one, and there was a truce to controversy, except indeed in those outlying skirmish-lines of thought whither news from headquarters does not permeate till it has become ancient history at its source.

The truce, however, was not for long. Hardly had contemporary thought begun to adjust itself to the conception of past ages of incomprehensible extent, each terminated by a catastrophe of the Noachian type, when a man appeared who made the utterly bewildering assertion that the geological record, instead of proving numerous catastrophic revolutions in the earth's past history, gives no warrant to the pretensions of any universal catastrophe whatever, near or remote.

This iconoclast was Charles Lyell, the Scotchman, who was soon to be famous as the greatest geologist of his time. As a young man he had become imbued with the force of the Huttonian proposition, that present causes are one with those that produced the past changes of the globe, and he carried that idea to what he conceived to be its logical conclusion. To his mind this excluded the thought of catastrophic changes in either inorganic or organic worlds.

But to deny catastrophism was to suggest a revolution in current thought. Needless to say, such revolution could not be effected without a long contest. For a score of years the matter was argued pro and con., often with most unscientific ardor. A mere outline of the controversy would fill a volume; yet the essential facts with which Lyell at last established his proposition, in its bearings on the organic world, may be epitomized in a few words. The evidence which seems to tell of past revolutions is the apparently sudden change of fossils from one stratum to another of the rocks. But Lyell showed that this change is not always complete. Some species live on from one alleged epoch into the next. By no means all the contemporaries of the mammoth are extinct, and numerous marine forms vastly more ancient still have living representatives.

Moreover, the blanks between strata in any particular vertical series are amply filled in with records in the form of thick strata in some geographically distant series. For example, in some regions Silurian rocks are directly overlaid by the coal measures; but elsewhere this sudden break is filled in with the Devonian rocks that tell of a great "age of fishes." So commonly are breaks in the strata in one region filled up in another that we are forced to conclude that the record shown by any single vertical series is of but local significance-- telling, perhaps, of a time when that particular sea-bed oscillated above the water-line, and so ceased to receive sediment until some future age when it had oscillated back again. But if this be the real significance of the seemingly sudden change from stratum to stratum, then the whole case for catastrophism is hopelessly lost; for such breaks in the

strata furnish the only suggestion geology can offer of sudden and catastrophic changes of wide extent.

Let us see how Lyell elaborates these ideas, particularly with reference to the rotation of species.[2]

"I have deduced as a corollary," he says, "that the species existing at any particular period must, in the course of ages, become extinct, one after the other. 'They must die out,' to borrow an emphatic expression from Buffon, 'because Time fights against them.' If the views which I have taken are just, there will be no difficulty in explaining why the habitations of so many species are now restrained within exceeding narrow limits. Every local revolution tends to circumscribe the range of some species, while it enlarges that of others; and if we are led to infer that new species originate in one spot only, each must require time to diffuse itself over a wide area. It will follow, therefore, from the adoption of our hypothesis that the recent origin of some species and the high antiquity of others are equally consistent with the general fact of their limited distribution, some being local because they have not existed long enough to admit of their wide dissemination; others, because circumstances in the animate or inanimate world have occurred to restrict the range within which they may once have obtained. . . .

"If the reader should infer, from the facts laid before him, that the successive extinction of animals and plants may be part of the constant and regular course of nature, he will naturally inquire whether there are any means provided for the repair of these losses? Is it possible as a part of the economy of our system that the habitable globe should to a certain extent become depopulated, both in the ocean and on the land, or that the variety of species should diminish until some new era arrives when a new and extraordinary effort of creative energy is to be displayed? Or is it possible that new species can be called into being from time to time, and yet that so astonishing a phenomenon can escape the naturalist?

"In the first place, it is obviously more easy to prove that a species once numerously represented in a given district has ceased to be than that some other which did not pre-exist had made its appearance--assuming always, for reasons before stated, that single stocks only of each animal and plant are originally created, and that individuals of new species did not suddenly start up in many different places at once.

"So imperfect has the science of natural history remained down to our own times that, within the memory of persons now living, the numbers of known animals and plants have doubled, or even quadrupled, in many classes. New and often conspicuous species are annually discovered in parts of the old continent long inhabited by the most civilized nations. Conscious, therefore, of the limited extent of our information, we always infer, when such discoveries are made, that the beings in question bad previously eluded our research, or had at least existed elsewhere, and only migrated at a recent period into the territories where we now find them.

"What kind of proofs, therefore, could we reasonably expect to find of the origin at a particular period of a new species?

"Perhaps, it may be said in reply, that within the last two or three centuries some forest tree or new quadruped might have been observed to appear suddenly in those parts of England or France which

had been most thoroughly investigated--that naturalists might have been able to show that no such being inhabited any other region of the globe, and that there was no tradition of anything similar having been observed in the district where it had made its appearance.

"Now, although this objection may seem plausible, yet its force will be found to depend entirely on the rate of fluctuation which we suppose to prevail in the animal world, and on the proportions which such conspicuous subjects of the animal and vegetable kingdoms bear to those which are less known and escape our observation. There are perhaps more than a million species of plants and animals, exclusive of the microscopic and infusory animalcules, now inhabiting the terraqueous globe, so that if only one of these were to become extinct annually, and one new one were to be every year called into being, much more than a million of years might be required to bring about a complete revolution of organic life.

"I am not hazarding at present any hypothesis as to the probable rate of change, but none will deny that when the annual birth and the annual death of one species on the globe is proposed as a mere speculation, this, at least, is to imagine no slight degree of instability in the animate creation. If we divide the surface of the earth into twenty regions of equal area, one of these might comprehend a space of land and water about equal in dimensions to Europe, and might contain a twentieth part of the million of species which may be assumed to exist in the animal kingdom. In this region one species only could, according to the rate of mortality before assumed, perish in twenty years, or only five out of fifty thousand in the course of a century. But as a considerable portion of the whole world belongs to the aquatic classes, with which we have a very imperfect acquaintance, we must exclude them from our consideration, and, if they constitute half of the entire number, then one species only might be lost in forty years among the terrestrial tribes. Now the mammalia, whether terrestrial or aquatic, bear so small a proportion to other classes of animals, forming less, perhaps, than a thousandth part of a whole, that, if the longevity of species in the different orders were equal, a vast period must elapse before it would come to the turn of this conspicuous class to lose one of their number. If one species only of the whole animal kingdom died out in forty years, no more than one mammifer might disappear in forty thousand years, in a region of the dimensions of Europe.

"It is easy, therefore, to see that in a small portion of such an area, in countries, for example, of the size of England and France, periods of much greater duration must elapse before it would be possible to authenticate the first appearance of one of the larger plants or animals, assuming the annual birth and death of one species to be the rate of vicissitude in the animal creation throughout the world."[3]

In a word, then, said Lyell, it becomes clear that the numberless species that have been exterminated in the past have died out one by one, just as individuals of a species die, not in vast shoals; if whole populations have passed away, it has been not by instantaneous extermination, but by the elimination of a species now here, now there, much as one generation succeeds another in the life history of any single species. The causes which have brought about such gradual exterminations, and in the long lapse of ages have resulted in rotations of population, are the same natural causes that are still in operation. Species have died out in the past as they are dying out in the present, under influence of changed surroundings, such as altered climate, or the migration into

their territory of more masterful species. Past and present causes are one--natural law is changeless and eternal.

Such was the essence of the Huttonian doctrine, which Lyell adopted and extended, and with which his name will always be associated. Largely through his efforts, though of course not without the aid of many other workers after a time, this idea--the doctrine of uniformitarianism, it came to be called--became the accepted dogma of the geologic world not long after the middle of the nineteenth century. The catastrophists, after clinging madly to their phantom for a generation, at last capitulated without terms: the old heresy became the new orthodoxy, and the way was paved for a fresh controversy.

THE ORIGIN OF SPECIES

The fresh controversy followed quite as a matter of course. For the idea of catastrophism had not concerned the destruction of species merely, but their introduction as well. If whole faunas had been extirpated suddenly, new faunas had presumably been introduced with equal suddenness by special creation; but if species die out gradually, the introduction of new species may be presumed to be correspondingly gradual. Then may not the new species of a later geological epoch be the modified lineal descendants of the extinct population of an earlier epoch?

The idea that such might be the case was not new. It had been suggested when fossils first began to attract conspicuous attention; and such sagacious thinkers as Buffon and Kant and Goethe and Erasmus Darwin had been disposed to accept it in the closing days of the eighteenth century. Then, in 1809, it had been contended for by one of the early workers in systematic paleontology--Jean Baptiste Lamarck, who had studied the fossil shells about Paris while Cuvier studied the vertebrates, and who had been led by these studies to conclude that there had been not merely a rotation but a progression of life on the globe. He found the fossil shells--the fossils of invertebrates, as he himself had christened them--in deeper strata than Cuvier's vertebrates; and he believed that there had been long ages when no higher forms than these were in existence, and that in successive ages fishes, and then reptiles, had been the highest of animate creatures, before mammals, including man, appeared. Looking beyond the pale of his bare facts, as genius sometimes will, he had insisted that these progressive populations had developed one from another, under influence of changed surroundings, in unbroken series.

Of course such a thought as this was hopelessly misplaced in a generation that doubted the existence of extinct species, and hardly less so in the generation that accepted catastrophism; but it had been kept alive by here and there an advocate like Geoffrey Saint-Hilaire, and now the banishment of catastrophism opened the way for its more respectful consideration. Respectful consideration was given it by Lyell in each recurring edition of his Principles, but such consideration led to its unqualified rejection. In its place Lyell put forward a modified hypothesis of special creation. He assumed that from time to time, as the extirpation of a species had left room, so to speak, for a new species, such new species had been created de novo; and he supposed that such intermittent, spasmodic impulses of creation manifest themselves nowadays quite as frequently as at any time in the past. He did not say in so many words that no one need be surprised to-day were he to see a new species of deer, for example, come up out of the ground before him, "pawing to get free," like Milton's lion, but his theory implied as much. And that theory, let it be noted, was not the

theory of Lyell alone, but of nearly all his associates in the geologic world. There is perhaps no other fact that will bring home to one so vividly the advance in thought of our own generation as the recollection that so crude, so almost unthinkable a conception could have been the current doctrine of science less than half a century ago.

This theory of special creation, moreover, excluded the current doctrine of uniformitarianism as night excludes day, though most thinkers of the time did not seem to be aware of the incompatibility of the two ideas. It may be doubted whether even Lyell himself fully realized it. If he did, he saw no escape from the dilemma, for it seemed to him that the record in the rocks clearly disproved the alternative Lamarckian hypothesis. And almost with one accord the paleontologists of the time sustained the verdict. Owen, Agassiz, Falconer, Barrande, Pictet, Forbes, repudiated the idea as unqualifiedly as their great predecessor Cuvier had done in the earlier generation. Some of them did, indeed, come to believe that there is evidence of a progressive development of life in the successive ages, but no such graded series of fossils had been discovered as would give countenance to the idea that one species had ever been transformed into another. And to nearly every one this objection seemed insuperable.

But in 1859 appeared a book which, though not dealing primarily with paleontology, yet contained a chapter that revealed the geological record in an altogether new light. The book was Charles Darwin's Origin of Species, the chapter that wonderful citation of the "Imperfections of the Geological Record." In this epoch-making chapter Darwin shows what conditions must prevail in any given place in order that fossils shall be formed, how unusual such conditions are, and how probable it is that fossils once imbedded in sediment of a sea-bed will be destroyed by metamorphosis of the rocks, or by denudation when the strata are raised above the water-level. Add to this the fact that only small territories of the earth have been explored geologically, he says, and it becomes clear that the paleontological record as we now possess it shows but a mere fragment of the past history of organisms on the earth. It is a history "imperfectly kept and written in a changing dialect. Of this history we possess the last volume alone, relating only to two or three countries. Of this volume only here and there a short chapter has been preserved, and of each page only here and there a few lines." For a paleontologist to dogmatize from such a record would be as rash, he thinks, as "for a naturalist to land for five minutes on a barren point of Australia and then discuss the number and range of its productions."

This citation of observations, which when once pointed out seemed almost self-evident, came as a revelation to the geological world. In the clarified view now possible old facts took on a new meaning. It was recalled that Cuvier had been obliged to establish a new order for some of the first fossil creatures he examined, and that Buckland had noted that the nondescript forms were intermediate in structure between allied existing orders. More recently such intermediate forms had been discovered over and over; so that, to name but one example, Owen had been able, with the aid of extinct species, to "dissolve by gradations the apparently wide interval between the pig and the camel." Owen, moreover, had been led to speak repeatedly of the "generalized forms" of extinct animals, and Agassiz had called them "synthetic or prophetic types," these terms clearly implying "that such forms are in fact intermediate or connecting links." Darwin himself had shown some years before that the fossil animals of any continent are closely related to the existing animals of that continent--edentates predominating, for example, in South America, and marsupials in Australia. Many observers had noted that recent strata everywhere show a fossil fauna more nearly

like the existing one than do more ancient strata; and that fossils from any two consecutive strata are far more closely related to each other than are the fossils of two remote formations, the fauna of each geological formation being, indeed, in a wide view, intermediate between preceding and succeeding faunas.

So suggestive were all these observations that Lyell, the admitted leader of the geological world, after reading Darwin's citations, felt able to drop his own crass explanation of the introduction of species and adopt the transmutation hypothesis, thus rounding out the doctrine of uniformitarianism to the full proportions in which Lamarck had conceived it half a century before. Not all paleontologists could follow him at once, of course; the proof was not yet sufficiently demonstrative for that; but all were shaken in the seeming security of their former position, which is always a necessary stage in the progress of thought. And popular interest in the matter was raised to white heat in a twinkling.

So, for the third time in this first century of its existence, paleontology was called upon to play a leading role in a controversy whose interest extended far beyond the bounds of staid truth-seeking science. And the controversy waged over the age of the earth had not been more bitter, that over catastrophism not more acrimonious, than that which now raged over the question of the transmutation of species. The question had implications far beyond the bounds of paleontology, of course. The main evidence yet presented had been drawn from quite other fields, but by common consent the record in the rocks might furnish a crucial test of the truth or falsity of the hypothesis. "He who rejects this view of the imperfections of the geological record," said Darwin, "will rightly reject the whole theory."

With something more than mere scientific zeal, therefore, paleontologists turned anew to the records in the rocks, to inquire what evidence in proof or refutation might be found in unread pages of the "great stone book." And, as might have been expected, many minds being thus prepared to receive new evidence, such evidence was not long withheld.

FOSSIL MAN

Indeed, at the moment of Darwin's writing a new and very instructive chapter of the geologic record was being presented to the public--a chapter which for the first time brought man into the story. In 1859 Dr. Falconer, the distinguished British paleontologist, made a visit to Abbeville, in the valley of the Somme, incited by reports that for a decade before bad been sent out from there by M. Boucher de Perthes. These reports had to do with the alleged finding of flint implements, clearly the work of man, in undisturbed gravel- beds, in the midst of fossil remains of the mammoth and other extinct animals. What Falconer saw there and what came of his visit may best be told in his own words:

"In September of 1856 I made the acquaintance of my distinguished friend M. Boucher de Perthes," wrote Dr. Falconer, "on the introduction of M. Desnoyers at Paris, when he presented to me the earlier volume of his Antiquites celtiques, etc., with which I thus became acquainted for the first time. I was then fresh from the examination of the Indian fossil remains of the valley of the Jumna; and the antiquity of the human race being a subject of interest to both, we conversed freely about it, each from a different point of view. M. de Perthes invited me to visit Abbeville, in order to

examine his antediluvian collection, fossil and geological, gleaned from the valley of the Somme. This I was unable to accomplish then, but I reserved it for a future occasion.

"In October, 1856, having determined to proceed to Sicily, I arranged by correspondence with M. Boucher de Perthes to visit Abbeville on my journey through France. I was at the time in constant communication with Mr. Prestwich about the proofs of the antiquity of the human race yielded by the Broxham Cave, in which he took a lively interest; and I engaged to communicate to him the opinions at which I should arrive, after my examination of the Abbeville collection. M. de Perthes gave me the freest access to his materials, with unreserved explanations of all the facts of the case that had come under his observation; and having considered his Menchecourt Section, taken with such scrupulous care, and identified the molars of elephas primigenius, which he had exhumed with his own hands deep in that section, along with flint weapons, presenting the same character as some of those found in the Broxham Cave, I arrived at the conviction that they were of contemporaneous age, although I was not prepared to go along with M. de Perthes in all his inferences regarding the hieroglyphics and in an industrial interpretation of the various other objects which he had met with."[4]

That Dr. Falconer was much impressed by the collection of M. de Perthes is shown in a communication which he sent at once to his friend Prestwich:

"I have been richly rewarded," he exclaims. "His collection of wrought flint implements, and of the objects of every description associated with them, far exceeds everything I expected to have seen, especially from a single locality. He has made great additions, since the publication of his first volume, in the second, which I now have by me. He showed me flint hatchets which HE HAD DUG UP with his own hands, mixed INDISCRIMINATELY with molars of elephas primigenius. I examined and identified plates of the molars and the flint objects which were got along with them. Abbeville is an out-of-the-way place, very little visited; and the French savants who meet him in Paris laugh at Monsieur de Perthes and his researches. But after devoting the greater part of a day to his vast collection, I am perfectly satisfied that there is a great deal of fair presumptive evidence in favor of many of his speculations regarding the remote antiquity of these industrial objects and their association with animals now extinct. M. Boucher's hotel is, from the ground floor to garret, a continued museum, filled with pictures, mediaeval art, and Gaulish antiquities, including antediluvian flint-knives, fossil-bones, etc. If, during next summer, you should happen to be paying a visit to France, let me strongly recommend you to come to Abbeville. I am sure you would be richly rewarded."[5]

This letter aroused the interest of the English geologists, and in the spring of 1859 Prestwich and Mr. (afterwards Sir John) Evans made a visit to Abbeville to see the specimens and examine at first hand the evidences as pointed out by Dr. Falconer. "The evidence yielded by the valley of the Somme," continues Falconer, in speaking of this visit, "was gone into with the scrupulous care and severe and exhaustive analysis which are characteristic of Mr. Prestwich's researches. The conclusions to which he was conducted were communicated to the Royal Society on May 12, 1859, in his celebrated memoir, read on May 26th and published in the Philosophical Transactions of 1860, which, in addition to researches made in the valley of the Somme, contained an account of similar phenomena presented by the valley of the Waveney, near Hoxne, in Suffolk. Mr. Evans communicated to the Society of Antiquaries a memoir on the character and geological position of

the 'Flint Implements in the Drift,' which appeared in the Archaeologia for 1860. The results arrived at by Mr. Prestwich were expressed as follows:

"First. That the flint implements are the result of design and the work of man.

"Second. That they are found in beds of gravel, sand, and clay, which have never been artificially disturbed.

"Third. That they occur associated with the remains of land, fresh-water, and marine testacea, of species now living, and most of them still common in the same neighborhood, and also with the remains of various mammalia--a few species now living, but more of extinct forms.

"Fourth. That the period at which their entombment took place was subsequent to the bowlder-clay period, and to that extent post-glacial; and also that it was among the latest in geological time--one apparently anterior to the surface assuming its present form, so far as it regards some of the minor features."[6]

These reports brought the subject of the very significant human fossils at Abbeville prominently before the public; whereas the publications of the original discoverer, Boucher de Perthes, bearing date of 1847, had been altogether ignored. A new aspect was thus given to the current controversy.

As Dr. Falconer remarked, geology was now passing through the same ordeal that astronomy passed in the age of Galileo. But the times were changed since the day when the author of the Dialogues was humbled before the Congregation of the Index, and now no Index Librorum Prohibitorum could avail to hide from eager human eyes such pages of the geologic story as Nature herself had spared. Eager searchers were turning the leaves with renewed zeal everywhere, and with no small measure of success. In particular, interest attached just at this time to a human skull which Dr. Fuhlrott had discovered in a cave at Neanderthal two or three years before--a cranium which has ever since been famous as the Neanderthal skull, the type specimen of what modern zoologists are disposed to regard as a distinct species of man, Homo neanderthalensis. Like others of the same type since discovered at Spy, it is singularly simian in character--low-arched, with receding forehead and enormous, protuberant eyebrows. When it was first exhibited to the scientists at Berlin by Dr. Fuhlrott, in 1857, its human character was doubted by some of the witnesses; of that, however, there is no present question.

This interesting find served to recall with fresh significance some observations that had been made in France and Belgium a long generation earlier, but whose bearings had hitherto been ignored. In 1826 MM. Tournal and Christol had made independent discoveries of what they believed to be human fossils in the caves of the south of France; and in 1827 Dr. Schmerling had found in the cave of Engis, in Westphalia, fossil bones of even greater significance. Schmerling's explorations had been made with the utmost care, and patience. At Engis he had found human bones, including skulls, intermingled with those of extinct mammals of the mammoth period in a way that left no doubt in his mind that all dated from the same geological epoch. He bad published a full account of his discoveries in an elaborate monograph issued in 1833.

But at that time, as it chanced, human fossils were under a ban as effectual as any ever pronounced by canonical index, though of far different origin. The oracular voice of Cuvier had declared against the authenticity of all human fossils. Some of the bones brought him for examination the great anatomist had pettishly pitched out of the window, declaring them fit only for a cemetery, and that had settled the matter for a generation: the evidence gathered by lesser workers could avail nothing against the decision rendered at the Delphi of Science. But no ban, scientific or canonical, can longer resist the germinative power of a fact, and so now, after three decades of suppression, the truth which Cuvier had buried beneath the weight of his ridicule burst its bonds, and fossil man stood revealed, if not as a flesh-and-blood, at least as a skeletal entity.

The reception now accorded our prehistoric ancestor by the progressive portion of the scientific world amounted to an ovation; but the unscientific masses, on the other hand, notwithstanding their usual fondness for tracing remote genealogies, still gave the men of Engis and Neanderthal the cold shoulder. Nor were all of the geologists quite agreed that the contemporaneity of these human fossils with the animals whose remains had been mingled with them had been fully established. The bare possibility that the bones of man and of animals that long preceded him had been swept together into the eaves in successive ages, and in some mysterious way intermingled there, was clung to by the conservatives as a last refuge. But even this small measure of security was soon to be denied them, for in 1865 two associated workers, M. Edouard Lartet and Mr. Henry Christy, in exploring the caves of Dordogne, unearthed a bit of evidence against which no such objection could be urged. This momentous exhibit was a bit of ivory, a fragment of the tusk of a mammoth, on which was scratched a rude but unmistakable outline portrait of the mammoth itself. If all the evidence as to man's antiquity before presented was suggestive merely, here at last was demonstration; for the cave-dwelling man could not well have drawn the picture of the mammoth unless he had seen that animal, and to admit that man and the mammoth had been contemporaries was to concede the entire case. So soon, therefore, as the full import of this most instructive work of art came to be realized, scepticism as to man's antiquity was silenced for all time to come.

In the generation that has elapsed since the first drawing of the cave-dweller artist was discovered, evidences of the wide-spread existence of man in an early epoch have multiplied indefinitely, and to-day the paleontologist traces the history of our race back beyond the iron and bronze ages, through a neolithic or polished-stone age, to a paleolithic or rough-stone age, with confidence born of unequivocal knowledge. And he looks confidently to the future explorer of the earth's fossil records to extend the history back into vastly more remote epochs, for it is little doubted that paleolithic man, the most ancient of our recognized progenitors, is a modern compared to those generations that represented the real childhood of our race.

THE FOSSIL-BEDS OF AMERICA

Coincidently with the discovery of these highly suggestive pages of the geologic story, other still more instructive chapters were being brought to light in America. It was found that in the Rocky Mountain region, in strata found in ancient lake beds, records of the tertiary period, or age of mammals, had been made and preserved with fulness not approached in any other region hitherto geologically explored. These records were made known mainly by Professors Joseph Leidy, O. C. Marsh, and E. D. Cope, working independently, and more recently by numerous younger paleontologists.

The profusion of vertebrate remains thus brought to light quite beggars all previous exhibits in point of mere numbers. Professor Marsh, for example, who was first in the field, found three hundred new tertiary species between the years 1870 and 1876. Meanwhile, in cretaceous strata, he unearthed remains of about two hundred birds with teeth, six hundred pterodactyls, or flying dragons, some with a spread of wings of twenty- five feet, and one thousand five hundred mosasaurs of the sea-serpent type, some of them sixty feet or more in length. In a single bed of Jurassic rock, not larger than a good-sized lecture-room, he found the remains of one hundred and sixty individuals of mammals, representing twenty species and nine genera; while beds of the same age have yielded three hundred reptiles, varying from the size of a rabbit to sixty or eighty feet in length.

But the chief interest of these fossils from the West is not their number but their nature; for among them are numerous illustrations of just such intermediate types of organisms as must have existed in the past if the succession of life on the globe has been an unbroken lineal succession. Here are reptiles with bat-like wings, and others with bird-like pelves and legs adapted for bipedal locomotion. Here are birds with teeth, and other reptilian characters. In short, what with reptilian birds and birdlike reptiles, the gap between modern reptiles and birds is quite bridged over. In a similar way, various diverse mammalian forms, as the tapir, the rhinoceros, and the horse, are linked together by fossil progenitors. And, most important of all, Professor Marsh has discovered a series of mammalian remains, occurring in successive geological epochs, which are held to represent beyond cavil the actual line of descent of the modern horse; tracing the lineage of our one-toed species back through two and three toed forms, to an ancestor in the eocene or early tertiary that had four functional toes and the rudiment of a fifth. This discovery is too interesting and too important not to be detailed at length in the words of the discoverer.

Marsh Describes the Fossil Horse

"It is a well-known fact," says Professor Marsh, "that the Spanish discoverers of America discovered no horses on this continent, and that the modern horse (Equus caballus, Linn.) was subsequently introduced from the Old World. It is, however, not so generally known that these animals had formerly been abundant here, and that long before, in tertiary time, near relatives of the horse, and probably his ancestors, existed in the far West in countless numbers and in a marvellous variety of forms. The remains of equine mammals, now known from the tertiary and quaternary deposits of this country, already represent more than double the number of genera and species hitherto found in the strata of the eastern hemisphere, and hence afford most important aid in tracing out the genealogy of the horses still existing.

"The animals of this group which lived in America during the three diversions of the tertiary period were especially numerous in the Rocky Mountain regions, and their remains are well preserved in the old lake basins which then covered so much of that country. The most ancient of these lakes--which extended over a considerable part of the present territories of Wyoming and Utah--remained so long in eocene times that the mud and sand, slowly deposited in it, accumulated to more than a mile in vertical thickness. In these deposits vast numbers of tropical animals were entombed, and here the oldest equine remains occur, four species of which have been described. These belong to the genus Orohippus (Marsh), and are all of a diminutive size, hardly bigger than a fox. The

skeletons of these animals resemble that of the horse in many respects, much more indeed than any other existing species, but, instead of the single toe on each foot, so characteristic of all modern equines, the various species of Orohippus had four toes before and three behind, all of which reached the ground. The skull, too, was proportionately shorter, and the orbit was not enclosed behind by a bridge of bone. There were fifty four teeth in all, and the premolars were larger than the molars. The crowns of these teeth were very short. The canine teeth were developed in both sexes, and the incisors did not have the "mark" which indicates the age of the modern horse. The radius and ulna were separate, and the latter was entire through the whole length. The tibia and fibula were distinct. In the forefoot all the digits except the pollex, or first, were well developed. The third digit is the largest, and its close resemblance to that of the horse is clearly marked. The terminal phalanx, or coffin-bone, has a shallow median bone in front, as in many species of this group in the later tertiary. The fourth digit exceeds the second in size, and the second is much the shortest of all. Its metacarpal bone is considerably curved outward. In the hind-foot of this genus there are but three digits. The fourth metatarsal is much larger than the second.

"The larger number of equine mammals now known from the tertiary deposits of this country, and their regular distributions through the subdivisions of this formation, afford a good opportunity to ascertain the probable descent of the modern horse. The American representative of the latter is the extinct Equus fraternus (Leidy), a species almost, if not wholly, identical with the Old World Equus caballus (Linnaeus), to which our recent horse belongs. Huxley has traced successfully the later genealogy of the horse through European extinct forms, but the line in America was probably a more direct one, and the record is more complete. Taking, then, as the extreme of a series, Orohippus agilis (Marsh), from the eocene, and Equus fraternus (Leidy), from the quaternary, intermediate forms may be intercalated with considerable certainty from thirty or more well-marked species that lived in the intervening periods. The natural line of descent would seem to be through the following genera: Orohippus, of the eocene; Miohippus and Anchitherium, of the miocene; Anchippus, Hipparion, Protohippus, Phohippus, of the pliocene; and Equus, quaternary and recent.

The most marked changes undergone by the successive equine genera are as follows: First, increase in size; second, increase in speed, through concentration of limb bones; third, elongation of head and neck, and modifications of skull. The eocene Orohippus was the size of a fox. Miohippus and Anchitherium, from the miocene, were about as large as a sheep. Hipparion and Pliohippus, of the pliocene, equalled the ass in height; while the size of the quaternary Equus was fully up to that of a modern horse.

"The increase of speed was equally well marked, and was a direct result of the gradual formation of the limbs. The latter were slowly concentrated by the reduction of their lateral elements and enlargement of the axial bone, until the force exerted by each limb came to act directly through its axis in the line of motion. This concentration is well seen--e.g., in the fore-limb. There was, first, a change in the scapula and humerus, especially in the latter, which facilitated motion in one line only; second, an expansion of the radius and reduction of the ulna, until the former alone remained entire and effective; third, a shortening of all the carpal bones and enlargement of the median ones, insuring a firmer wrist; fourth, an increase of size of the third digit, at the expense of those of each side, until the former alone supported the limb.

"Such is, in brief, a general outline of the more marked changes that seemed to have produced in America the highly specialized modern Equus from his diminutive four-toed predecessor, the eocene Orohippus. The line of descent appears to have been direct, and the remains now known supply every important intermediate form. It is, of course, impossible to say with certainty through which of the three-toed genera of the pliocene that lived together the succession came. It is not impossible that the latter species, which appear generically identical, are the descendants of more distinct pliocene types, as the persistent tendency in all the earlier forms was in the same direction. Considering the remarkable development of the group through the tertiary period, and its existence even later, it seems very strange that none of the species should have survived, and that we are indebted for our present horse to the Old World."[7]

PALEONTOLOGY OF EVOLUTION

These and such-like revelations have come to light in our own time--are, indeed, still being disclosed. Needless to say, no index of any sort now attempts to conceal them; yet something has been accomplished towards the same end by the publication of the discoveries in Smithsonian bulletins and in technical memoirs of government surveys. Fortunately, however, the results have been rescued from that partial oblivion by such interpreters as Professors Huxley and Cope, so the unscientific public has been allowed to gain at least an inkling of the wonderful progress of paleontology in our generation.

The writings of Huxley in particular epitomize the record. In 1862 he admitted candidly that the paleontological record as then known, so far as it bears on the doctrine of progressive development, negatives that doctrine. In 1870 he was able to "soften somewhat the Brutus-like severity" of his former verdict, and to assert that the results of recent researches seem "to leave a clear balance in favor of the doctrine of the evolution of living forms one from another." Six years later, when reviewing the work of Marsh in America and of Gaudry in Pikermi, he declared that, "on the evidence of paleontology, the evolution of many existing forms of animal life from their predecessors is no longer an hypothesis, but an historical fact." In 1881 he asserted that the evidence gathered in the previous decade had been so unequivocal that, had the transmutation hypothesis not existed, "the paleontologist would have had to invent it."

Since then the delvers after fossils have piled proof on proof in bewildering profusion. The fossil-beds in the "bad lands" of western America seem inexhaustible. And in the Connecticut River Valley near relatives of the great reptiles which Professor Marsh and others have found in such profusion in the West left their tracks on the mud-flats--since turned to sandstone; and a few skeletons also have been found. The bodies of a race of great reptiles that were the lords of creation of their day have been dissipated to their elements, while the chance indentations of their feet as they raced along the shores, mere footprints on the sands, have been preserved among the most imperishable of the memory-tablets of the world.

Of the other vertebrate fossils that have been found in the eastern portions of America, among the most abundant and interesting are the skeletons of mastodons. Of these one of the largest and most complete is that which was unearthed in the bed of a drained lake near Newburg, New York, in 1845. This specimen was larger than the existing elephants, and had tusks eleven feet in length. It

was mounted and described by Dr. John C. Warren, of Boston, and has been famous for half a century as the "Warren mastodon."

But to the student of racial development as recorded by the fossils all these sporadic finds have but incidental interest as compared with the rich Western fossil- beds to which we have already referred. From records here unearthed, the racial evolution of many mammals has in the past few years been made out in greater or less detail. Professor Cope has traced the ancestry of the camels (which, like the rhinoceroses, hippopotami, and sundry other forms now spoken of as "Old World," seem to have had their origin here) with much completeness.

A lemuroid form of mammal, believed to be of the type from which man has descended, has also been found in these beds. It is thought that the descendants of this creature, and of the other "Old-World" forms above referred to, found their way to Asia, probably, as suggested by Professor Marsh, across a bridge at Bering Strait, to continue their evolution on the other hemisphere, becoming extinct in the land of their nativity. The ape-man fossil found in the tertiary strata of the island of Java in 1891 by the Dutch surgeon Dr. Eugene Dubois, and named Pithecanthropus erectus, may have been a direct descendant of the American tribe of primitive lemurs, though this is only a conjecture.

Not all the strange beasts which have left their remains in our "bad lands" are represented by living descendants. The titanotheres, or brontotheridae, for example, a gigantic tribe, offshoots of the same stock which produced the horse and rhinoceros, represented the culmination of a line of descent. They developed rapidly in a geological sense, and flourished about the middle of the tertiary period; then, to use Agassiz's phrase," time fought against them." The story of their evolution has been worked out by Professors Leidy, Marsh, Cope, and H. F. Osborne.

A recent bit of paleontological evidence bearing on the question of the introduction of species is that presented by Dr. J. L. Wortman in connection with the fossil lineage of the edentates. It was suggested by Marsh, in 1877, that these creatures, whose modern representatives are all South American, originated in North America long before the two continents had any land connection. The stages of degeneration by which these animals gradually lost the enamel from their teeth, coming finally to the unique condition of their modern descendants of the sloth tribe, are illustrated by strikingly graded specimens now preserved in the American Museum of Natural History, as shown by Dr. Wortman.

All these and a multitude of other recent observations that cannot be even outlined here tell the same story. With one accord paleontologists of our time regard the question of the introduction of new species as solved. As Professor Marsh has said, "to doubt evolution today is to doubt science; and science is only another name for truth."

Thus the third great battle over the meaning of the fossil records has come to a conclusion. Again there is a truce to controversy, and it may seem to the casual observer that the present stand of the science of fossils is final and impregnable. But does this really mean that a full synopsis of the story of paleontology has been told? Or do we only await the coming of the twentieth-century Lamarck or Darwin, who shall attack the fortified knowledge of to-day with the batteries of a new generalization?

IV. THE ORIGIN AND DEVELOPMENT OF MODERN GEOLOGY

JAMES HUTTON

One might naturally suppose that the science of the earth which lies at man's feet would at least have kept pace with the science of the distant stars. But perhaps the very obviousness of the phenomena delayed the study of the crust of the earth. It is the unattainable that allures and mystifies and enchants the developing mind. The proverbial child spurns its toys and cries for the moon.

So in those closing days of the eighteenth century, when astronomers had gone so far towards explaining the mysteries of the distant portions of the universe, we find a chaos of opinion regarding the structure and formation of the earth. Guesses were not wanting to explain the formation of the world, it is true, but, with one or two exceptions, these are bizarre indeed. One theory supposed the earth to have been at first a solid mass of ice, which became animated only after a comet had dashed against it. Other theories conceived the original globe as a mass of water, over which floated vapors containing the solid elements, which in due time were precipitated as a crust upon the waters. In a word, the various schemes supposed the original mass to have been ice, or water, or a conglomerate of water and solids, according to the random fancies of the theorists; and the final separation into land and water was conceived to have taken place in all the ways which fancy, quite unchecked by any tenable data, could invent.

Whatever important changes in the general character of the surface of the globe were conceived to have taken place since its creation were generally associated with the Mosaic: deluge, and the theories which attempted to explain this catastrophe were quite on a par with those which dealt with a remoter period of the earth's history. Some speculators, holding that the interior of the globe is a great abyss of waters, conceived that the crust had dropped into this chasm and had thus been inundated. Others held that the earth had originally revolved on a vertical axis, and that the sudden change to its present position bad caused the catastrophic shifting of its oceans. But perhaps the favorite theory was that which supposed a comet to have wandered near the earth, and in whirling about it to have carried the waters, through gravitation, in a vast tide over the continents.

Thus blindly groped the majority of eighteenth-century philosophers in their attempts to study what we now term geology. Deluded by the old deductive methods, they founded not a science, but the ghost of a science, as immaterial and as unlike anything in nature as any other phantom that could be conjured from the depths of the speculative imagination. And all the while the beckoning earth lay beneath the feet of these visionaries; but their eyes were fixed in air.

At last, however, there came a man who had the penetration to see that the phantom science of geology needed before all else a body corporeal, and who took to himself the task of supplying it. This was Dr. James Hutton, of Edinburgh, physician, farmer, and manufacturing chemist--patient, enthusiastic, level-headed devotee of science. Inspired by his love of chemistry to study the character of rocks and soils, Hutton had not gone far before the earth stood revealed to him in a

new light. He saw, what generations of predecessors had blindly refused to see, that the face of nature everywhere, instead of being rigid and immutable, is perennially plastic, and year by year is undergoing metamorphic changes. The solidest rocks are day by day disintegrated slowly, but none the less surely, by wind and rain and frost, by mechanical attrition and chemical decomposition, to form the pulverized earth and clay. This soil is being swept away by perennial showers, and carried off to the oceans. The oceans themselves beat on their shores, and eat insidiously into the structure of sands and rocks. Everywhere, slowly but surely, the surface of the land is being worn away; its substance is being carried to burial in the seas.

Should this denudation continue long enough, thinks Hutton, the entire surface of the continents must be worn away. Should it be continued LONG ENOUGH! And with that thought there flashes on his mind an inspiring conception--the idea that solar time is long, indefinitely long. That seems a simple enough thought --almost a truism--to the twentieth-century mind; but it required genius to conceive it in the eighteenth. Hutton pondered it, grasped its full import, and made it the basis of his hypothesis, his "theory of the earth."

MODERN GEOLOGY

The hypothesis is this--that the observed changes of the surface of the earth, continued through indefinite lapses of time, must result in conveying all the land at last to the sea; in wearing continents away till the oceans overflow them. What then? Why, as the continents wear down, the oceans are filling up. Along their bottoms the detritus of wasted continents is deposited in strata, together with the bodies of marine animals and vegetables. Why might not this debris solidify to form layers of rocks--the basis of new continents? Why not, indeed?

But have we any proof that such formation of rocks in an ocean-bed has, in fact, occurred? To be sure we have. It is furnished by every bed of limestone, every outcropping fragment of fossil-bearing rock, every stratified cliff. How else than through such formation in an ocean-bed came these rocks to be stratified? How else came they to contain the shells of once living organisms imbedded in their depths? The ancients, finding fossil shells imbedded in the rocks, explained them as mere freaks of "nature and the stars." Less superstitious generations had repudiated this explanation, but had failed to give a tenable solution of the mystery. To Hutton it is a mystery no longer. To him it seems clear that the basis of the present continents was laid in ancient sea-beds, formed of the detritus of continents yet more ancient.

But two links are still wanting to complete the chain of Hutton's hypothesis. Through what agency has the ooze of the ocean-bed been transformed into solid rock? and through what agency has this rock been lifted above the surface of the water to form new continents? Hutton looks about him for a clew, and soon he finds it. Everywhere about us there are outcropping rocks that are not stratified, but which give evidence to the observant eye of having once been in a molten state. Different minerals are mixed together; pebbles are scattered through masses of rock like plums in a pudding; irregular crevices in otherwise solid masses of rock--so-called veinings--are seen to be filled with equally solid granite of a different variety, which can have gotten there in no conceivable way, so Hutton thinks, but by running in while molten, as liquid metal is run into the moulds of the founder. Even the stratified rocks, though they seemingly have not been melted, give evidence in some

instances of having been subjected to the action of heat. Marble, for example, is clearly nothing but calcined limestone.

With such evidence before him, Hutton is at no loss to complete his hypothesis. The agency which has solidified the ocean-beds, he says, is subterranean heat. The same agency, acting excessively, has produced volcanic cataclysms, upheaving ocean-beds to form continents. The rugged and uneven surfaces of mountains, the tilted and broken character of stratified rocks everywhere, are the standing witnesses of these gigantic upheavals.

And with this the imagined cycle is complete. The continents, worn away and carried to the sea by the action of the elements, have been made over into rocks again in the ocean-beds, and then raised once more into continents. And this massive cycle, In Hutton's scheme, is supposed to have occurred not once only, but over and over again, times without number. In this unique view ours is indeed a world without beginning and without end; its continents have been making and unmaking in endless series since time began.

Hutton formulated his hypothesis while yet a young man, not long after the middle of the century. He first gave it publicity in 1781, in a paper before the Royal Society of Edinburgh:

"A solid body of land could not have answered the purpose of a habitable world," said Hutton, "for a soil is necessary to the growth of plants, and a soil is nothing but the material collected from the destruction of the solid land. Therefore the surface of this land inhabited by man, and covered by plants and animals, is made by nature to decay, in dissolving from that hard and compact state in which it is found; and this soil is necessarily washed away by the continual circulation of the water running from the summits of the mountains towards the general receptacle of that fluid.

"The heights of our land are thus levelled with our shores, our fertile plains are formed from the ruins of the mountains; and those travelling materials are still pursued by the moving water, and propelled along the inclined surface of the earth. These movable materials, delivered into the sea, cannot, for a long continuance, rest upon the shore, for by the agitation of the winds, the tides, and the currents every movable thing is carried farther and farther along the shelving bottom of the sea, towards the unfathomable regions of the ocean.

"If the vegetable soil is thus constantly removed from the surface of the land, and if its place is then to be supplied from the dissolution of the solid earth as here represented, we may perceive an end to this beautiful machine; an end arising from no error in its constitution as a world, but from that destructibility of its land which is so necessary in the system of the globe, in the economy of life and vegetation.

"The immense time necessarily required for the total destruction of the land must not be opposed to that view of future events which is indicated by the surest facts and most approved principles. Time, which measures everything in our idea, and is often deficient to our schemes, is to nature endless and as nothing; it cannot limit that by which alone it has existence; and as the natural course of time, which to us seems infinite, cannot be bounded by any operation that may have an end, the progress of things upon this globe that in the course of nature cannot be limited by time must proceed in a continual succession. We are, therefore, to consider as inevitable the destruction

of our land, so far as effected by those operations which are necessary in the purpose of the globe, considered as a habitable world, and so far as we have not examined any other part of the economy of nature, in which other operations and a different intention might appear.

"We have now considered the globe of this earth as a machine, constructed upon chemical as well as mechanical principles, by which its different parts are all adapted, in form, in quality, and quantity, to a certain end--an end attained with certainty of success, and an end from which we may perceive wisdom in contemplating the means employed.

"But is this world to be considered thus merely as a machine, to last no longer than its parts retain their present position, their proper forms and qualities? Or may it not be also considered as an organized body such as has a constitution, in which the necessary decay of the machine is naturally repaired in the exertion of those productive powers by which it has been formed?

"This is the view in which we are now to examine the globe; to see if there be, in the constitution of the world, a reproductive operation by which a ruined constitution may be again repaired and a duration of stability thus procured to the machine considered as a world containing plants and animals.

"If no such reproductive power, or reforming operation, after due inquiry, is to be found in the constitution of this world, we should have reason to conclude that the system of this earth has either been intentionally made imperfect or has not been the work of infinite power and wisdom."[1]

This, then, was the important question to be answered--the question of the constitution of the globe. To accomplish this, it was necessary, first of all, to examine without prejudice the material already in hand, adding such new discoveries from time to time as might be made, but always applying to the whole unvarying scientific principles and inductive methods of reasoning.

"If we are to take the written history of man for the rule by which we should judge of the time when the species first began," said Hutton, "that period would be but little removed from the present state of things. The Mosaic history places this beginning of man at no great distance; and there has not been found, in natural history, any document by which high antiquity might be attributed to the human race. But this is not the case with regard to the inferior species of animals, particularly those which inhabit the ocean and its shores. We find in natural history monuments which prove that those animals had long existed; and we thus procure a measure for the computation of a period of time extremely remote, though far from being precisely ascertained.

"In examining things present, we have data from which to reason with regard to what has been; and from what actually has been we have data for concluding with regard to that which is to happen hereafter. Therefore, upon the supposition that the operations of nature are equable and steady, we find, in natural appearances, means for concluding a certain portion of time to have necessarily elapsed in the production of those events of which we see the effects.

"It is thus that, in finding the relics of sea animals of every kind in the solid body of our earth, a natural history of those animals is formed, which includes a certain portion of time; and for the ascertaining this portion of time we must again have recourse to the regular operations of this

world. We shall thus arrive at facts which indicate a period to which no other species of chronology is able to remount.

"We find the marks of marine animals in the most solid parts of the earth, consequently those solid parts have been formed after the ocean was inhabited by those animals which are proper to that fluid medium. If, therefore, we knew the natural history of these solid parts, and could trace the operations of the globe by which they have been formed, we would have some means for computing the time through which those species of animals have continued to live. But how shall we describe a process which nobody has seen performed and of which no written history gives any account? This is only to be investigated, first, in examining the nature of those solid bodies the history of which we want to know; and, secondly, in examining the natural operations of the globe, in order to see if there now exist such operations as, from the nature of the solid bodies, appear to have been necessary for their formation.

"There are few beds of marble or limestone in which may not be found some of those objects which indicate the marine object of the mass. If, for example, in a mass of marble taken from a quarry upon the top of the Alps or Andes there shall be found one cockle-shell or piece of coral, it must be concluded that this bed of stone has been originally formed at the bottom of the sea, as much as another bed which is evidently composed almost altogether of cockle-shells and coral. If one bed of limestone is thus found to have been of marine origin, every concomitant bed of the same kind must be also concluded to have been formed in the same manner.

"In those calcareous strata, which are evidently of marine origin, there are many parts which are of sparry structure--that is to say, the original texture of those beds in such places has been dissolved, and a new structure has been assumed which is peculiar to a certain state of the calcareous earth. This change is produced by crystallization, in consequence of a previous state of fluidity, which has so disposed the concerting parts as to allow them to assume a regular shape and structure proper to that substance. A body whose external form has been modified by this process is called a CRYSTAL; one whose internal arrangement of parts is determined by it is said to be of a SPARRY STRUCTURE, and this is known from its fracture.

"There are, in all the regions of the earth, huge masses of calcareous matter in that crystalline form or sparry state in which, perhaps, no vestige can be found of any organized body, nor any indication that such calcareous matter has belonged to animals; but as in other masses this sparry structure or crystalline state is evidently assumed by the marine calcareous substances in operations which are natural to the globe, and which are necessary to the consolidation of the strata, it does not appear that the sparry masses in which no figured body is formed have been originally different from other masses, which, being only crystallized in part, and in part still retaining their original form, have ample evidence of their marine origin.

"We are led, in this manner, to conclude that all the strata of the earth, not only those consisting of such calcareous masses, but others superincumbent upon these, have had their origin at the bottom of the sea.

"The general amount of our reasoning is this, that nine-tenths, perhaps, or ninety-nine-hundredths, of this earth, so far as we see, have been formed by natural operations of the globe in collecting

loose materials and depositing them at the bottom of the sea; consolidating those collections in various degrees, and either elevating those consolidated masses above the level on which they were formed or lowering the level of that sea.

"Let us now consider how far the other proposition of strata being elevated by the power of heat above the level of the sea may be confirmed from the examination of natural appearances. The strata formed at the bottom of the ocean are necessarily horizontal in their position, or nearly so, and continuous in their horizontal direction or extent. They may be changed and gradually assume the nature of each other, so far as concerns the materials of which they are formed, but there cannot be any sudden change, fracture, or displacement naturally in the body of a stratum. But if the strata are cemented by the heat of fusion, and erected with an expansive power acting below, we may expect to find every species of fracture, dislocation, and contortion in those bodies and every degree of departure from a horizontal towards a vertical position.

"The strata of the globe are actually found in every possible position: for from horizontal they are frequently found vertical; from continuous they are broken and separated in every possible direction; and from a plane they are bent and doubled. It is impossible that they could have originally been formed, by the known laws of nature, in their present state and position; and the power that has been necessarily required for their change has not been inferior to that which might have been required for their elevation from the place in which they have been formed."[2]

From all this, therefore, Hutton reached the conclusion that the elevation of the bodies of land above the water on the earth's surface had been effected by the same force which had acted in consolidating the strata and giving them stability. This force he conceived to be exerted by the expansion of heated matter.

"We have," he said, "been now supposing that the beginning of our present earth had been laid in the bottom of the ocean, at the completion of the former land, but this was only for the sake of distinctness. The just view is this, that when the former land of the globe had been complete, so as to begin to waste and be impaired by the encroachment of the sea, the present land began to appear above the surface of the ocean. In this manner we suppose a due proportion to be always preserved of land and water upon the surface of the globe, for the purpose of a habitable world such as this which we possess. We thus also allow time and opportunity for the translation of animals and plants to occupy the earth.

"But if the earth on which we live began to appear in the ocean at the time when the LAST began to be resolved, it could not be from the materials of the continent immediately preceding this which we examine that the present earth has been constructed; for the bottom of the ocean must have been filled with materials before land could be made to appear above its surface.

"Let us suppose that the continent which is to succeed our land is at present beginning to appear above the water in the middle of the Pacific Ocean; it must be evident that the materials of this great body, which is formed and ready to be brought forth, must have been collected from the destruction of an earth which does not now appear. Consequently, in this true statement of the case there is necessarily required the destruction of an animal and vegetable earth prior to the former land; and the materials of that earth which is first in our account must have been collected at the

bottom of the ocean, and begun to be concocted for the production of the present earth, when the land immediately preceding the present had arrived at its full extent.

"We have now got to the end of our reasoning; we have no data further to conclude immediately from that which actually is; but we have got enough; we have the satisfaction to find that in nature there are wisdom, system, and consistency. For having in the natural history of the earth seen a succession of worlds, we may from this conclude that there is a system in nature; in like manner as, from seeing revolutions of the planets, it is concluded that there is a system by which they are intended to continue those revolutions. But if the succession of worlds is established in the system of nature, it is in vain to look for anything higher in the origin of the earth. The result, therefore, of our present inquiry is that we find no vestige of a beginning--no prospect of an end."

Altogether remarkable as this paper seems in the light of later knowledge, neither friend nor foe deigned to notice it at the moment. It was not published in book form until the last decade of the century, when Hutton had lived with and worked over his theory for almost fifty years. Then it caught the eye of the world. A school of followers expounded the Huttonian doctrines; a rival school under Werner in Germany opposed some details of the hypothesis, and the educated world as a whole viewed the disputants askance. The very novelty of the new views forbade their immediate acceptance. Bitter attacks were made upon the "heresies," and that was meant to be a soberly tempered judgment which in 1800 pronounced Hutton's theories "not only hostile to sacred history, but equally hostile to the principles of probability, to the results of the ablest observations on the mineral kingdom, and to the dictates of rational philosophy." And all this because Hutton's theory presupposed the earth to have been in existence more than six thousand years.

Thus it appears that though the thoughts of men had widened, in those closing days of the eighteenth century, to include the stars, they had not as yet expanded to receive the most patent records that are written everywhere on the surface of the earth. Before Hutton's views could be accepted, his pivotal conception that time is long must be established by convincing proofs. The evidence was being gathered by William Smith, Cuvier, and other devotees of the budding science of paleontology in the last days of the century, but their labors were not brought to completion till a subsequent epoch.

NEPTUNISTS VERSUS PLUTONISTS

In the mean time, James Hutton's theory that continents wear away and are replaced by volcanic upheaval gained comparatively few adherents. Even the lucid Illustrations of the Huttonian Theory, which Playfair, the pupil and friend of the great Scotchman, published in 1802, did not at once prove convincing. The world had become enamoured of the rival theory of Hutton's famous contemporary, Werner of Saxony --the theory which taught that "in the beginning" all the solids of the earth's present crust were dissolved in the heated waters of a universal sea. Werner affirmed that all rocks, of whatever character, had been formed by precipitation from this sea as the waters cooled; that even veins have originated in this way; and that mountains are gigantic crystals, not upheaved masses. In a word, he practically ignored volcanic action, and denied in toto the theory of metamorphosis of rocks through the agency of heat.

The followers of Werner came to be known as Neptunists; the Huttonians as Plutonists. The history of geology during the first quarter of the nineteenth century is mainly a recital of the intemperate controversy between these opposing schools; though it should not be forgotten that, meantime, the members of the Geological Society of London were making an effort to hunt for facts and avoid compromising theories. Fact and theory, however, were too closely linked to be thus divorced.

The brunt of the controversy settled about the unstratified rocks--granites and their allies--which the Plutonists claimed as of igneous origin. This contention had the theoretical support of the nebular hypothesis, then gaining ground, which supposed the earth to be a cooling globe. The Plutonists laid great stress, too, on the observed fact that the temperature of the earth increases at a pretty constant ratio as descent towards its centre is made in mines. But in particular they appealed to the phenomena of volcanoes.

The evidence from this source was gathered and elaborated by Mr. G. Poulett Scrope, secretary of the Geological Society of England, who, in 1823, published a classical work on volcanoes in which he claimed that volcanic mountains, including some of the highest- known peaks, are merely accumulated masses of lava belched forth from a crevice in the earth's crust.

"Supposing the globe to have had any irregular shape when detached from the sun," said Scrope, "the vaporization of its surface, and, of course, of its projecting angles, together with its rotatory motion on its axis and the liquefaction of its outer envelope, would necessarily occasion its actual figure of an oblate spheroid. As the process of expansion proceeded in depth, the original granitic beds were first partially disaggregated, next disintegrated, and more or less liquefied, the crystals being merged in the elastic vehicle produced by the vaporization of the water contained between the laminae.

"Where this fluid was produced in abundance by great dilatation--that is, in the outer and highly disintegrated strata, the superior specific gravity of the crystals forced it to ooze upward, and thus a great quantity of aqueous vapor was produced on the surface of the globe. As this elastic fluid rose into outer space, its continually increasing expansion must have proportionately lowered its temperature; and, in consequence, a part was recondensed into water and sank back towards the more solid surface of the globe.

"And in this manner, for a certain time, a violent reciprocation of atmospheric phenomena must have continued--torrents of vapor rising outwardly, while equally tremendous torrents of condensed vapor, or rain, fell towards the earth. The accumulation of the latter on the yet unstable and unconsolidated surface of the globe constituted the primeval ocean. The surface of this ocean was exposed to continued vaporization owing to intense heat; but this process, abstracting caloric from the stratum of the water below, by partially cooling it, tended to preserve the remainder in a liquid form. The ocean will have contained, both in solution and suspension, many of the matters carried upward from the granitic bed in which the vapors from whose condensation it proceeded were produced, and which they had traversed in their rise. The dissolved matters will have been silex, carbonates, and sulphates of lime, and those other mineral substances which water at an intense temperature and under such circumstances was enabled to hold in solution. The suspended substances will have been all the lighter and finer particles of the upper beds where the disintegration had been extreme; and particularly their mica, which, owing to the tenuity of its

plate-shaped crystals, would be most readily carried up by the ascending fluid, and will have remained longest in suspension.

"But as the torrents of vapor, holding these various matters in solution and suspension, were forced upward, the greater part of the disintegrated crystals by degrees subsided; those of felspar and quartz first, the mica being, as observed above, from the form of its plates, of peculiar buoyancy, and therefore held longest in suspension.

"The crystals of felspar and quartz as they subsided, together with a small proportion of mica, would naturally arrange themselves so as to have their longest dimensions more or less parallel to the surface on which they rest; and this parallelism would be subsequently increased, as we shall see hereafter, by the pressure of these beds sustained between the weight of the supported column of matter and the expansive force beneath them. These beds I conceive, when consolidated, to constitute the gneiss formation.

"The farther the process of expansion proceeded in depth, the more was the column of liquid matter lengthened, which, gravitating towards the centre of the globe, tended to check any further expansion. It is, therefore, obvious that after the globe settled into its actual orbit, and thenceforward lost little of its enveloping matter, the whole of which began from that moment to gravitate towards its centre, the progress of expansion inwardly would continually increase in rapidity; and a moment must have at length arrived hen the forces of expansion and repression had reached an equilibrium and the process was stopped from progressing farther inwardly by the great pressure of the gravitating column of liquid.

This column may be considered as consisting of different strata, though the passage from one extremity of complete solidity to the other of complete expansion, in reality, must have been perfectly gradual. The lowest stratum, immediately above the extreme limit of expansion, will have been granite barely DISAGGREGATED, and rendered imperfectly liquid by the partial vaporization of its contained water.

"The second stratum was granite DISINTEGRATED; aqueous vapor, having been produced in such abundance as to be enabled to rise upward, partially disintegrating the crystals of felspar and mica, and superficially dissolving those of quartz. This mass would reconsolidate into granite, though of a smaller grain than the preceding rock.

"The third stratum was so disintegrated that a greater part of the mica had been carried up by the escaping vapor IN SUSPENSION, and that of quartz in solution; the felspar crystals, with the remaining quartz and mica, SUBSIDING by their specific gravity and arranging themselves in horizontal planes.

"The consolidation of this stratum produced the gneiss formation.

"The fourth zone will have been composed of the ocean of turbid and heated water, holding mica, etc., in suspension, and quartz, carbonate of lime, etc., in solution, and continually traversed by reciprocating bodies of heated water rising from below, and of cold fluid sinking from the surface, by reason of their specific gravities.

"The disturbance thus occasioned will have long retarded the deposition of the suspended particles. But this must by degrees have taken place, the quartz grains and the larger and coarser plates of mica subsiding first and the finest last.

"But the fragments of quartz and mica were not deposited alone; a great proportion of the quartz held in SOLUTION must have been precipitated at the same time as the water cooled, and therefore by degrees lost its faculty of so much in solution. Thus was gradually produced the formation of mica-schist, the mica imperfectly recrystallizing or being merely aggregated together in horizontal plates, between which the quartz either spread itself generally in minute grains or unified into crystalline nuclei. On other spots, instead of silex, carbonate of lime was precipitated, together with more or less of the nucaceous sediment, and gave rise to saccharoidal limestones. At a later period, when the ocean was yet further cooled down, rock-salt and sulphate of lime were locally precipitated in a similar mode.

"The fifth stratum was aeriform, and consisted in great part of aqueous vapors; the remainder being a compound of other elastic fluids (permanent gases) which had been formed probably from the volatilization of some of the substances contained in the primitive granite and carried upward with the aqueous vapor from below. These gases will have been either mixed together or otherwise disposed, according to their different specific gravities or chemical affinities, and this stratum constituted the atmosphere or aerial envelope of the globe.

"When, in this manner, the general and positive expansion of the globe, occasioned by the sudden reduction of outward pressure, had ceased (in consequence of the REPRESSIVE FORCE, consisting of the weight of its fluid envelope, having reached an equilibrium with the EXPANSIVE FORCE, consisting of the caloric of the heated nucleus), the rapid superficial evaporation of the ocean continued; and, by gradually reducing its temperature, occasioned the precipitation of a proportionate quantity of the minerals it held in solution, particularly its silex. These substances falling to the bottom, accompanied by a large proportion of the matters held in solution, particularly the mica, in consequence of the greater comparative tranquillity of the ocean, agglomerated these into more or less compact beds of rock (the mica-schist formation), producing the first crust or solid envelope of the globe. Upon this, other stratified rocks, composed sometimes of a mixture, sometimes of an alternation of precipitations, sediments, and occasionally of conglomerates, were by degrees deposited, giving rise to the TRANSITION formations.

"Beneath this crust a new process now commenced. The outer zones of crystalline matter having been suddenly refrigerated by the rapid vaporization and partial escape of the water they contained, abstracted caloric from the intensely heated nucleus of the globe. These crystalline zones were of unequal density, the expansion they had suffered diminishing from above downward.

"Their expansive force was, however, equal at all points, their temperature everywhere bearing an inverse ratio to their density. But when by the accession of caloric from the inner and unliquefied nucleus the temperature, and consequently the expansive force of the lower strata of dilated crystalline matter, was augmented, it acted upon the upper and more liquefied strata. These being prevented from yielding OUTWARDLY by the tenacity and weight of the solid involucrum of precipitated and sedimental deposits which overspread them, sustained a pressure out of proportion

to their expansive force, and were in consequence proportionately condensed, and by the continuance of the process, where the overlying strata were sufficiently resistant, finally consolidated.

"This process of consolidation must have progressed from above downward, with the increase of the expansive force in the lower strata, commencing from the upper surface, which, its temperature being lowest, offered the least resistance to the force of compression.

"By this process the upper zone of crystalline matter, which had intumesced so far as to allow of the escape of its aqueous vapor and of much of its mica and quartz, was resolidified, the component crystals arranging themselves in planes perpendicular to the direction of the pressure by which the mass was consolidated--that is, to the radius of the globe. The gneiss formation, as already observed, was the result.

"The inferior zone of barely disintegrated granite, from which only a part of the steam and quartz and none of the mica had escaped, reconsolidated in a confused or granitoidal manner; but exhibits marks of the process it had undergone in its broken crystals of felspar and mica, its rounded and superficially dissolved grains of quartz, its imbedded fragments (broken from the more solid parts of the mass, as it rose, and enveloped by the softer parts), its concretionary nodules and new minerals, etc.

"Beneath this, the granite which had been simply disintegrated was again solidified, and returned in all respects to its former condition. The temperature, however, and with it the expansive force of the inferior zone, was continually on the increase, the caloric of the interior of the globe still endeavoring to put itself in equilibrio by passing off towards the less-intensely heated crust.

"This continually increasing expansive force must at length have overcome the resistance opposed by the tenacity and weight of the overlying consolidated strata. It is reasonable to suppose that this result took place contemporaneously, or nearly so, on many spots, wherever accidental circumstances in the texture or composition of the oceanic deposits led them to yield more readily; and in this manner were produced those original fissures in the primeval crust of the earth through some of which (fissures of elevation) were intruded portions of interior crystalline zones in a solid or nearly solid state, together with more or less of the intumescent granite, in the manner above described; while others (fissures of eruption) gave rise to extravasations of the heated crystalline matter, in the form of lavas--that is, still further liquefied by the greater comparative reduction of the pressure they endured."[3]

The Neptunists stoutly contended for the aqueous origin of volcanic as of other mountains. But the facts were with Scrope, and as time went on it came to be admitted that not merely volcanoes, but many "trap" formations not taking the form of craters, had been made by the obtrusion of molten rock through fissures in overlying strata. Such, for example, to cite familiar illustrations, are Mount Holyoke, in Massachusetts, and the well-known formation of the Palisades along the Hudson.

But to admit the "Plutonic" origin of such widespread formations was practically to abandon the Neptunian hypothesis. So gradually the Huttonian explanation of the origin of granites and other "igneous" rocks, whether massed or in veins, came to be accepted. Most geologists then came to

think of the earth as a molten mass, on which the crust rests as a mere film. Some, indeed, with Lyell, preferred to believe that the molten areas exist only as lakes in a solid crust, heated to melting, perhaps, by electrical or chemical action, as Davy suggested. More recently a popular theory attempts to reconcile geological facts with the claim of the physicists, that the earth's entire mass is at least as rigid as steel, by supposing that a molten film rests between the observed solid crust and the alleged solid nucleus. But be that as it may, the theory that subterranean heat has been instrumental in determining the condition of "primary" rocks, and in producing many other phenomena of the earth's crust, has never been in dispute since the long controversy between the Neptunists and the Plutonists led to its establishment.

LYELL AND UNIFORMITARIANISM

If molten matter exists beneath the crust of the earth, it must contract in cooling, and in so doing it must disturb the level of the portion of the crust already solidified. So a plausible explanation of the upheaval of continents and mountains was supplied by the Plutonian theory, as Hutton had from the first alleged. But now an important difference of opinion arose as to the exact rationale of such upheavals. Hutton himself, and practically every one else who accepted his theory, had supposed that there are long periods of relative repose, during which the level of the crust is undisturbed, followed by short periods of active stress, when continents are thrown up with volcanic suddenness, as by the throes of a gigantic earthquake. But now came Charles Lyell with his famous extension of the "uniformitarian" doctrine, claiming that past changes of the earth's surface have been like present changes in degree as well as in kind. The making of continents and mountains, he said, is going on as rapidly to-day as at any time in the past. There have been no gigantic cataclysmic upheavals at any time, but all changes in level of the strata as a whole have been gradual, by slow oscillation, or at most by repeated earthquake shocks such as are still often experienced.

In support of this very startling contention Lyell gathered a mass of evidence of the recent changes in level of continental areas. He corroborated by personal inspection the claim which had been made by Playfair in 1802, and by Von Buch in 1807, that the coast-line of Sweden is rising at the rate of from a few inches to several feet in a century. He cited Darwin's observations going to prove that Patagonia is similarly rising, and Pingel's claim that Greenland is slowly sinking. Proof as to sudden changes of level of several feet, over large areas, due to earthquakes, was brought forward in abundance. Cumulative evidence left it no longer open to question that such oscillatory changes of level, either upward or downward, are quite the rule, and it could not be denied that these observed changes, if continued long enough in one direction, would produce the highest elevations. The possibility that the making of even the highest ranges of mountains had been accomplished without exaggerated catastrophic action came to be freely admitted.

It became clear that the supposedly stable-land surfaces are in reality much more variable than the surface of the "shifting sea"; that continental masses, seemingly so fixed, are really rising and falling in billows thousands of feet in height, ages instead of moments being consumed in the sweep between crest and hollow.

These slow oscillations of land surfaces being understood, many geological enigmas were made clear-- such as the alternation of marine and fresh-water formations in a vertical series, which Cuvier and Brongniart had observed near Paris; or the sandwiching of layers of coal, of subaerial

formation, between layers of subaqueous clay or sandstone, which may be observed everywhere in the coal measures. In particular, the extreme thickness of the sedimentary strata as a whole, many times exceeding the depth of the deepest known sea, was for the first time explicable when it was understood that such strata had formed in slowly sinking ocean-beds.

All doubt as to the mode of origin of stratified rocks being thus removed, the way was opened for a more favorable consideration of that other Huttonian doctrine of the extremely slow denudation of land surfaces. The enormous amount of land erosion will be patent to any one who uses his eyes intelligently in a mountain district. It will be evident in any region where the strata are tilted--as, for example, the Alleghanies-- that great folds of strata which must once have risen miles in height have in many cases been worn entirely away, so that now a valley marks the location of the former eminence. Where the strata are level, as in the case of the mountains of Sicily, the Scotch Highlands, and the familiar Catskills, the evidence of denudation is, if possible, even more marked; for here it is clear that elevation and valley have been carved by the elements out of land that rose from the sea as level plateaus.

But that this herculean labor of land-sculpturing could have been accomplished by the slow action of wind and frost and shower was an idea few men could grasp within the first half-century after Hutton propounded it; nor did it begin to gain general currency until Lyell's crusade against catastrophism, begun about 1830, had for a quarter of a century accustomed geologists to the thought of slow, continuous changes producing final results of colossal proportions. And even long after that it was combated by such men as Murchison, Director-General of the Geological Survey of Great Britain, then accounted the foremost field-geologist of his time, who continued to believe that the existing valleys owe their main features to subterranean forces of upheaval. Even Murchison, however, made some recession from the belief of the Continental authorities, Elie de Beaumont and Leopold von Buch, who contended that the mountains had sprung up like veritable jacks-in-the-box. Von Buch, whom his friend and fellow-pupil Von Humboldt considered the foremost geologist of the time, died in 1853, still firm in his early faith that the erratic bowlders found high on the Jura had been hurled there, like cannon-balls, across the valley of Geneva by the sudden upheaval of a neighboring mountain-range.

AGASSIZ AND THE GLACIAL THEORY

The bowlders whose presence on the crags of the Jura the old Gerinan accounted for in a manner so theatrical had long been a source of contention among geologists. They are found not merely on the Jura, but on numberless other mountains in all north-temperate latitudes, and often far out in the open country, as many a farmer who has broken his plough against them might testify. The early geologists accounted for them, as for nearly everything else, with their supposititious Deluge. Brongniart and Cuvier and Buckland and their contemporaries appeared to have no difficulty in conceiving that masses of granite weighing hundreds of tons had been swept by this current scores or hundreds of miles from their source. But, of course, the uniformitarian faith permitted no such explanation, nor could it countenance the projection idea; so Lyell was bound to find some other means of transportation for the puzzling erratics.

The only available medium was ice, but, fortunately, this one seemed quite sufficient. Icebergs, said Lyell, are observed to carry all manner of debris, and deposit it in the sea-bottoms. Present

land surfaces have often been submerged beneath the sea. During the latest of these submergences icebergs deposited the bowlders now scattered here and there over the land. Nothing could be simpler or more clearly uniformitarian. And even the catastrophists, though they met Lyell amicably on almost no other theoretical ground, were inclined to admit the plausibility of his theory of erratics. Indeed, of all Lyell's nonconformist doctrines, this seemed the one most likely to meet with general acceptance.

Yet, even as this iceberg theory loomed large and larger before the geological world, observations were making in a different field that were destined to show its fallacy. As early as 1815 a sharp-eyed chamois- hunter of the Alps, Perraudin by name, had noted the existence of the erratics, and, unlike most of his companion hunters, had puzzled his head as to how the bowlders got where he saw them. He knew nothing of submerged continents or of icebergs, still less of upheaving mountains; and though he doubtless had heard of the Flood, he had no experience of heavy rocks floating like corks in water. Moreover, he had never observed stones rolling uphill and perching themselves on mountain-tops, and he was a good enough uniformitarian (though he would have been puzzled indeed had any one told him so) to disbelieve that stones in past times had disported themselves differently in this regard from stones of the present. Yet there the stones are. How did they get there?

The mountaineer thought that he could answer that question. He saw about him those gigantic serpent- like streams of ice called glaciers, "from their far fountains slow rolling on," carrying with them blocks of granite and other debris to form moraine deposits. If these glaciers had once been much more extensive than they now are, they might have carried the bowlders and left them where we find them. On the other hand, no other natural agency within the sphere of the chamois-hunter's knowledge could have accomplished this, ergo the glaciers must once have been more extensive. Perraudin would probably have said that common-sense drove him to this conclusion; but be that as it may, he had conceived one of the few truly original and novel ideas of which the nineteenth century can boast.

Perraudin announced his idea to the greatest scientist in his little world--Jean de Charpentier, director of the mines at Bex, a skilled geologist who had been a fellow-pupil of Von Buch and Von Humboldt under Werner at the Freiberg School of Mines. Charpentier laughed at the mountaineer's grotesque idea, and thought no more about it. And ten years elapsed before Perraudin could find any one who treated his notion with greater respect. Then he found a listener in M. Venetz, a civil engineer, who read a paper on the novel glacial theory before a local society in 1823. This brought the matter once more to the attention of De Charpentier, who now felt that there might be something in it worth investigation.

A survey of the field in the light of the new theory soon convinced Charpentier that the chamois-hunter had all along been right. He became an enthusiastic supporter of the idea that the Alps had once been imbedded in a mass of ice, and in 1836 he brought the notion to the attention of Louis Agassiz, who was spending the summer in the Alps. Agassiz was sceptical at first, but soon became a convert.

In 1840 Agassiz published a paper in which the results of his Alpine studies were elaborated.

"Let us consider," he says, "those more considerable changes to which glaciers are subject, or rather, the immense extent which they had in the prehistoric period. This former immense extension, greater than any that tradition has preserved, is proved, in the case of nearly every valley in the Alps, by facts which are both many and well established. The study of these facts is even easy if the student is looking out for them, and if he will seize the least indication of their presence; and, if it were a long time before they were observed and connected with glacial action, it is because the evidences are often isolated and occur at places more or less removed from the glacier which originated them. If it be true that it is the prerogative of the scientific observer to group in the field of his mental vision those facts which appear to be without connection to the vulgar herd, it is, above all, in such a case as this that he is called upon to do so. I have often compared these feeble effects, produced by the glacial action of former ages, with the appearance of the markings upon a lithographic stone, prepared for the purpose of preservation, and upon which one cannot see the lines of the draughtsman's work unless it is known beforehand where and how to search for them.

"The fact of the former existence of glaciers which have now disappeared is proved by the survival of the various phenomena which always accompany them, and which continue to exist even after the ice has melted. These phenomena are as follows:

"1. Moraines.--The disposition and composition of moraines enable them to be always recognized, even when they are no longer adjacent to a glacier nor immediately surround its lower extremities. I may remark that lateral and terminal moraines alone enable us to recognize with certainty the limits of glacial extension, because they can be easily distinguished from the dikes and irregularly distributed stones carried down by the Alpine torrents, The lateral moraines deposited upon the sides of valleys are rarely affected by the larger torrents, but they are, however, often cut by the small streams which fall down the side of a mountain, and which, by interfering with their continuity, make them so much more difficult to recognize.

"2. The Perched Bowlders.--It often happens that glaciers encounter projecting points of rock, the sides of which become rounded, and around which funnel- like cavities are formed with more or less profundity. When glaciers diminish and retire, the blocks which have fallen into these funnels often remain perched upon the top of the projecting rocky point within it, in such a state of equilibrium that any idea of a current of water as the cause of their transportation is completely inadmissible on account of their position. When such points of rock project above the surface of the glacier or appear as a more considerable islet in the midst of its mass (such as is the case in the Jardin of the Mer de Glace, above Montavert), such projections become surrounded on all sides by stones which ultimately form a sort of crown around the summit whenever the glaciers decrease or retire completely. Water currents never produce anything like this; but, on the contrary, whenever a stream breaks itself against a projecting rock, the stones which it carries down are turned aside and form a more or less regular trail. Never, under such circumstances, can the stones remain either at the top or at the sides of the rock, for, if such a thing were possible, the rapidity of the current would be accelerated by the increased resistance, and the moving bowlders would be carried beyond the obstruction before they were finally deposited.

"3. The polished and striated rocks, such as have been described in Chapter XIV., afford yet further evidence of the presence of a glacier; for, as has been said already, neither a current nor the action of waves upon an extensive beach produces such effects. The general direction of the channels and

furrows indicates the direction of the general movement of the glacier, and the streaks which vary more or less from this direction are produced by the local effects of oscillation and retreat, as we shall presently see.

"4. The Lapiaz, or Lapiz, which the inhabitants of German Switzerland call Karrenfelder, cannot always be distinguished from erosions, because, both produced as they are by water, they do not differ in their exterior characteristics, but only in their positions. Erosions due to torrents are always found in places more or less depressed, and never occur upon large inclined surfaces. The Lapiaz, on the contrary, are frequently found upon the projecting parts of the sides of valleys in places where it is not possible to suppose that water has ever formed a current. Some geologists, in their embarrassment to explain these phenomena, have supposed that they were due to the infiltration of acidulated water, but this hypothesis is purely gratuitous.

"We will now describe the remains of these various phenomena as they are found in the Alps outside the actual glacial limits, in order to prove that at a certain epoch glaciers were much larger than they are to-day.

"The ancient moraines, situated as they are at a great distance from those of the present day, are nowhere so distinct or so frequent as in Valais, where MM. Venetz and J. de Charpentier noticed them for the first time; but as their observations are as yet unpublished, and they themselves gave me the information, it would be an appropriation of their discovery if I were to describe them here in detail. I will limit myself to say that there can be found traces, more or less distinct, of ancient terminal moraines in the form of vaulted dikes at the foot of every glacier, at a distance of a few minutes' walk, a quarter of an hour, a half-hour, an hour, and even of several leagues from their present extremities. These traces become less distinct in proportion to their distance from the glacier, and, since they are also often traversed by torrents, they are not as continuous as the moraines which are nearer to the glaciers. The farther these ancient moraines are removed from the termination of a glacier, the higher up they reach upon the sides of the valley, which proves to us that the thickness of the glacier must have been greater when its size was larger. At the same time, their number indicates so many stopping-places in the retreat of the glacier, or so many extreme limits of its extension--limits which were never reached again after it had retired. I insist upon this point, because if it is true that all these moraines demonstrate a larger extent of the glacier, they also prove that their retreat into their present boundaries, far from having been catastrophic, was marked on the contrary by periods of repose more or less frequent, which caused the formation of a series of concentric moraines which even now indicate their retrogression.

"The remains of longitudinal moraines are less frequent, less distinct, and more difficult to investigate, because, indicating as they do the levels to which the edges of the glacier reached at different epochs, it is generally necessary to look for them above the line of the paths along the escarpments of the valleys, and hence it is not always possible to follow them along a valley. Often, also, the sides of a valley which enclosed a glacier are so steep that it is only here and there that the stones have remained in place. They are, nevertheless, very distinct in the lower part of the valley of the Rhone, between Martigny and the Lake of Geneva, where several parallel ridges can be observed, one above the other, at a height of one thousand, one thousand two hundred, and even one thousand five hundred feet above the Rhone. It is between St. Maurice and the cascade of Pissevache, close to the hamlet of Chaux-Fleurie, that they are most accessible, for at this place the

sides of the valley at different levels ascend in little terraces, upon which the moraines have been preserved. They are also very distinct above the Bains de Lavey, and above the village of Monthey at the entrance of the Val d'Illiers, where the sides of the valley are less inclined than in many other places.

"The perched bowlders which are found in the Alpine valleys, at considerable distances from the glaciers, occupy at times positions so extraordinary that they excite in a high degree the curiosity of those who see them. For instance, when one sees an angular stone perched upon the top of an isolated pyramid, or resting in some way in a very steep locality, the first inquiry of the mind is, When and how have these stones been placed in such positions, where the least shock would seem to turn them over? But this phenomenon is not in the least astonishing when it is seen to occur also within the limits of actual glaciers, and it is recalled by what circumstances it is occasioned.

"The most curious examples of perched stones which can be cited are those which command the northern part of the cascade of Pissevache, close to Chaux-Fleurie, and those above the Bains de Lavey, close to the village of Morcles; and those, even more curious, which I have seen in the valley of St. Nicolas and Oberhasli. At Kirchet, near Meiringen, can be seen some very remarkable crowns of bowlders around several domes of rock which appear to have been projected above the surface of the glacier which surrounded them. Something very similar can be seen around the top of the rock of St. Triphon.

"The extraordinary phenomenon of perched stones could not escape the observing eye of De Saussure, who noticed several at Saleve, of which he described the positions in the following manner: 'One sees,' said he, 'upon the slope of an inclined meadow, two of these great bowlders of granite, elevated one upon the other, above the grass at a height of two or three feet, upon a base of limestone rock on which both rest. This base is a continuation of the horizontal strata of the mountain, and is even united with it visibly on its lower face, being cut perpendicularly upon the other sides, and is not larger than the stone which it supports.' But seeing that the entire mountain is composed of the same limestone, De Saussure naturally concluded that it would be absurd to think that it was elevated precisely and only beneath the blocks of granite. But, on the other hand, since he did not know the manner in which these perched stones are deposited in our days by glacial action, he had recourse to another explanation: He supposes that the rock was worn away around its base by the continual erosion of water and air, while the portion of the rock which served as the base for the granite had been protected by it. This explanation, although very ingenious, could no longer be admitted after the researches of M. Elie de Beaumont had proved that the action of atmospheric agencies was not by a good deal so destructive as was theretofore supposed. De Saussure speaks also of a detached bowlder, situated upon the opposite side of the Tete-Noire, 'which is,' he says, 'of so great a size that one is tempted to believe that it was formed in the place it occupies; and it is called Barme russe, because it is worn away beneath in the form of a cave which can afford accommodation for more than thirty persons at a time."[4]

But the implications of the theory of glaciers extend, so Agassiz has come to believe, far beyond the Alps. If the Alps had been covered with an ice sheet, so had many other regions of the northern hemisphere. Casting abroad for evidences of glacial action, Agassiz found them everywhere in the form of transported erratics, scratched and polished outcropping rocks, and moraine-like deposits. Finally, he became convinced that the ice sheet that covered the Alps had spread over the whole of

the higher latitudes of the northern hemisphere, forming an ice cap over the globe. Thus the common-sense induction of the chamois- hunter blossomed in the mind of Agassiz into the conception of a universal ice age.

In 1837 Agassiz had introduced his theory to the world, in a paper read at Neuchatel, and three years later he published his famous Etudes sur les Glaciers, from which we have just quoted. Never did idea make a more profound disturbance in the scientific world. Von Buch treated it with alternate ridicule, contempt, and rage; Murchison opposed it with customary vigor; even Lyell, whose most remarkable mental endowment was an unfailing receptiveness to new truths, could not at once discard his iceberg theory in favor of the new claimant. Dr. Buckland, however, after Agassiz had shown him evidence of former glacial action in his own Scotland, became a convert--the more readily, perhaps, as it seemed to him to oppose the uniformitarian idea. Gradually others fell in line, and after the usual imbittered controversy and the inevitable full generation of probation, the idea of an ice age took its place among the accepted tenets of geology. All manner of moot points still demanded attention--the cause of the ice age, the exact extent of the ice sheet, the precise manner in which it produced its effects, and the exact nature of these effects; and not all of these have even yet been determined. But, details aside, the ice age now has full recognition from geologists as an historical period. There may have been many ice ages, as Dr. Croll contends; there was surely one; and the conception of such a period is one of the very few ideas of our century that no previous century had even so much as faintly adumbrated.

THE GEOLOGICAL AGES

But, for that matter, the entire subject of historical geology is one that had but the barest beginning before our century. Until the paleontologist found out the key to the earth's chronology, no one--not even Hutton-- could have any definite idea as to the true story of the earth's past. The only conspicuous attempt to classify the strata was that made by Werner, who divided the rocks into three systems, based on their supposed order of deposition, and called primary, transition, and secondary.

Though Werner's observations were confined to the small province of Saxony, he did not hesitate to affirm that all over the world the succession of strata would be found the same as there, the concentric layers, according to this conception, being arranged about the earth with the regularity of layers on an onion. But in this Werner was as mistaken as in his theoretical explanation of the origin of the "primary" rocks. It required but little observation to show that the exact succession of strata is never precisely the same in any widely separated regions. Nevertheless, there was a germ of truth in Werner's system. It contained the idea, however faultily interpreted, of a chronological succession of strata; and it furnished a working outline for the observers who were to make out the true story of geological development. But the correct interpretation of the observed facts could only be made after the Huttonian view as to the origin of strata had gained complete acceptance.

When William Smith, having found the true key to this story, attempted to apply it, the territory with which he had to deal chanced to be one where the surface rocks are of that later series which Werner termed secondary. He made numerous subdivisions within this system, based mainly on the fossils. Meantime it was found that, judged by the fossils, the strata that Brongniart and Cuvier studied near Paris were of a still more recent period (presumed at first to be due to the latest

deluge), which came to be spoken of as tertiary. It was in these beds, some of which seemed to have been formed in fresh-water lakes, that many of the strange mammals which Cuvier first described were found.

But the "transition" rocks, underlying the "secondary" system that Smith studied, were still practically unexplored when, along in the thirties, they were taken in hand by Roderick Impey Murchison, the reformed fox-hunter and ex-captain, who had turned geologist to such notable advantage, and Adam Sedgwick, the brilliant Woodwardian professor at Cambridge.

Working together, these two friends classified the

transition rocks into chronological groups, since familiar to every one in the larger outlines as the Silurian system (age of invertebrates) and the Devonian system (age of fishes)--names derived respectively from the country of the ancient Silures, in Wales and Devonshire, England. It was subsequently discovered that these systems of strata, which crop out from beneath newer rocks in restricted areas in Britain, are spread out into broad, undisturbed sheets over thousands of miles in continental Europe and in America. Later on Murchison studied them in Russia, and described them, conjointly with Verneuil and Von Kerserling, in a ponderous and classical work. In America they were studied by Hall, Newberry, Whitney, Dana, Whitfield, and other pioneer geologists, who all but anticipated their English contemporaries.

The rocks that are of still older formation than those studied by Murchison and Sedgwick (corresponding in location to the "primary" rocks of Werner's conception) are the surface feature of vast areas in Canada, and were first prominently studied there by William I. Logan, of the Canadian Government Survey, as early as 1846, and later on by Sir William Dawson. These rocks --comprising the Laurentian system--were formerly supposed to represent parts of the original crust of the earth, formed on first cooling from a molten state; but they are now more generally regarded as once-stratified deposits metamorphosed by the action of heat.

Whether "primitive" or metamorphic, however, these Canadian rocks, and analogous ones beneath the fossiliferous strata of other countries, are the oldest portions of the earth's crust of which geology has any present knowledge. Mountains of this formation, as the Adirondacks and the Storm King range, overlooking the Hudson near West Point, are the patriarchs of their kind, beside which Alleghanies and Sierra Nevadas are recent upstarts, and Rockies, Alps, and Andes are mere parvenus of yesterday.

The Laurentian rocks were at first spoken of as representing "Azoic" time; but in 1846 Dawson found a formation deep in their midst which was believed to b e the fossil relic of a very low form of life, and after that it became customary to speak of the system as "Eozoic." Still more recently the title of Dawson's supposed fossil to rank as such has been questioned, and Dana's suggestion that the early rocks be termed merely Archman has met with general favor. Murchison and Sedgwick's Silurian, Devonian, and Carboniferous groups (the ages of invertebrates, of fishes, and of coal plants, respectively) are together spoken of as representing Paleozoic time. William Smith's system of strata, next above these, once called "secondary," represents Mesozoic time, or the age of reptiles. Still higher, or more recent, are Cuvier and Brongniart's tertiary rocks, representing the age

of mammals. Lastly, the most recent formations, dating back, however, to a period far enough from recent in any but a geological sense, are classed as quaternary, representing the age of man.

It must not be supposed, however, that the successive "ages" of the geologist are shut off from one another in any such arbitrary way as this verbal classification might seem to suggest. In point of fact, these "ages" have no better warrant for existence than have the "centuries" and the "weeks" of every-day computation. They are convenient, and they may even stand for local divisions in the strata, but they are bounded by no actual gaps in the sweep of terrestrial events.

Moreover, it must be understood that the "ages" of different continents, though described under the same name, are not necessarily of exact contemporaneity. There is no sure test available by which it could be shown that the Devonian age, for instance, as outlined in the strata of Europe, did not begin millions of years earlier or later than the period whose records are said to represent the Devonian age in America. In attempting to decide such details as this, mineralogical data fail us utterly. Even in rocks of adjoining regions identity of structure is no proof of contemporaneous origin; for the veritable substance of the rock of one age is ground up to build the rocks of subsequent ages. Furthermore, in seas where conditions change but little the same form of rock may be made age after age. It is believed that chalk-beds still forming in some of our present seas may form one continuous mass dating back to earliest geologic ages. On the other hand, rocks different in character maybe formed at the same time in regions not far apart--say a sandstone along shore, a coral limestone farther seaward, and a chalk-bed beyond. This continuous stratum, broken in the process of upheaval, might seem the record of three different epochs.

Paleontology, of course, supplies far better chronological tests, but even these have their limitations. There has been no time since rocks now in existence were formed, if ever, when the earth had a uniform climate and a single undiversified fauna over its entire land surface, as the early paleontologists supposed. Speaking broadly, the same general stages have attended the evolution of organic forms everywhere, but there is nothing to show that equal periods of time witnessed corresponding changes in diverse regions, but quite the contrary. To cite but a single illustration, the marsupial order, which is the dominant mammalian type of the living fauna of Australia to-day, existed in Europe and died out there in the tertiary age. Hence a future geologist might think the Australia of to-day contemporaneous with a period in Europe which in reality antedated it by perhaps millions of years.

All these puzzling features unite to render the subject of historical geology anything but the simple matter the fathers of the science esteemed it. No one would now attempt to trace the exact sequence of formation of all the mountains of the globe, as Elie de Beaumont did a half-century ago. Even within the limits of a single continent, the geologist must proceed with much caution in attempting to chronicle the order in which its various parts rose from the matrix of the sea. The key to this story is found in the identification of the strata that are the surface feature in each territory. If Devonian rocks are at the surface in any given region, for example, it would appear that this region became a land surface in the Devonian age, or just afterwards. But a moment's consideration shows that there is an element of uncertainty about this, due to the steady denudation that all land surfaces undergo. The Devonian rocks may lie at the surface simply because the thousands of feet of carboniferous strata that once lay above them have been worn away. All that the cautious geologist

dare assert, therefore, is that the region in question did not become permanent land surface earlier than the Devonian age.

But to know even this is much--sufficient, indeed, to establish the chronological order of elevation, if not its exact period, for all parts of any continent that have been geologically explored--understanding always that there must be no scrupling about a latitude of a few millions or perhaps tens of millions of years here and there.

Regarding our own continent, for example, we learn through the researches of a multitude of workers that in the early day it was a mere archipelago. Its chief island--the backbone of the future continent--was a great V-shaped area surrounding what is now Hudson Bay, an area built tip, perhaps, through denudation of a yet more ancient polar continent, whose existence is only conjectured. To the southeast an island that is now the Adirondack Mountains, and another that is now the Jersey Highlands rose above the waste of waters, and far to the south stretched probably a line of islands now represented by the Blue Ridge Mountains. Far off to the westward another line of islands foreshadowed our present Pacific border. A few minor islands in the interior completed the archipelago.

From this bare skeleton the continent grew, partly by the deposit of sediment from the denudation of the original islands (which once towered miles, perhaps, where now they rise thousands of feet), but largely also by the deposit of organic remains, especially in the interior sea, which teemed with life. In the Silurian ages, invertebrates--brachiopods and crinoids and cephalopods--were the dominant types. But very early--no one knows just when--there came fishes of many strange forms, some of the early ones enclosed in turtle-like shells. Later yet, large spaces within the interior sea having risen to the surface, great marshes or forests of strange types of vegetation grew and deposited their remains to form coal-beds. Many times over such forests were formed, only to be destroyed by the oscillations of the land surface. All told, the strata of this Paleozoic period aggregate several miles in thickness, and the time consumed in their formation stands to all later time up to the present, according to Professor Dana's estimate, as three to one.

Towards the close of this Paleozoic era the Appalachian Mountains were slowly upheaved in great convoluted folds, some of them probably reaching three or four miles above the sea-level, though the tooth of time has since gnawed them down to comparatively puny limits. The continental areas thus enlarged were peopled during the ensuing Mesozoic time with multitudes of strange reptiles, many of them gigantic in size. The waters, too, still teeming with invertebrates and fishes, had their quota of reptilian monsters; and in the air were flying reptiles, some of which measured twenty-five feet from tip to tip of their batlike wings. During this era the Sierra Nevada Mountains rose. Near the eastern border of the forming continent the strata were perhaps now too thick and stiff to bend into mountain folds, for they were rent into great fissures, letting out floods of molten lava, remnants of which are still in evidence after ages of denudation, as the Palisades along the Hudson, and such elevations as Mount Holyoke in western Massachusetts.

Still there remained a vast interior sea, which later on, in the tertiary age, was to be divided by the slow uprising of the land, which only yesterday--that is to say, a million, or three or five or ten million, years ago-- became the Rocky Mountains. High and erect these young mountains stand to this day, their sharp angles and rocky contours vouching for their youth, in strange contrast with the

shrunken forms of the old Adirondacks, Green Mountains, and Appalachians, whose lowered heads and rounded shoulders attest the weight of ages. In the vast lakes which still remained on either side of the Rocky range, tertiary strata were slowly formed to the ultimate depth of two or three miles, enclosing here and there those vertebrate remains which were to be exposed again to view by denudation when the land rose still higher, and then, in our own time, to tell so wonderful a story to the paleontologist.

Finally, the interior seas were filled, and the shore lines of the continent assumed nearly their present outline.

Then came the long winter of the glacial epoch--perhaps of a succession of glacial epochs. The ice sheet extended southward to about the fortieth parallel, driving some animals before it, and destroying those that were unable to migrate. At its fulness, the great ice mass lay almost a mile in depth over New England, as attested by the scratched and polished rock surfaces and deposited erratics in the White Mountains. Such a mass presses down with a weight of about one hundred and twenty-five tons to the square foot, according to Dr. Croll's estimate. It crushed and ground everything beneath it more or less, and in some regions planed off hilly surfaces into prairies. Creeping slowly forward, it carried all manner of debris with it. When it melted away its terminal moraine built up the nucleus of the land masses now known as Long Island and Staten Island; other of its deposits formed the "drumlins" about Boston famous as Bunker and Breed's hills; and it left a long, irregular line of ridges of "till" or bowlder clay and scattered erratics clear across the country at about the latitude of New York city.

As the ice sheet slowly receded it left minor moraines all along its course. Sometimes its deposits dammed up river courses or inequalities in the surface, to form the lakes which everywhere abound over Northern territories. Some glacialists even hold the view first suggested by Ramsey, of the British Geological Survey, that the great glacial sheets scooped out the basins of many lakes, including the system that feeds the St. Lawrence. At all events, it left traces of its presence all along the line of its retreat, and its remnants exist to this day as mountain glaciers and the polar ice cap. Indeed, we live on the border of the last glacial epoch, for with the closing of this period the long geologic past merges into the present.

PAST, PRESENT, AND FUTURE

And the present, no less than the past, is a time of change. This is the thought which James Hutton conceived more than a century ago, but which his contemporaries and successors were so very slow to appreciate. Now, however, it has become axiomatic--one can hardly realize that it was ever doubted. Every new scientific truth, says Agassiz, must pass through three stages --first, men say it is not true; then they declare it hostile to religion; finally, they assert that every one has known it always. Hutton's truth that natural law is changeless and eternal has reached this final stage. Nowhere now could you find a scientist who would dispute the truth of that text which Lyell, quoting from Playfair's Illustrations of the Huttonian Theory, printed on the title-page of his Principles: "Amid all the revolutions of the globe the economy of Nature has been uniform, and her laws are the only things that have resisted the general movement. The rivers and the rocks, the seas and the continents, have been changed in all their parts; but the laws which direct those changes, and the rules to which they are subject, have remained invariably the same."

But, on the other hand, Hutton and Playfair, and in particular Lyell, drew inferences from this principle which the modern physicist can by no means admit. To them it implied that the changes on the surface of the earth have always been the same in degree as well as in kind, and must so continue while present forces hold their sway. In other words, they thought of the world as a great perpetual-motion machine. But the modern physicist, given truer mechanical insight by the doctrines of the conservation and the dissipation of energy, will have none of that. Lord Kelvin, in particular, has urged that in the periods of our earth's in fancy and adolescence its developmental changes must have been, like those of any other infant organism, vastly more rapid and pronounced than those of a later day; and to every clear thinker this truth also must now seem axiomatic.

Whoever thinks of the earth as a cooling globe can hardly doubt that its crust, when thinner, may have heaved under strain of the moon's tidal pull--whether or not that body was nearer--into great billows, daily rising and falling, like waves of the present seas vastly magnified.

Under stress of that same lateral pressure from contraction which now produces the slow depression of the Jersey coast, the slow rise of Sweden, the occasional belching of an insignificant volcano, the jetting of a geyser, or the trembling of an earthquake, once large areas were rent in twain, and vast floods of lava flowed over thousands of square miles of the earth's surface, perhaps, at a single jet; and, for aught we know to the contrary, gigantic mountains may have heaped up their contorted heads in cataclysms as spasmodic as even the most ardent catastrophist of the elder day of geology could have imagined.

The atmosphere of that early day, filled with vast volumes of carbon, oxygen, and other chemicals that have since been stored in beds of coal, limestone, and granites, may have worn down the rocks on the one hand and built up organic forms on the other, with a rapidity that would now seem hardly conceivable.

And yet while all these anomalous things went on, the same laws held sway that now are operative; and a true doctrine of uniformitarianism would make no unwonted concession in conceding them all--though most of the imbittered geological controversies of the middle of the nineteenth century were due to the failure of both parties to realize that simple fact.

And as of the past and present, so of the future. The same forces will continue to operate; and under operation of these unchanging forces each day will differ from every one that has preceded it. If it be true, as every physicist believes, that the earth is a cooling globe, then, whatever its present stage of refrigeration, the time must come when its surface contour will assume a rigidity of level not yet attained. Then, just as surely, the slow action of the elements will continue to wear away the land surfaces, particle by particle, and transport them to the ocean, as it does to-day, until, compensation no longer being afforded by the upheaval of the continents, the last foot of dry land will sink for the last time beneath the water, the last mountain- peak melting away, and our globe, lapsing like any other organism into its second childhood, will be on the surface--as presumably it was before the first continent rose--one vast "waste of waters." As puny man conceives time and things, an awful cycle will have lapsed; in the sweep of the cosmic life, a pulse- beat will have throbbed.

V. THE NEW SCIENCE OF METEOROLOGY

METEORITES

"An astonishing miracle has just occurred in our district," wrote M. Marais, a worthy if undistinguished citizen of France, from his home at L'Aigle, under date of "the 13th Floreal, year 11"--a date which outside of France would be interpreted as meaning May 3, 1803. This "miracle" was the appearance of a "fireball" in broad daylight--"perhaps it was wildfire," says the naive chronicle--which "hung over the meadow," being seen by many people, and then exploded with a loud sound, scattering thousands of stony fragments over the surface of a territory some miles in extent.

Such a "miracle" could not have been announced at a more opportune time. For some years the scientific world had been agog over the question whether such a form of lightning as that reported--appearing in a clear sky, and hurling literal thunderbolts--had real existence. Such cases had been reported often enough, it is true. The "thunderbolts" themselves were exhibited as sacred relics before many an altar, and those who doubted their authenticity had been chided as having "an evil heart of unbelief." But scientific scepticism had questioned the evidence, and late in the eighteenth century a consensus of opinion in the French Academy had declined to admit that such stones had been "conveyed to the earth by lightning," let alone any more miraculous agency.

In 1802, however, Edward Howard had read a paper before the Royal Society in which, after reviewing the evidence recently put forward, he had reached the conclusion that the fall of stones from the sky, sometimes or always accompanied by lightning, must be admitted as an actual phenomenon, however inexplicable. So now, when the great stone-fall at L'Aigle was announced, the French Academy made haste to send the brilliant young physicist Jean Baptiste Biot to investigate it, that the matter might, if possible, be set finally at rest. The investigation was in all respects successful, and Biot's report transferred the stony or metallic lightning-bolt--the aerolite or meteorite--from the realm of tradition and conjecture to that of accepted science.

But how explain this strange phenomenon? At once speculation was rife. One theory contended that the stony masses had not actually fallen, but had been formed from the earth by the action of the lightning; but this contention was early abandoned. The chemists were disposed to believe that the aerolites had been formed by the combination of elements floating in the upper atmosphere. Geologists, on the other hand, thought them of terrestrial origin, urging that they might have been thrown up by volcanoes. The astronomers, as represented by Olbers and Laplace, modified this theory by suggesting that the stones might, indeed, have been cast out by volcanoes, but by volcanoes situated not on the earth, but on the moon.

And one speculator of the time took a step even more daring, urging that the aerolites were neither of telluric nor selenitic origin, nor yet children of the sun, as the old Greeks had, many of them, contended, but that they are visitants from the depths of cosmic space. This bold speculator was the distinguished German physicist Ernst F. F. Chladni, a man of no small repute in his day. As early as 1794 he urged his cosmical theory of meteorites, when the very existence of meteorites was denied by most scientists. And he did more: he declared his belief that these falling stones were really one

in origin and kind with those flashing meteors of the upper atmosphere which are familiar everywhere as "shooting-stars."

Each of these coruscating meteors, he affirmed, must tell of the ignition of a bit of cosmic matter entering the earth's atmosphere. Such wandering bits of matter might be the fragments of shattered worlds, or, as Chladni thought more probable, merely aggregations of "world stuff" never hitherto connected with any large planetary mass.

Naturally enough, so unique a view met with very scant favor. Astronomers at that time saw little to justify it; and the non-scientific world rejected it with fervor as being "atheistic and heretical," because its acceptance would seem to imply that the universe is not a perfect mechanism.

Some light was thrown on the moot point presently by the observations of Brandes and Benzenberg, which tended to show that falling-stars travel at an actual speed of from fifteen to ninety miles a second. This observation tended to discredit the selenitic theory, since an object, in order to acquire such speed in falling merely from the moon, must have been projected with an initial velocity not conceivably to be given by any lunar volcanic impulse. Moreover, there was a growing conviction that there are no active volcanoes on the moon, and other considerations of the same tenor led to the complete abandonment of the selenitic theory.

But the theory of telluric origin of aerolites was by no means so easily disposed of. This was an epoch when electrical phenomena were exciting unbounded and universal interest, and there was a not unnatural tendency to appeal to electricity in explanation of every obscure phenomenon; and in this case the seeming similarity between a lightning flash and the flash of an aerolite lent color to the explanation. So we find Thomas Forster, a meteorologist of repute, still adhering to the atmospheric theory of formation of aerolites in his book published in 1823; and, indeed, the prevailing opinion of the time seemed divided between various telluric theories, to the neglect of any cosmical theory whatever.

But in 1833 occurred a phenomenon which set the matter finally at rest. A great meteoric shower occurred in November of that year, and in observing it Professor Denison Olmstead, of Yale, noted that all the stars of the shower appeared to come from a single centre or vanishing-point in the heavens, and that this centre shifted its position with the stars, and hence was not telluric. The full significance of this observation was at once recognized by astronomers; it demonstrated beyond all cavil the cosmical origin of the shooting-stars. Some conservative meteorologists kept up the argument for the telluric origin for some decades to come, as a matter of course--such a band trails always in the rear of progress. But even these doubters were silenced when the great shower of shooting- stars appeared again in 1866, as predicted by Olbers and Newton, radiating from the same point of the heavens as before.

Since then the spectroscope has added its confirmatory evidence as to the identity of meteorite and shooting-star, and, moreover, has linked these atmospheric meteors with such distant cosmic residents as comets and nebulae. Thus it appears that Chladni's daring hypothesis of 1794 has been more than verified, and that the fragments of matter dissociated from planetary connection--which be postulated and was declared atheistic for postulating--have been shown to be billions of times

more numerous than any larger cosmic bodies of which we have cognizance--so widely does the existing universe differ from man's preconceived notions as to what it should be.

Thus also the "miracle" of the falling stone, against which the scientific scepticism of yesterday presented "an evil heart of unbelief," turns out to be the most natural phenomena, inasmuch as it is repeated in our atmosphere some millions of times each day.

THE AURORA BOREALIS

If fire-balls were thought miraculous and portentous in days of yore, what interpretation must needs have been put upon that vastly more picturesque phenomenon, the aurora? "Through all the city," says the Book of Maccabees, "for the space of almost forty days, there were seen horsemen running in the air, in cloth of gold, armed with lances, like a band of soldiers: and troops of horsemen in array encountering and running one against another, with shaking of shields and multitude of pikes, and drawing of swords, and casting of darts, and glittering of golden ornaments and harness." Dire omens these; and hardly less ominous the aurora seemed to all succeeding generations that observed it down well into the eighteenth century--as witness the popular excitement in England in 1716 over the brilliant aurora of that year, which became famous through Halley's description.

But after 1752, when Franklin dethroned the lightning, all spectacular meteors came to be regarded as natural phenomena, the aurora among the rest. Franklin explained the aurora--which was seen commonly enough in the eighteenth century, though only recorded once in the seventeenth--as due to the accumulation of electricity on the surface of polar snows, and its discharge to the equator through the upper atmosphere. Erasmus Darwin suggested that the luminosity might be due to the ignition of hydrogen, which was supposed by many philosophers to form the upper atmosphere. Dalton, who first measured the height of the aurora, estimating it at about one hundred miles, thought the phenomenon due to magnetism acting on ferruginous particles in the air, and his explanation was perhaps the most popular one at the beginning of the last century.

Since then a multitude of observers have studied the aurora, but the scientific grasp has found it as elusive in fact as it seems to casual observation, and its exact nature is as undetermined to-day as it was a hundred years ago. There has been no dearth of theories concerning it, however. Blot, who studied it in the Shetland Islands in 1817, thought it due to electrified ferruginous dust, the origin of which he ascribed to Icelandic volcanoes. Much more recently the idea of ferruginous particles has been revived, their presence being ascribed not to volcanoes, but to the meteorites constantly being dissipated in the upper atmosphere. Ferruginous dust, presumably of such origin, has been found on the polar snows, as well as on the snows of mountain-tops, but whether it could produce the phenomena of auroras is at least an open question.

Other theorists have explained the aurora as due to the accumulation of electricity on clouds or on spicules of ice in the upper air. Yet others think it due merely to the passage of electricity through rarefied air itself. Humboldt considered the matter settled in yet another way when Faraday showed, in 1831, that magnetism may produce luminous effects. But perhaps the prevailing theory of to-day assumes that the aurora is due to a current of electricity generated at the equator and passing through upper regions of space, to enter the earth at the magnetic poles--simply reversing the course which Franklin assumed.

The similarity of the auroral light to that generated in a vacuum bulb by the passage of electricity lends support to the long-standing supposition that the aurora is of electrical origin, but the subject still awaits complete elucidation. For once even that mystery- solver the spectroscope has been baffled, for the line it sifts from the aurora is not matched by that of any recognized substance. A like line is found in the zodiacal light, it is true, but this is of little aid, for the zodiacal light, though thought by some astronomers to be due to meteor swarms about the sun, is held to be, on the whole, as mysterious as the aurora itself.

Whatever the exact nature of the aurora, it has long been known to be intimately associated with the phenomena of terrestrial magnetism. Whenever a brilliant aurora is visible, the world is sure to be visited with what Humboldt called a magnetic storm--a "storm" which manifests itself to human senses in no way whatsoever except by deflecting the magnetic needle and conjuring with the electric wire. Such magnetic storms are curiously associated also with spots on the sun--just how no one has explained, though the fact itself is unquestioned. Sun-spots, too, seem directly linked with auroras, each of these phenomena passing through periods of greatest and least frequency in corresponding cycles of about eleven years' duration.

It was suspected a full century ago by Herschel that the variations in the number of sun-spots had a direct effect upon terrestrial weather, and he attempted to demonstrate it by using the price of wheat as a criterion of climatic conditions, meantime making careful observation of the sun-spots. Nothing very definite came of his efforts in this direction, the subject being far too complex to be determined without long periods of observation. Latterly, however, meteorologists, particularly in the tropics, are disposed to think they find evidence of some such connection between sun-spots and the weather as Herschel suspected. Indeed, Mr. Meldrum declares that there is a positive coincidence between periods of numerous sun-spots and seasons of excessive rain in India.

That some such connection does exist seems intrinsically probable. But the modern meteorologist, learning wisdom of the past, is extremely cautious about ascribing casual effects to astronomical phenomena. He finds it hard to forget that until recently all manner of climatic conditions were associated with phases of the moon; that not so very long ago showers of falling-stars were considered "prognostic" of certain kinds of weather; and that the "equinoctial storm" had been accepted as a verity by every one, until the unfeeling hand of statistics banished it from the earth.

Yet, on the other hand, it is easily within the possibilities that the science of the future may reveal associations between the weather and sun-spots, auroras, and terrestrial magnetism that as yet are hardly dreamed of. Until such time, however, these phenomena must feel themselves very grudgingly admitted to the inner circle of meteorology. More and more this science concerns itself, in our age of concentration and specialization, with weather and climate. Its votaries no longer concern themselves with stars or planets or comets or shooting-stars--once thought the very essence of guides to weather wisdom; and they are even looking askance at the moon, and asking her to show cause why she also should not be excluded from their domain. Equally little do they care for the interior of the earth, since they have learned that the central emanations of heat which Mairan imagined as a main source of aerial warmth can claim no such distinction. Even such problems as why the magnetic pole does not coincide with the geographical, and why the force of terrestrial magnetism decreases from the magnetic poles to the magnetic equator, as Humboldt first

discovered that it does, excite them only to lukewarm interest; for magnetism, they say, is not known to have any connection whatever with climate or weather.

EVAPORATION, CLOUD FORMATION, AND DEW

There is at least one form of meteor, however, of those that interested our forebears whose meteorological importance they did not overestimate. This is the vapor of water. How great was the interest in this familiar meteor at the beginning of the century is attested by the number of theories then extant regarding it; and these conflicting theories bear witness also to the difficulty with which the familiar phenomenon of the evaporation of water was explained.

Franklin had suggested that air dissolves water much as water dissolves salt, and this theory was still popular, though Deluc had disproved it by showing that water evaporates even more rapidly in a vacuum than in air. Deluc's own theory, borrowed from earlier chemists, was that evaporation is the chemical union of particles of water with particles of the supposititious element heat. Erasmus Darwin combined the two theories, suggesting that the air might hold a variable quantity of vapor in mere solution, and in addition a permanent moiety in chemical combination with caloric.

Undisturbed by these conflicting views, that strangely original genius, John Dalton, afterwards to be known as perhaps the greatest of theoretical chemists, took the question in hand, and solved it by showing that water exists in the air as an utterly independent gas. He reached a partial insight into the matter in 1793, when his first volume of meteorological essays was published; but the full elucidation of the problem came to him in 1801. The merit of his studies was at once recognized, but the tenability of his hypothesis was long and ardently disputed.

While the nature of evaporation was in dispute, as a matter of course the question of precipitation must be equally undetermined. The most famous theory of the period was that formulated by Dr. Hutton in a paper read before the Royal Society of Edinburgh, and published in the volume of transactions which contained also the same author's epoch-making paper on geology. This "theory of rain" explained precipitation as due to the cooling of a current of saturated air by contact with a colder current, the assumption being that the surplusage of moisture was precipitated in a chemical sense, just as the excess of salt dissolved in hot water is precipitated when the water cools. The idea that the cooling of the saturated air causes the precipitation of its moisture is the germ of truth that renders this paper of Hutton's important. All correct later theories build on this foundation.

"Let us suppose the surface of this earth wholly covered with water," said Hutton, "and that the sun were stationary, being always vertical in one place; then, from the laws of heat and rarefaction, there would be formed a circulation in the atmosphere, flowing from the dark and cold hemisphere to the heated and illuminated place, in all directions, towards the place of the greatest cold.

"As there is for the atmosphere of this earth a constant cooling cause, this fluid body could only arrive at a certain degree of heat; and this would be regularly decreasing from the centre of illumination to the opposite point of the globe, most distant from the light and heat. Between these two regions of extreme heat and cold there would, in every place, be found two streams of air following in opposite directions. If those streams of air, therefore, shall be supposed as both sufficiently saturated with humidity, then, as they are of different temperatures, there would be

formed a continual condensation of aqueous vapor, in some middle region of the atmosphere, by the commixtion of part of those two opposite streams.

"Hence there is reason to believe that in this supposed case there would be formed upon the surface of the globe three different regions--the torrid region, the temperate, and the frigid. These three regions would continue stationary; and the operations of each would be continual. In the torrid region, nothing but evaporation and heat would take place; no cloud could be formed, because in changing the transparency of the atmosphere to opacity it would be heated immediately by the operation of light, and thus the condensed water would be again evaporated. But this power of the sun would have a termination; and it is these that would begin the region of temperate heat and of continual rain. It is not probable that the region of temperance would reach far beyond the region of light; and in the hemisphere of darkness there would be found a region of extreme cold and perfect dryness.

"Let us now suppose the earth as turning on its axis in the equinoctial situation. The torrid region would thus be changed into a zone, in which there would be night and day; consequently, here would be much temperance, compared with the torrid region now considered; and here perhaps there would be formed periodical condensation and evaporation of humidity, corresponding to the seasons of night and day. As temperance would thus be introduced into the region of torrid extremity, so would the effect of this change be felt over all the globe, every part of which would now be illuminated, consequently heated in some degree. Thus we would have a line of great heat and evaporation, graduating each way into a point of great cold and congelation. Between these two extremes of heat and cold there would be found in each hemisphere a region of much temperance, in relation to heat, but of much humidity in the atmosphere, perhaps of continual rain and condensation.

"The supposition now formed must appear extremely unfit for making this globe a habitable world in every part; but having thus seen the effect of night and day in temperating the effects of heat and cold in every place, we are now prepared to contemplate the effects of supposing this globe to revolve around the sun with a certain inclination of its axis. By this beautiful contrivance, that comparatively uninhabited globe is now divided into two hemispheres, each of which is thus provided with a summer and a winter season. But our present view is limited to the evaporation and condensation of humidity; and, in this contrivance of the seasons, there must appear an ample provision for those alternate operations in every part; for as the place of the vertical sun is moved alternately from one tropic to the other, heat and cold, the original causes of evaporation and condensation, must be carried over all the globe, producing either annual seasons of rain or diurnal seasons of condensation and evaporation, or both these seasons, more or less--that is, in some degree.

"The original cause of motion in the atmosphere is the influence of the sun heating the surface of the earth exposed to that luminary. We have not supposed that surface to have been of one uniform shape and similar substance; from whence it has followed that the annual propers of the sun, perhaps also the diurnal propers, would produce a regular condensation of rain in certain regions, and the evaporation of humidity in others; and this would have a regular progress in certain determined seasons, and would not vary. But nothing can be more distant from this supposition, that is the natural constitution of the earth; for the globe is composed of sea and land, in no regular

shape or mixture, while the surface of the land is also irregular with respect to its elevations and depressions, and various with regard to the humidity and dryness of that part which is exposed to heat as the cause of evaporation. Hence a source of the most valuable motions in the fluid atmosphere with aqueous vapor, more or less, so far as other natural operations will admit; and hence a source of the most irregular commixture of the several parts of this elastic fluid, whether saturated or not with aqueous vapor.

"According to the theory, nothing is required for the production of rain besides the mixture of portions of the atmosphere with humidity, and of mixing the parts that are in different degrees of heat. But we have seen the causes of saturating every portion of the atmosphere with humidity and of mixing the parts which are in different degrees of heat. Consequently, over all the surface of the globe there should happen occasionally rain and evaporation, more or less; and also, in every place, those vicissitudes should be observed to take place with some tendency to regularity, which, however, may be so disturbed as to be hardly distinguishable upon many occasions. Variable winds and variable rains should be found in proportion as each place is situated in an irregular mixture of land and water; whereas regular winds should be found in proportion to the uniformity of the surface; and regular rains in proportion to the regular changes of those winds by which the mixture of the atmosphere necessary to the rain may be produced. But as it will be acknowledged that this is the case in almost all this earth where rain appears according to the conditions here specified, the theory is found to be thus in conformity with nature, and natural appearances are thus explained by the theory."[1]

The next ambitious attempt to explain the phenomena of aqueous meteors was made by Luke Howard, in his remarkable paper on clouds, published in the Philosophical Magazine in 1803--the paper in which the names cirrus, cumulus, stratus, etc., afterwards so universally adopted, were first proposed. In this paper Howard acknowledges his indebtedness to Dalton for the theory of evaporation; yet he still clings to the idea that the vapor, though independent of the air, is combined with particles of caloric. He holds that clouds are composed of vapor that has previously risen from the earth, combating the opinions of those who believe that they are formed by the union of hydrogen and oxygen existing independently in the air; though he agrees with these theorists that electricity has entered largely into the modus operandi of cloud formation. He opposes the opinion of Deluc and De Saussure that clouds are composed of particles of water in the form of hollow vesicles (miniature balloons, in short, perhaps filled with hydrogen), which untenable opinion was a revival of the theory as to the formation of all vapor which Dr. Halley had advocated early in the eighteenth century.

Of particular interest are Howard's views as to the formation of dew, which he explains as caused by the particles of caloric forsaking the vapor to enter the cool body, leaving the water on the surface. This comes as near the truth, perhaps, as could be expected while the old idea as to the materiality of heat held sway. Howard believed, however, that dew is usually formed in the air at some height, and that it settles to the surface, opposing the opinion, which had gained vogue in France and in America (where Noah Webster prominently advocated it), that dew ascends from the earth.

The complete solution of the problem of dew formation-- which really involved also the entire question of precipitation of watery vapor in any form--was made by Dr. W. C. Wells, a man of

American birth, whose life, however, after boyhood, was spent in Scotland (where as a young man he enjoyed the friendship of David Hume) and in London. Inspired, no doubt, by the researches of Mack, Hutton, and their confreres of that Edinburgh school, Wells made observations on evaporation and precipitation as early as 1784, but other things claimed his attention; and though he asserts that the subject was often in his mind, he did not take it up again in earnest until about 1812.

Meantime the observations on heat of Rumford and Davy and Leslie had cleared the way for a proper interpretation of the facts--about the facts themselves there had long been practical unanimity of opinion. Dr. Black, with his latent-heat observations, had really given the clew to all subsequent discussions of the subject of precipitation of vapor; and from this time on it had been known that heat is taken up when water evaporates, and given out again when it condenses. Dr. Darwin had shown in 1788, in a paper before the Royal Society, that air gives off heat on contracting and takes it up on expanding; and Dalton, in his essay of 1793, had explained this phenomenon as due to the condensation and vaporization of the water contained in the air.

But some curious and puzzling observations which Professor Patrick Wilson, professor of astronomy in the University of Glasgow, had communicated to the Royal Society of Edinburgh in 1784, and some similar ones made by Mr. Six, of Canterbury, a few years later, had remained unexplained. Both these gentlemen observed that the air is cooler where dew is forming than the air a few feet higher, and they inferred that the dew in forming had taken up heat, in apparent violation of established physical principles.

It remained for Wells, in his memorable paper of 1816, to show that these observers had simply placed the cart before the horse. He made it clear that the air is not cooler because the dew is formed, but that the dew is formed because the air is cooler--having become so through radiation of heat from the solids on which the dew forms. The dew itself, in forming, gives out its latent heat, and so tends to equalize the temperature.

Wells's paper is so admirable an illustration of the lucid presentation of clearly conceived experiments and logical conclusions that we should do it injustice not to present it entire. The author's mention of the observations of Six and Wilson gives added value to his own presentation.

Dr. Wells's Essay on Dew

"I was led in the autumn of 1784, by the event of a rude experiment, to think it probable that the formation of dew is attended with the production of cold. In 1788, a paper on hoar-frost, by Mr. Patrick Wilson, of Glasgow, was published in the first volume of the Transactions of the Royal Society of Edinburgh, by which it appeared that this opinion bad been entertained by that gentleman before it had occurred to myself. In the course of the same year, Mr. Six, of Canterbury, mentioned in a paper communicated to the Royal Society that on clear and dewy nights he always found the mercury lower in a thermometer laid upon the ground in a meadow in his neighborhood than it was in a similar thermometer suspended in the air six feet above the former; and that upon one night the difference amounted to five degrees of Fahrenheit's scale. Mr. Six, however, did not suppose, agreeably to the opinion of Mr. Wilson and myself, that the cold was occasioned by the formation of dew, but imagined that it proceeded partly from the low temperature of the air, through which the dew, already formed in the atmosphere, had descended, and partly from the

evaporation of moisture from the ground, on which his thermometer had been placed. The conjecture of Mr. Wilson and the observations of Mr. Six, together with many facts which I afterwards learned in the course of reading, strengthened my opinion; but I made no attempt, before the autumn of 1811, to ascertain by experiment if it were just, though it had in the mean time almost daily occurred to my thoughts. Happening, in that season, to be in that country in a clear and calm night, I laid a thermometer upon grass wet with dew, and suspended a second in the air, two feet above the other. An hour afterwards the thermometer on the grass was found to be eight degrees lower, by Fahrenheit's division, than the one in the air. Similar results having been obtained from several similar experiments, made during the same autumn, I determined in the next spring to prosecute the subject with some degree of steadiness, and with that view went frequently to the house of one of my friends who lives in Surrey.

At the end of two months I fancied that I had collected information worthy of being published; but, fortunately, while preparing an account of it I met by accident with a small posthumous work by Mr. Six, printed at Canterbury in 1794, in which are related differences observed on dewy nights between thermometers placed upon grass and others in the air that are much greater than those mentioned in the paper presented by him to the Royal Society in 1788. In this work, too, the cold of the grass is attributed, in agreement with the opinion of Mr. Wilson, altogether to the dew deposited upon it. The value of my own observations appearing to me now much diminished, though they embraced many points left untouched by Mr. Six, I gave up my intentions of making them known. Shortly after, however, upon considering the subject more closely, I began to suspect that Mr. Wilson, Mr. Six, and myself had all committed an error regarding the cold which accompanies dew as an effect of the formation of that fluid. I therefore resumed my experiments, and having by means of them, I think, not only established the justness of my suspicions, but ascertained the real cause both of dew and of several other natural appearances which have hitherto received no sufficient explanation, I venture now to submit to the consideration of the learned an account of some of my labors, without regard to the order of time in which they were performed, and of various conclusions which may be drawn from them, mixed with facts and opinions already published by others:

"There are various occurrences in nature which seem to me strictly allied to dew, though their relation to it be not always at first sight perceivable. The statement and explanation of several of these will form the concluding part of the present essay.

"1. I observed one morning, in winter, that the insides of the panes of glass in the windows of my bedchamber were all of them moist, but that those which had been covered by an inside shutter during the night were much more so than the others which had been uncovered. Supposing that this diversity of appearance depended upon a difference of temperature, I applied the naked bulbs of two delicate thermometers to a covered and uncovered pane; on which I found that the former was three degrees colder than the latter. The air of the chamber, though no fire was kept in it, was at this time eleven and one-half degrees warmer than that without. Similar experiments were made on many other mornings, the results of which were that the warmth of the internal air exceeded that of the external from eight to eighteen degrees, the temperature of the covered panes would be from one to five degrees less than the uncovered; that the covered were sometimes dewed, while the uncovered were dry; that at other times both were free from moisture; that the outsides of the covered and uncovered panes had similar differences with respect to heat, though not so great as

those of the inner surfaces; and that no variation in the quantity of these differences was occasioned by the weather's being cloudy or fair, provided the heat of the internal air exceeded that of the external equally in both of those states of the atmosphere.

"The remote reason of these differences did not immediately present itself. I soon, however, saw that the closed shutter shielded the glass which it covered from the heat that was radiated to the windows by the walls and furniture of the room, and thus kept it nearer to the temperature of the external air than those parts could be which, from being uncovered, received the heat emitted to them by the bodies just mentioned.

"In making these experiments, I seldom observed the inside of any pane to be more than a little damped, though it might be from eight to twelve degrees colder than the general mass of the air in the room; while, in the open air, I had often found a great dew to form on substances only three or four degrees colder than the atmosphere. This at first surprised me; but the cause now seems plain. The air of the chamber had once been a portion of the external atmosphere, and had afterwards been heated, when it could receive little accessories to its original moisture. It constantly required being cooled considerably before it was even brought back to its former nearness to repletion with water; whereas the whole external air is commonly, at night, nearly replete with moisture, and therefore readily precipitates dew on bodies only a little colder than itself.

"When the air of a room is warmer than the external atmosphere, the effect of an outside shutter on the temperature of the glass of the window will be directly opposite to what has just been stated; since it must prevent the radiation, into the atmosphere, of the heat of the chamber transmitted through the glass.

"2. Count Rumford appears to have rightly conjectured that the inhabitants of certain hot countries, who sleep at nights on the tops of their houses, are cooled during this exposure by the radiation of their heat to the sky; or, according to his manner of expression, by receiving frigorific rays from the heavens. Another fact of this kind seems to be the greater chill which we often experience upon passing at night from the cover of a house into the air than might have been expected from the cold of the external atmosphere. The cause, indeed, is said to be the quickness of transition from one situation to another. But if this were the whole reason, an equal chill would be felt in the day, when the difference, in point of heat, between the internal and external air was the same as at night, which is not the case. Besides, if I can trust my own observation, the feeling of cold from this cause is more remarkable in a clear than in a cloudy night, and in the country than in towns. The following appears to be the manner in which these things are chiefly to be explained:

"During the day our bodies while in the open air, although not immediately exposed to the sun's rays, are yet constantly deriving heat from them by means of the reflection of the atmosphere. This heat, though it produces little change on the temperature of the air which it traverses, affords us some compensation for the heat which we radiate to the heavens. At night, also, if the sky be overcast, some compensation will be made to us, both in the town and in the country, though in a less degree than during the day, as the clouds will remit towards the earth no inconsiderable quantity of heat. But on a clear night, in an open part of the country, nothing almost can be returned to us from above in place of the heat which we radiate upward. In towns, however, some

compensation will be afforded even on the clearest nights for the heat which we lose in the open air by that which is radiated to us from the sun round buildings.

To our loss of heat by radiation at times that we derive little compensation from the radiation of other bodies is probably to be attributed a great part of the hurtful effects of the night air. Descartes says that these are not owing to dew, as was the common opinion of his contemporaries, but to the descent of certain noxious vapors which have been exhaled from the earth during the heat of the day, and are afterwards condensed by the cold of a serene night. The effects in question certainly cannot be occasioned by dew, since that fluid does not form upon a healthy human body in temperate climates; but they may, notwithstanding, arise from the same cause that produces dew on those substances which do not, like the human body, possess the power of generating heat for the supply of what they lose by radiation or any other means."[2]

This explanation made it plain why dew forms on a clear night, when there are no clouds to reflect the radiant heat. Combined with Dalton's theory that vapor is an independent gas, limited in quantity in any given space by the temperature of that space, it solved the problem of the formation of clouds, rain, snow, and hoar-frost. Thus this paper of Wells's closed the epoch of speculation regarding this field of meteorology, as Hutton's paper of 1784 had opened it. The fact that the volume containing Hutton's paper contained also his epoch-making paper on geology finds curiously a duplication in the fact that Wells's volume contained also his essay on Albinism, in which the doctrine of natural selection was for the first time formulated, as Charles Darwin freely admitted after his own efforts had made the doctrine famous.

ISOTHERMS AND OCEAN CURRENTS

The very next year after Dr. Wells's paper was published there appeared in France the third volume of the Memoires de Physique et de Chimie de la Societe d'Arcueil, and a new epoch in meteorology was inaugurated. The society in question was numerically an inconsequential band, listing only a dozen members; but every name was a famous one: Arago, Berard, Berthollet, Biot, Chaptal, De Candolle, Dulong, Gay-Lussac, Humboldt, Laplace, Poisson, and Thenard--rare spirits every one. Little danger that the memoirs of such a band would be relegated to the dusty shelves where most proceedings of societies belong--no milk-for-babes fare would be served to such a company.

The particular paper which here interests us closes this third and last volume of memoirs. It is entitled "Des Lignes Isothermes et de la Distribution de la Chaleursurle Globe." The author is Alexander Humboldt. Needless to say, the topic is handled in a masterly manner. The distribution of heat on the surface of the globe, on the mountain-sides, in the interior of the earth; the causes that regulate such distribution; the climatic results--these are the topics discussed. But what gives epochal character to the paper is the introduction of those isothermal lines circling the earth in irregular course, joining together places having the same mean annual temperature, and thus laying the foundation for a science of comparative climatology.

It is true the attempt to study climates comparatively was not new. Mairan had attempted it in those papers in which he developed his bizarre ideas as to central emanations of heat. Euler had brought his profound mathematical genius to bear on the topic, evolving the "extraordinary conclusion that under the equator at midnight the cold ought to be more rigorous than at the poles in winter." And

in particular Richard Kirwan, the English chemist, had combined the mathematical and the empirical methods and calculated temperatures for all latitudes. But Humboldt differs from all these predecessors in that he grasps the idea that the basis of all such computations should be not theory, but fact. He drew his isothermal lines not where some occult calculation would locate them on an ideal globe, but where practical tests with the thermometer locate them on our globe as it is. London, for example, lies in the same latitude as the southern extremity of Hudson Bay; but the isotherm of London, as Humboldt outlines it, passes through Cincinnati.

Of course such deviations of climatic conditions between places in the same latitude had long been known. As Humboldt himself observes, the earliest settlers of America were astonished to find themselves subjected to rigors of climate for which their European experience had not at all prepared them. Moreover, sagacious travellers, in particular Cook's companion on his second voyage, young George Forster, had noted as a general principle that the western borders of continents in temperate regions are always warmer than corresponding latitudes of their eastern borders; and of course the general truth of temperatures being milder in the vicinity of the sea than in the interior of continents had long been familiar. But Humboldt's isothermal lines for the first time gave tangibility to these ideas, and made practicable a truly scientific study of comparative climatology.

In studying these lines, particularly as elaborated by further observations, it became clear that they are by no means haphazard in arrangement, but are dependent upon geographical conditions which in most cases are not difficult to determine. Humboldt himself pointed out very clearly the main causes that tend to produce deviations from the average--or, as Dove later on called it, the normal--temperature of any given latitude. For example, the mean annual temperature of a region (referring mainly to the northern hemisphere) is raised by the proximity of a western coast; by a divided configuration of the continent into peninsulas; by the existence of open seas to the north or of radiating continental surfaces to the south; by mountain ranges to shield from cold winds; by the infrequency of swamps to become congealed; by the absence of woods in a dry, sandy soil; and by the serenity of sky in the summer months and the vicinity of an ocean current bringing water which is of a higher temperature than that of the surrounding sea.

Conditions opposite to these tend, of course, correspondingly to lower the temperature. In a word, Humboldt says the climatic distribution of heat depends on the relative distribution of land and sea, and on the "hypsometrical configuration of the continents"; and he urges that "great meteorological phenomena cannot be comprehended when considered independently of geognostic relations"--a truth which, like most other general principles, seems simple enough once it is pointed out.

With that broad sweep of imagination which characterized him, Humboldt speaks of the atmosphere as the "aerial ocean, in the lower strata and on the shoals of which we live," and he studies the atmospheric phenomena always in relation to those of that other ocean of water. In each of these oceans there are vast permanent currents, flowing always in determinate directions, which enormously modify the climatic conditions of every zone. The ocean of air is a vast maelstrom, boiling up always under the influence of the sun's heat at the equator, and flowing as an upper current towards either pole, while an undercurrent from the poles, which becomes the trade-winds, flows towards the equator to supply its place.

But the superheated equatorial air, becoming chilled, descends to the surface in temperate latitudes, and continues its poleward journey as the anti-trade-winds. The trade-winds are deflected towards the west, because in approaching the equator they constantly pass over surfaces of the earth having a greater and greater velocity of rotation, and so, as it were, tend to lag behind-- an explanation which Hadley pointed out in 1735, but which was not accepted until Dalton independently worked it out and promulgated it in 1793. For the opposite reason, the anti-trades are deflected towards the east; hence it is that the western, borders of continents in temperate zones are bathed in moist sea-breezes, while their eastern borders lack this cold- dispelling influence.

In the ocean of water the main currents run as more sharply circumscribed streams--veritable rivers in the sea. Of these the best known and most sharply circumscribed is the familiar Gulf Stream, which has its origin in an equatorial current, impelled westward by trade-winds, which is deflected northward in the main at Cape St. Roque, entering the Caribbean Sea and Gulf of Mexico, to emerge finally through the Strait of Florida, and journey off across the Atlantic to warm the shores of Europe.

Such, at least, is the Gulf Stream as Humboldt understood it. Since his time, however, ocean currents in general, and this one in particular, have been the subject of no end of controversy, it being hotly disputed whether either causes or effects of the Gulf Stream are just what Humboldt, in common with others of his time, conceived them to be. About the middle of the century Lieutenant M. F. Maury, the distinguished American hydrographer and meteorologist, advocated a theory of gravitation as the chief cause of the currents, claiming that difference in density, due to difference in temperature and saltness, would sufficiently account for the oceanic circulation. This theory gained great popularity through the wide circulation of Maury's Physical Geography of the Sea, which is said to have passed through more editions than any other scientific book of the period; but it was ably and vigorously combated by Dr. James Croll, the Scottish geologist, in his Climate and Time, and latterly the old theory that ocean currents are due to the trade-winds has again come into favor. Indeed, very recently a model has been constructed, with the aid of which it is said to have been demonstrated that prevailing winds in the direction of the actual trade-winds would produce such a current as the Gulf Stream.

Meantime, however, it is by no means sure that gravitation does not enter into the case to the extent of producing an insensible general oceanic circulation, independent of the Gulf Stream and similar marked currents, and similar in its larger outlines to the polar- equatorial circulation of the air. The idea of such oceanic circulation was first suggested in detail by Professor Lenz, of St. Petersburg, in 1845, but it was not generally recognized until Dr. Carpenter independently hit upon the idea more than twenty years later. The plausibility of the conception is obvious; yet the alleged fact of such circulation has been hotly disputed, and the question is still sub judice.

But whether or not such general circulation of ocean water takes place, it is beyond dispute that the recognized currents carry an enormous quantity of heat from the tropics towards the poles. Dr. Croll, who has perhaps given more attention to the physics of the subject than almost any other person, computes that the Gulf Stream conveys to the North Atlantic one- fourth as much heat as that body receives directly from the sun, and he argues that were it not for the transportation of heat by this and similar Pacific currents, only a narrow tropical region of the globe would be warm enough for habitation by the existing faunas. Dr. Croll argues that a slight change in the relative

values of northern and southern trade-winds (such as he believes has taken place at various periods in the past) would suffice to so alter the equatorial current which now feeds the Gulf Stream that its main bulk would be deflected southward instead of northward, by the angle of Cape St. Roque. Thus the Gulf Stream would be nipped in the bud, and, according to Dr. Croll's estimates, the results would be disastrous for the northern hemisphere. The anti-trades, which now are warmed by the Gulf Stream, would then blow as cold winds across the shores of western Europe, and in all probability a glacial epoch would supervene throughout the northern hemisphere.

The same consequences, so far as Europe is concerned at least, would apparently ensue were the Isthmus of Panama to settle into the sea, allowing the Caribbean current to pass into the Pacific. But the geologist tells us that this isthmus rose at a comparatively recent geological period, though it is hinted that there had been some time previously a temporary land connection between the two continents. Are we to infer, then, that the two Americas in their unions and disunions have juggled with the climate of the other hemisphere? Apparently so, if the estimates made of the influence of the Gulf Stream be tenable. It is a far cry from Panama to Russia. Yet it seems within the possibilities that the meteorologist may learn from the geologist of Central America something that will enable him to explain to the paleontologist of Europe how it chanced that at one time the mammoth and rhinoceros roamed across northern Siberia, while at another time the reindeer and musk-ox browsed along the shores of the Mediterranean.

Possibilities, I said, not probabilities. Yet even the faint glimmer of so alluring a possibility brings home to one with vividness the truth of Humboldt's perspicuous observation that meteorology can be properly comprehended only when studied in connection with the companion sciences. There are no isolated phenomena in nature.

CYCLONES AND ANTI-CYCLONES

Yet, after all, it is not to be denied that the chief concern of the meteorologist must be with that other medium, the "ocean of air, on the shoals of which we live." For whatever may be accomplished by water currents in the way of conveying heat, it is the wind currents that effect the final distribution of that heat. As Dr. Croll has urged, the waters of the Gulf Stream do not warm the shores of Europe by direct contact, but by warming the anti-trade-winds, which subsequently blow across the continent. And everywhere the heat accumulated by water becomes effectual in modifying climate, not so much by direct radiation as by diffusion through the medium of the air.

This very obvious importance of aerial currents led to their practical study long before meteorology had any title to the rank of science, and Dalton's explanation of the trade-winds had laid the foundation for a science of wind dynamics before the beginning of the nineteenth century. But no substantial further advance in this direction was effected until about 1827, when Heinrich W. Dove, of Konigsberg, afterwards to be known as perhaps the foremost meteorologist of his generation, included the winds among the subjects of his elaborate statistical studies in climatology.

Dove classified the winds as permanent, periodical, and variable. His great discovery was that all winds, of whatever character, and not merely the permanent winds, come under the influence of the earth's rotation in such a way as to be deflected from their course, and hence to take on a gyratory motion--that, in short, all local winds are minor eddies in the great polar-equatorial whirl, and tend

to reproduce in miniature the character of that vast maelstrom. For the first time, then, temporary or variable winds were seen to lie within the province of law.

A generation later, Professor William Ferrel, the American meteorologist, who had been led to take up the subject by a perusal of Maury's discourse on ocean winds, formulated a general mathematical law, to the effect that any body moving in a right line along the surface of the earth in any direction tends to have its course deflected, owing to the earth's rotation, to the right hand in the northern and to the left hand in the southern hemisphere. This law had indeed been stated as early as 1835 by the French physicist Poisson, but no one then thought of it as other than a mathematical curiosity; its true significance was only understood after Professor Ferrel had independently rediscovered it (just as Dalton rediscovered Hadley's forgotten law of the trade-winds) and applied it to the motion of wind currents.

Then it became clear that here is a key to the phenomena of atmospheric circulation, from the great polar-equatorial maelstrom which manifests itself in the trade-winds to the most circumscribed riffle which is announced as a local storm. And the more the phenomena were studied, the more striking seemed the parallel between the greater maelstrom and these lesser eddies. Just as the entire atmospheric mass of each hemisphere is seen, when viewed as a whole, to be carried in a great whirl about the pole of that hemisphere, so the local disturbances within this great tide are found always to take the form of whirls about a local storm-centre--which storm-centre, meantime, is carried along in the major current, as one often sees a little whirlpool in the water swept along with the main current of the stream. Sometimes, indeed, the local eddy, caught as it were in an ancillary current of the great polar stream, is deflected from its normal course and may seem to travel against the stream; but such deviations are departures from the rule. In the great majority of cases, for example, in the north temperate zone, a storm-centre (with its attendant local whirl) travels to the northeast, along the main current of the anti-trade-wind, of which it is a part; and though exceptionally its course may be to the southeast instead, it almost never departs so widely from the main channel as to progress to the westward. Thus it is that storms sweeping over the United States can be announced, as a rule, at the seaboard in advance of their coming by telegraphic communication from the interior, while similar storms come to Europe off the ocean unannounced. Hence the more practical availability of the forecasts of weather bureaus in the former country.

But these local whirls, it must be understood, are local only in a very general sense of the word, inasmuch as a single one may be more than a thousand miles in diameter, and a small one is two or three hundred miles across. But quite without regard to the size of the whirl, the air composing it conducts itself always in one of two ways. It never whirls in concentric circles; it always either rushes in towards the centre in a descending spiral, in which case it is called a cyclone, or it spreads out from the centre in a widening spiral, in which case it is called an anti-cyclone. The word cyclone is associated in popular phraseology with a terrific storm, but it has no such restriction in technical usage. A gentle zephyr flowing towards a "storm- centre" is just as much a cyclone to the meteorologist as is the whirl constituting a West-Indian hurricane. Indeed, it is not properly the wind itself that is called the cyclone in either case, but the entire system of whirls--including the storm-centre itself, where there may be no wind at all.

What, then, is this storm-centre? Merely an area of low barometric pressure--an area where the air has become lighter than the air of surrounding regions. Under influence of gravitation the air seeks its level just as water does; so the heavy air comes flowing in from all sides towards the low-pressure area, which thus becomes a "storm-centre." But the inrushing currents never come straight to their mark. In accordance with Ferrel's law, they are deflected to the right, and the result, as will readily be seen, must be a vortex current, which whirls always in one direction--namely, from left to right, or in the direction opposite to that of the hands of a watch held with its face upward. The velocity of the cyclonic currents will depend largely upon the difference in barometric pressure between the storm-centre and the confines of the cyclone system. And the velocity of the currents will determine to some extent the degree of deflection, and hence the exact path of the descending spiral in which the wind approaches the centre. But in every case and in every part of the cyclone system it is true, as Buys Ballot's famous rule first pointed out, that a person standing with his back to the wind has the storm-centre at his left.

The primary cause of the low barometric pressure which marks the storm-centre and establishes the cyclone is expansion of the air through excess of temperature. The heated air, rising into cold upper regions, has a portion of its vapor condensed into clouds, and now a new dynamic factor is added, for each particle of vapor, in condensing, gives up its modicum of latent heat. Each pound of vapor thus liberates, according to Professor Tyndall's estimate, enough heat to melt five pounds of cast iron; so the amount given out where large masses of cloud are forming must enormously add to the convection currents of the air, and hence to the storm-developing power of the forming cyclone. Indeed, one school of meteorologists, of whom Professor Espy was the leader, has held that, without such added increment of energy constantly augmenting the dynamic effects, no storm could long continue in violent action. And it is doubted whether any storm could ever attain, much less continue, the terrific force of that most dreaded of winds of temperate zones, the tornado--a storm which obeys all the laws of cyclones, but differs from ordinary cyclones in having a vortex core only a few feet or yards in diameter-- without the aid of those great masses of condensing vapor which always accompany it in the form of storm- clouds.

The anti-cyclone simply reverses the conditions of the cyclone. Its centre is an area of high pressure, and the air rushes out from it in all directions towards surrounding regions of low pressure. As before, all parts of the current will be deflected towards the right, and the result, clearly, is a whirl opposite in direction to that of the cyclone. But here there is a tendency to dissipation rather than to concentration of energy, hence, considered as a storm-generator, the anti-cyclone is of relative insignificance.

In particular the professional meteorologist who conducts a "weather bureau"--as, for example, the chief of the United States signal-service station in New York--is so preoccupied with the observation of this phenomenon that cyclone-hunting might be said to be his chief pursuit. It is for this purpose, in the main, that government weather bureaus or signal- service departments have been established all over the world. Their chief work is to follow up cyclones, with the aid of telegraphic reports, mapping their course and recording the attendant meteorological conditions. Their so-called predictions or forecasts are essentially predications, gaining locally the effect of predictions because the telegraph outstrips the wind.

At only one place on the globe has it been possible as yet for the meteorologist to make long-time forecasts meriting the title of predictions. This is in the middle Ganges Valley of northern India. In this country the climatic conditions are largely dependent upon the periodical winds called monsoons, which blow steadily landward from April to October, and seaward from October to April. The summer monsoons bring the all-essential rains; if they are delayed or restricted in extent, there will be drought and consequent famine. And such restriction of the monsoon is likely to result when there has been an unusually deep or very late snowfall on the Himalayas, because of the lowering of spring temperature by the melting snow. Thus here it is possible, by observing the snowfall in the mountains, to predict with some measure of success the average rainfall of the following summer. The drought of 1896, with the consequent famine and plague that devastated India the following winter, was thus predicted some months in advance.

This is the greatest present triumph of practical meteorology. Nothing like it is yet possible anywhere in temperate zones. But no one can say what may not be possible in times to come, when the data now being gathered all over the world shall at last be co-ordinated, classified, and made the basis of broad inductions. Meteorology is pre-eminently a science of the future.

VI MODERN THEORIES OF HEAT AND LIGHT

THE eighteenth-century philosopher made great strides in his studies of the physical properties of matter and the application of these properties in mechanics, as the steam-engine, the balloon, the optic telegraph, the spinning-jenny, the cotton-gin, the chronometer, the perfected compass, the Leyden jar, the lightning-rod, and a host of minor inventions testify. In a speculative way he had thought out more or less tenable conceptions as to the ultimate nature of matter, as witness the theories of Leibnitz and Boscovich and Davy, to which we may recur. But he had not as yet conceived the notion of a distinction between matter and energy, which is so fundamental to the physics of a later epoch. He did not speak of heat, light, electricity, as forms of energy or "force"; he conceived them as subtile forms of matter--as highly attenuated yet tangible fluids, subject to gravitation and chemical attraction; though he had learned to measure none of them but heat with accuracy, and this one he could test only within narrow limits until late in the century, when Josiah Wedgwood, the famous potter, taught him to gauge the highest temperatures with the clay pyrometer.

He spoke of the matter of heat as being the most universally distributed fluid in nature; as entering in some degree into the composition of nearly all other substances; as being sometimes liquid, sometimes condensed or solid, and as having weight that could be detected with the balance. Following Newton, he spoke of light as a "corpuscular emanation" or fluid, composed of shining particles which possibly are transmutable into particles of heat, and which enter into chemical combination with the particles of other forms of matter. Electricity he considered a still more subtile kind of matter-perhaps an attenuated form of light. Magnetism, "vital fluid," and by some even a "gravic fluid," and a fluid of sound were placed in the same scale; and, taken together, all these supposed subtile forms of matter were classed as "imponderables."

This view of the nature of the "imponderables" was in some measure a retrogression, for many seventeenth- century philosophers, notably Hooke and Huygens and Boyle, had held more correct views; but the materialistic conception accorded so well with the eighteenth- century tendencies of thought that only here and there a philosopher like Euler called it in question, until well on towards the close of the century. Current speech referred to the materiality of the "imponderables " unquestioningly. Students of meteorology--a science that was just dawning--explained atmospheric phenomena on the supposition that heat, the heaviest imponderable, predominated in the lower atmosphere, and that light, electricity, and magnetism prevailed in successively higher strata. And Lavoisier, the most philosophical chemist of the century, retained heat and light on a par with oxygen, hydrogen, iron, and the rest, in his list of elementary substances.

COUNT RUMFORD AND THE VIBRATORY THEORY OF HEAT

But just at the close of the century the confidence in the status of the imponderables was rudely shaken in the minds of philosophers by the revival of the old idea of Fra Paolo and Bacon and Boyle, that heat, at any rate, is not a material fluid, but merely a mode of motion or vibration among the particles of "ponderable" matter. The new champion of the old doctrine as to the nature of heat was a very distinguished philosopher and diplomatist of the time, who, it may be worth recalling, was an American. He was a sadly expatriated American, it is true, as his name, given all the official appendages, will amply testify; but he had been born and reared in a Massachusetts village none the less, and he seems always to have retained a kindly interest in the land of his nativity, even though he lived abroad in the service of other powers during all the later years of his life, and was knighted by England, ennobled by Bavaria, and honored by the most distinguished scientific bodies of Europe. The American, then, who championed the vibratory theory of heat, in opposition to all current opinion, in this closing era of the eighteenth century, was Lieutenant-General Sir Benjamin Thompson, Count Rumford, F.R.S.

Rumford showed that heat may be produced in indefinite quantities by friction of bodies that do not themselves lose any appreciable matter in the process, and claimed that this proves the immateriality of heat. Later on he added force to the argument by proving, in refutation of the experiments of Bowditch, that no body either gains or loses weight in virtue of being heated or cooled. He thought he had proved that heat is only a form of motion.

His experiment for producing indefinite quantities of heat by friction is recorded by him in his paper entitled, "Inquiry Concerning the Source of Heat Excited by Friction."

"Being engaged, lately, in superintending the boring of cannon in the workshops of the military arsenal at Munich," he says, "I was struck with the very considerable degree of heat which a brass gun acquires in a short time in being bored; and with the still more intense heat (much greater than that of boiling water, as I found by experiment) of the metallic chips separated from it by the borer.

"Taking a cannon (a brass six-pounder), cast solid, and rough, as it came from the foundry, and fixing it horizontally in a machine used for boring, and at the same time finishing the outside of the cannon by turning, I caused its extremity to be cut off; and by turning down the metal in that part, a solid cylinder was formed, 7 3/4 inches in diameter and 9 8/10 inches long; which, when finished,

remained joined to the rest of the metal (that which, properly speaking, constituted the cannon) by a small cylindrical neck, only 2 1/5 inches in diameter and 3 8/10 inches long.

"This short cylinder, which was supported in its horizontal position, and turned round its axis by means of the neck by which it remained united to the cannon, was now bored with the horizontal borer used in boring cannon.

"This cylinder being designed for the express purpose of generating heat by friction, by having a blunt borer forced against its solid bottom at the same time that it should be turned round its axis by the force of horses, in order that the heat accumulated in the cylinder might from time to time be measured, a small, round hole 0.37 of an inch only in diameter and 4.2 inches in depth, for the purpose of introducing a small cylindrical mercurial thermometer, was made in it, on one side, in a direction perpendicular to the axis of the cylinder, and ending in the middle of the solid part of the metal which formed the bottom of the bore.

"At the beginning of the experiment, the temperature of the air in the shade, as also in the cylinder, was just sixty degrees Fahrenheit. At the end of thirty minutes, when the cylinder had made 960 revolutions about its axis, the horses being stopped, a cylindrical mercury thermometer, whose bulb was 32/100 of an inch in diameter and 3 1/4 inches in length, was introduced into the hole made to receive it in the side of the cylinder, when the mercury rose almost instantly to one hundred and thirty degrees.

"In order, by one decisive experiment, to determine whether the air of the atmosphere had any part or not in the generation of the heat, I contrived to repeat the experiment under circumstances in which it was evidently impossible for it to produce any effect whatever. By means of a piston exactly fitted to the mouth of the bore of the cylinder, through the middle of which piston the square iron bar, to the end of which the blunt steel borer was fixed, passed in a square hole made perfectly air-tight, the excess of the external air, to the inside of the bore of the cylinder, was effectually prevented. I did not find, however, by this experiment that the exclusion of the air diminished in the smallest degree the quantity of heat excited by the friction.

"There still remained one doubt, which, though it appeared to me to be so slight as hardly to deserve any attention, I was, however, desirous to remove. The piston which choked the mouth of the bore of the cylinder, in order that it might be air-tight, was fitted into it with so much nicety, by means of its collars of leather, and pressed against it with so much force, that, notwithstanding its being oiled, it occasioned a considerable degree of friction when the hollow cylinder was turned round its axis. Was not the heat produced, or at least some part of it, occasioned by this friction of the piston? and, as the external air had free access to the extremity of the bore, where it came into contact with the piston, is it not possible that this air may have had some share in the generation of the heat produced?

"A quadrangular oblong deal box, water-tight, being provided with holes or slits in the middle of each of its ends, just large enough to receive, the one the square iron rod to the end of which the blunt steel borer was fastened, the other the small cylindrical neck which joined the hollow cylinder to the cannon; when this box (which was occasionally closed above by a wooden cover or lid moving on hinges) was put into its place-- that is to say, when, by means of the two vertical

opening or slits in its two ends, the box was fixed to the machinery in such a manner that its bottom being in the plane of the horizon, its axis coincided with the axis of the hollow metallic cylinder, it is evident, from the description, that the hollow, metallic cylinder would occupy the middle of the box, without touching it on either side; and that, on pouring water into the box and filling it to the brim, the cylinder would be completely covered and surrounded on every side by that fluid. And, further, as the box was held fast by the strong, square iron rod which passed in a square hole in the centre of one of its ends, while the round or cylindrical neck which joined the hollow cylinder to the end of the cannon could turn round freely on its axis in the round hole in the centre of the other end of it, it is evident that the machinery could be put in motion without the least danger of forcing the box out of its place, throwing the water out of it, or deranging any part of the apparatus."

Everything being thus ready, the box was filled with cold water, having been made water-tight by means of leather collars, and the machinery put in motion. "The result of this beautiful experiment," says Rumford, "was very striking, and the pleasure it afforded me amply repaid me for all the trouble I had had in contriving and arranging the complicated machinery used in making it. The cylinder, revolving at the rate of thirty-two times in a minute, had been in motion but a short time when I perceived, by putting my hand into the water and touching the outside of the cylinder, that heat was generated, and it was not long before the water which surrounded the cylinder began to be sensibly warm.

"At the end of one hour I found, by plunging a thermometer into the box, . . . that its temperature had been raised no less than forty-seven degrees Fahrenheit, being now one hundred and seven degrees Fahrenheit. ... One hour and thirty minutes after the machinery had been put in motion the heat of the water in the box was one hundred and forty-two degrees. At the end of two hours ... it was raised to one hundred and seventy-eight degrees; and at two hours and thirty minutes it ACTUALLY BOILED!

"It would be difficult to describe the surprise and astonishment expressed in the countenances of the bystanders on seeing so large a quantity of cold water heated, and actually made to boil, without any fire. Though there was, in fact, nothing that could justly be considered as a surprise in this event, yet I acknowledge fairly that it afforded me a degree of childish pleasure which, were I ambitious of the reputation of a GRAVE PHILOSOPHER, I ought most certainly rather to hide than to discover...."

Having thus dwelt in detail on these experiments, Rumford comes now to the all-important discussion as to the significance of them--the subject that had been the source of so much speculation among the philosophers-- the question as to what heat really is, and if there really is any such thing (as many believed) as an igneous fluid, or a something called caloric.

"From whence came this heat which was continually given off in this manner, in the foregoing experiments?" asks Rumford. "Was it furnished by the small particles of metal detached from the larger solid masses on their being rubbed together? This, as we have already seen, could not possibly have been the case.

"Was it furnished by the air? This could not have been the case; for, in three of the experiments, the machinery being kept immersed in water, the access of the air of the atmosphere was completely prevented.

"Was it furnished by the water which surrounded the machinery? That this could not have been the case is evident: first, because this water was continually RECEIVING heat from the machinery, and could not, at the same time, be GIVING TO and RECEIVING HEAT FROM the same body; and, secondly, because there was no chemical decomposition of any part of this water. Had any such decomposition taken place (which, indeed, could not reasonably have been expected), one of its component elastic fluids (most probably hydrogen) must, at the same time, have been set at liberty, and, in making its escape into the atmosphere, would have been detected; but, though I frequently examined the water to see if any air-bubbles rose up through it, and had even made preparations for catching them if they should appear, I could perceive none; nor was there any sign of decomposition of any kind whatever, or other chemical process, going on in the water.

"Is it possible that the heat could have been supplied by means of the iron bar to the end of which the blunt steel borer was fixed? Or by the small neck of gun-metal by which the hollow cylinder was united to the cannon? These suppositions seem more improbable even than either of the before-mentioned; for heat was continually going off, or OUT OF THE MACHINERY, by both these passages during the whole time the experiment lasted.

"And in reasoning on this subject we must not forget to consider that most remarkable circumstance, that the source of the heat generated by friction in these experiments appeared evidently to be INEXHAUSTIBLE.

"It is hardly necessary to add that anything which any INSULATED body, or system of bodies, can continue to furnish WITHOUT LIMITATION cannot possibly be a MATERIAL substance; and it appears to me to be extremely difficult, if not quite impossible, to form any distinct idea of anything capable of being excited and communicated, in the manner the heat was excited and communicated in these experiments, except in MOTION."[1]

THOMAS YOUNG AND THE WAVE THEORY OF LIGHT

But contemporary judgment, while it listened respectfully to Rumford, was little minded to accept his verdict. The cherished beliefs of a generation are not to be put down with a single blow. Where many minds have a similar drift, however, the first blow may precipitate a general conflict; and so it was here. Young Humphry Davy had duplicated Rumford's experiments, and reached similar conclusions; and soon others fell into line. Then, in 1800, Dr. Thomas Young-- "Phenomenon Young" they called him at Cambridge, because he was reputed to know everything--took up the cudgels for the vibratory theory of light, and it began to be clear that the two "imponderables," heat and light, must stand or fall together; but no one as yet made a claim against the fluidity of electricity.

Before we take up the details of the assault made by Young upon the old doctrine of the materiality of light, we must pause to consider the personality of Young himself. For it chanced that this Quaker physician was one of those prodigies who come but few times in a century, and the full list

of whom in the records of history could be told on one's thumbs and fingers. His biographers tell us things about him that read like the most patent fairy-tales. As a mere infant in arms he had been able to read fluently. Before his fourth birthday came he had read the Bible twice through, as well as Watts's Hymns--poor child!--and when seven or eight he had shown a propensity to absorb languages much as other children absorb nursery tattle and Mother Goose rhymes. When he was fourteen, a young lady visiting the household of his tutor patronized the pretty boy by asking to see a specimen of his penmanship. The pretty boy complied readily enough, and mildly rebuked his interrogator by rapidly writing some sentences for her in fourteen languages, including such as, Arabian, Persian, and Ethiopic.

Meantime languages had been but an incident in the education of the lad. He seems to have entered every available field of thought--mathematics, physics, botany, literature, music, painting, languages, philosophy, archaeology, and so on to tiresome lengths--and once he had entered any field he seldom turned aside until he had reached the confines of the subject as then known and added something new from the recesses of his own genius. He was as versatile as Priestley, as profound as Newton himself. He had the range of a mere dilettante, but everywhere the full grasp of the master. He took early for his motto the saying that what one man has done, another man may do. Granting that the other man has the brain of a Thomas Young, it is a true motto.

Such, then, was the young Quaker who came to London to follow out the humdrum life of a practitioner of medicine in the year 1801. But incidentally the young physician was prevailed upon to occupy the interims of early practice by fulfilling the duties of the chair of Natural Philosophy at the Royal Institution, which Count Rumford had founded, and of which Davy was then Professor of Chemistry--the institution whose glories have been perpetuated by such names as Faraday and Tyndall, and which the Briton of to-day speaks of as the "Pantheon of Science." Here it was that Thomas Young made those studies which have insured him a niche in the temple of fame not far removed from that of Isaac Newton.

As early as 1793, when he was only twenty, Young had begun to Communicate papers to the Royal Society of London, which were adjudged worthy to be printed in full in the Philosophical Transactions; so it is not strange that he should have been asked to deliver the Bakerian lecture before that learned body the very first year after he came to London. The lecture was delivered November 12, 1801. Its subject was "The Theory of Light and Colors," and its reading marks an epoch in physical science; for here was brought forward for the first time convincing proof of that undulatory theory of light with which every student of modern physics is familiar--the theory which holds that light is not a corporeal entity, but a mere pulsation in the substance of an all-pervading ether, just as sound is a pulsation in the air, or in liquids or solids.

Young had, indeed, advocated this theory at an earlier date, but it was not until 1801 that he hit upon the idea which enabled him to bring it to anything approaching a demonstration. It was while pondering over the familiar but puzzling phenomena of colored rings into which white light is broken when reflected from thin films--Newton's rings, so called--that an explanation occurred to him which at once put the entire undulatory theory on a new footing. With that sagacity of insight which we call genius, he saw of a sudden that the phenomena could be explained by supposing that when rays of light fall on a thin glass, part of the rays being reflected from the upper surface, other rays, reflected from the lower surface, might be so retarded in their course through the glass that the

two sets would interfere with one another, the forward pulsation of one ray corresponding to the backward pulsation of another, thus quite neutralizing the effect. Some of the component pulsations of the light being thus effaced by mutual interference, the remaining rays would no longer give the optical effect of white light; hence the puzzling colors.

Here is Young's exposition of the subject:

Of the Colors of Thin Plates

"When a beam of light falls upon two refracting surfaces, the partial reflections coincide perfectly in direction; and in this case the interval of retardation taken between the surfaces is to their radius as twice the cosine of the angle of refraction to the radius.

"Let the medium between the surfaces be rarer than the surrounding mediums; then the impulse reflected at the second surface, meeting a subsequent undulation at the first, will render the particles of the rarer medium capable of wholly stopping the motion of the denser and destroying the reflection, while they themselves will be more strongly propelled than if they had been at rest, and the transmitted light will be increased. So that the colors by reflection will be destroyed, and those by transmission rendered more vivid, when the double thickness or intervals of retardation are any multiples of the whole breadth of the undulations; and at intermediate thicknesses the effects will be reversed according to the Newtonian observation.

"If the same proportions be found to hold good with respect to thin plates of a denser medium, which is, indeed, not improbable, it will be necessary to adopt the connected demonstrations of Prop. IV., but, at any rate, if a thin plate be interposed between a rarer and a denser medium, the colors by reflection and transmission may be expected to change places.

Of the Colors of Thick Plates

"When a beam of light passes through a refracting surface, especially if imperfectly polished, a portion of it is irregularly scattered, and makes the surface visible in all directions, but most conspicuously in directions not far distant from that of the light itself; and if a reflecting surface be placed parallel to the refracting surface, this scattered light, as well as the principal beam, will be reflected, and there will be also a new dissipation of light, at the return of the beam through the refracting surface. These two portions of scattered light will coincide in direction; and if the surfaces be of such a form as to collect the similar effects, will exhibit rings of colors. The interval of retardation is here the difference between the paths of the principal beam and of the scattered light between the two surfaces; of course, wherever the inclination of the scattered light is equal to that of the beam, although in different planes, the interval will vanish and all the undulations will conspire. At other inclinations, the interval will be the difference of the secants from the secant of the inclination, or angle of refraction of the principal beam. From these causes, all the colors of concave mirrors observed by Newton and others are necessary consequences; and it appears that their production, though somewhat similar, is by no means as Newton imagined, identical with the production of thin plates."[2]

By following up this clew with mathematical precision, measuring the exact thickness of the plate and the space between the different rings of color, Young was able to show mathematically what must be the length of pulsation for each of the different colors of the spectrum. He estimated that the undulations of red light, at the extreme lower end of the visible spectrum, must number about thirty-seven thousand six hundred and forty to the inch, and pass any given spot at a rate of four hundred and sixty-three millions of millions of undulations in a second, while the extreme violet numbers fifty-nine thousand seven hundred and fifty undulations to the inch, or seven hundred and thirty-five millions of millions to the second.

The Colors of Striated Surfaces

Young similarly examined the colors that are produced by scratches on a smooth surface, in particular testing the light from "Mr. Coventry's exquisite micrometers," which consist of lines scratched on glass at measured intervals. These microscopic tests brought the same results as the other experiments. The colors were produced at certain definite and measurable angles, and the theory of interference of undulations explained them perfectly, while, as Young affirmed with confidence, no other hypothesis hitherto advanced would explain them at all. Here are his words:

"Let there be in a given plane two reflecting points very near each other, and let the plane be so situated that the reflected image of a luminous object seen in it may appear to coincide with the points; then it is obvious that the length of the incident and reflected ray, taken together, is equal with respect to both points, considering them as capable of reflecting in all directions. Let one of the points be now depressed below the given plane; then the whole path of the light reflected from it will be lengthened by a line which is to the depression of the point as twice the cosine of incidence to the radius.

"If, therefore, equal undulations of given dimensions be reflected from two points, situated near enough to appear to the eye but as one, whenever this line is equal to half the breadth of a whole undulation the reflection from the depressed point will so interfere with the reflection from the fixed point that the progressive motion of the one will coincide with the retrograde motion of the other, and they will both be destroyed; but when this line is equal to the whole breadth of an undulation, the effect will be doubled, and when to a breadth and a half, again destroyed; and thus for a considerable number of alternations, and if the reflected undulations be of a different kind, they will be variously affected, according to their proportions to the various length of the line which is the difference between the lengths of their two paths, and which may be denominated the interval of a retardation.

"In order that the effect may be the more perceptible, a number of pairs of points must be united into two parallel lines; and if several such pairs of lines be placed near each other, they will facilitate the observation. If one of the lines be made to revolve round the other as an axis, the depression below the given plane will be as the sine of the inclination; and while the eye and the luminous object remain fixed the difference of the length of the paths will vary as this sine.

"The best subjects for the experiment are Mr. Coventry's exquisite micrometers; such of them as consist of parallel lines drawn on glass, at a distance of one- five-hundredth of an inch, are the most convenient. Each of these lines appears under a microscope to consist of two or more finer lines,

exactly parallel, and at a distance of somewhat more than a twentieth more than the adjacent lines. I placed one of these so as to reflect the sun's light at an angle of forty-five degrees, and fixed it in such a manner that while it revolved round one of the lines as an axis, I could measure its angular motion; I found that the longest red color occurred at the inclination 10 1/4 degrees, 20 3/4 degrees, 32 degrees, and 45 degrees; of which the sines are as the numbers 1, 2, 3, and 4. At all other angles also, when the sun's light was reflected from the surface, the color vanished with the inclination, and was equal at equal inclinations on either side.

This experiment affords a very strong confirmation of the theory. It is impossible to deduce any explanation of it from any hypothesis hitherto advanced; and I believe it would be difficult to invent any other that would account for it. There is a striking analogy between this separation of colors and the production of a musical note by successive echoes from equidistant iron palisades, which I have found to correspond pretty accurately with the known velocity of sound and the distances of the surfaces.

"It is not improbable that the colors of the integuments of some insects, and of some other natural bodies, exhibiting in different lights the most beautiful versatility, may be found to be of this description, and not to be derived from thin plates. In some cases a single scratch or furrow may produce similar effects, by the reflection of its opposite edges."[3]

This doctrine of interference of undulations was the absolutely novel part of Young's theory. The all- compassing genius of Robert Hooke had, indeed, very nearly apprehended it more than a century before, as Young himself points out, but no one else bad so much as vaguely conceived it; and even with the sagacious Hooke it was only a happy guess, never distinctly outlined in his own mind, and utterly ignored by all others. Young did not know of Hooke's guess until he himself had fully formulated the theory, but he hastened then to give his predecessor all the credit that could possibly be adjudged his due by the most disinterested observer. To Hooke's contemporary, Huygens, who was the originator of the general doctrine of undulation as the explanation of light, Young renders full justice also. For himself he claims only the merit of having demonstrated the theory which these and a few others of his predecessors had advocated without full proof.

The following year Dr. Young detailed before the Royal Society other experiments, which threw additional light on the doctrine of interference; and in 1803 he cited still others, which, he affirmed, brought the doctrine to complete demonstration. In applying this demonstration to the general theory of light, he made the striking suggestion that "the luminiferous ether pervades the substance of all material bodies with little or no resistance, as freely, perhaps, as the wind passes through a grove of trees." He asserted his belief also that the chemical rays which Ritter had discovered beyond the violet end of the visible spectrum are but still more rapid undulations of the same character as those which produce light. In his earlier lecture he had affirmed a like affinity between the light rays and the rays of radiant heat which Herschel detected below the red end of the spectrum, suggesting that "light differs from heat only in the frequency of its undulations or vibrations--those undulations which are within certain limits with respect to frequency affecting the optic nerve and constituting light, and those which are slower and probably stronger constituting heat only." From the very outset he had recognized the affinity between sound and light; indeed, it had been this affinity that led him on to an appreciation of the undulatory theory of light.

But while all these affinities seemed so clear to the great co-ordinating brain of Young, they made no such impression on the minds of his contemporaries. The immateriality of light had been substantially demonstrated, but practically no one save its author accepted the demonstration. Newton's doctrine of the emission of corpuscles was too firmly rooted to be readily dislodged, and Dr. Young had too many other interests to continue the assault unceasingly. He occasionally wrote something touching on his theory, mostly papers contributed to the Quarterly Review and similar periodicals, anonymously or under pseudonym, for he had conceived the notion that too great conspicuousness in fields outside of medicine would injure his practice as a physician. His views regarding light (including the original papers from the Philosophical Transactions of the Royal Society) were again given publicity in full in his celebrated volume on natural philosophy, consisting in part of his lectures before the Royal Institution, published in 1807; but even then they failed to bring conviction to the philosophic world. Indeed, they did not even arouse a controversial spirit, as his first papers had done.

ARAGO AND FRESNEL CHAMPION THE WAVE THEORY

So it chanced that when, in 1815, a young French military engineer, named Augustin Jean Fresnel, returning from the Napoleonic wars, became interested in the phenomena of light, and made some experiments concerning diffraction which seemed to him to controvert the accepted notions of the materiality of light, he was quite unaware that his experiments had been anticipated by a philosopher across the Channel. He communicated his experiments and results to the French Institute, supposing them to be absolutely novel. That body referred them to a committee, of which, as good fortune would have it, the dominating member was Dominique Francois Arago, a man as versatile as Young himself, and hardly less profound, if perhaps not quite so original. Arago at once recognized the merit of Fresnel's work, and soon became a convert to the theory. He told Fresnel that Young had anticipated him as regards the general theory, but that much remained to be done, and he offered to associate himself with Fresnel in prosecuting the investigation. Fresnel was not a little dashed to learn that his original ideas had been worked out by another while he was a lad, but he bowed gracefully to the situation and went ahead with unabated zeal.

The championship of Arago insured the undulatory theory a hearing before the French Institute, but by no means sufficed to bring about its general acceptance. On the contrary, a bitter feud ensued, in which Arago was opposed by the "Jupiter Olympus of the Academy," Laplace, by the only less famous Poisson, and by the younger but hardly less able Biot. So bitterly raged the feud that a life-long friendship between Arago and Biot was ruptured forever. The opposition managed to delay the publication of Fresnel's papers, but Arago continued to fight with his customary enthusiasm and pertinacity, and at last, in 1823, the Academy yielded, and voted Fresnel into its ranks, thus implicitly admitting the value of his work.

It is a humiliating thought that such controversies as this must mar the progress of scientific truth; but fortunately the story of the introduction of the undulatory theory has a more pleasant side. Three men, great both in character and in intellect, were concerned in pressing its claims--Young, Fresnel, and Arago--and the relations of these men form a picture unmarred by any of those petty jealousies that so often dim the lustre of great names. Fresnel freely acknowledged Young's priority so soon as his attention was called to it; and Young applauded the work of the Frenchman, and aided with his counsel in the application of the undulatory theory to the problems of polarization of

light, which still demanded explanation, and which Fresnel's fertility of experimental resource and profundity of mathematical insight sufficed in the end to conquer.

After Fresnel's admission to the Institute in 1823 the opposition weakened, and gradually the philosophers came to realize the merits of a theory which Young had vainly called to their attention a full quarter- century before. Now, thanks largely to Arago, both Young and Fresnel received their full meed of appreciation. Fresnel was given the Rumford medal of the Royal Society of England in 1825, and chosen one of the foreign members of the society two years later, while Young in turn was elected one of the eight foreign members of the French Academy. As a fitting culmination of the chapter of felicities between the three friends, it fell to the lot of Young, as Foreign Secretary of the Royal Society, to notify Fresnel of the honors shown him by England's representative body of scientists; while Arago, as Perpetual Secretary of the French Institute, conveyed to Young in the same year the notification that he had been similarly honored by the savants of France.

A few months later Fresnel was dead, and Young survived him only two years. Both died prematurely, but their great work was done, and the world will remember always and link together these two names in connection with a theory which in its implications and importance ranks little below the theory of universal gravitation.

VII. THE MODERN DEVELOPMENT OF ELECTRICITY AND MAGNETISM

GALVANI AND VOLTA

The full importance of Young's studies of light might perhaps have gained earlier recognition had it not chanced that, at the time when they were made, the attention of the philosophic world was turned with the fixity and fascination of a hypnotic stare upon another field, which for a time brooked no rival. How could the old, familiar phenomenon, light, interest any one when the new agent, galvanism, was in view? As well ask one to fix attention on a star while a meteorite blazes across the sky.

Galvanism was so called precisely as the Roentgen ray was christened at a later day--as a safe means of begging the question as to the nature of the phenomena involved. The initial fact in galvanism was the discovery of Luigi Galvani (1737-1798), a physician of Bologna, in 1791, that by bringing metals in contact with the nerves of a frog's leg violent muscular contractions are produced. As this simple little experiment led eventually to the discovery of galvanic electricity and the invention of the galvanic battery, it may be regarded as the beginning of modern electricity.

The story is told that Galvani was led to his discovery while preparing frogs' legs to make a broth for his invalid wife. As the story runs, he had removed the skins from several frogs' legs, when, happening to touch the exposed muscles with a scalpel which had lain in close proximity to an electrical machine, violent muscular action was produced. Impressed with this phenomenon, he began a series of experiments which finally resulted in his great discovery. But be this story authentic or not, it is certain that Galvani experimented for several years upon frogs' legs suspended upon wires and hooks, until he finally constructed his arc of two different metals, which, when

arranged so that one was placed in contact with a nerve and the other with a muscle, produced violent contractions.

These two pieces of metal form the basic principle of the modern galvanic battery, and led directly to Alessandro Volta's invention of his "voltaic pile," the immediate ancestor of the modern galvanic battery. Volta's experiments were carried on at the same time as those of Galvani, and his invention of his pile followed close upon Galvani's discovery of the new form of electricity. From these facts the new form of electricity was sometimes called "galvanic" and sometimes "voltaic" electricity, but in recent years the term "galvanism" and "galvanic current" have almost entirely supplanted the use of the term voltaic.

It was Volta who made the report of Galvani's wonderful discovery to the Royal Society of London, read on January 31, 1793. In this letter he describes Galvani's experiments in detail and refers to them in glowing terms of praise. He calls it one of the "most beautiful and important discoveries," and regarded it as the germ or foundation upon which other discoveries were to be made. The prediction proved entirely correct, Volta himself being the chief discoverer.

Working along lines suggested by Galvani's discovery, Volta constructed an apparatus made up of a number of disks of two different kinds of metal, such as tin and silver, arranged alternately, a piece of some moist, porous substance, like paper or felt, being interposed between each pair of disks. With this "pile," as it was called, electricity was generated, and by linking together several such piles an electric battery could be formed.

This invention took the world by storm. Nothing like the enthusiasm it created in the philosophic world had been known since the invention of the Leyden jar, more than half a century before. Within a few weeks after Volta's announcement, batteries made according to his plan were being experimented with in every important laboratory in Europe.

As the century closed, half the philosophic world was speculating as to whether "galvanic influence" were a new imponderable, or only a form of electricity; and the other half was eagerly seeking to discover what new marvels the battery might reveal. The least imaginative man could see that here was an invention that would be epoch-making, but the most visionary dreamer could not even vaguely adumbrate the real measure of its importance.

It was evident at once that almost any form of galvanic battery, despite imperfections, was a more satisfactory instrument for generating electricity than the frictional machine hitherto in use, the advantage lying in the fact that the current from the galvanic battery could be controlled practically at will, and that the apparatus itself was inexpensive and required comparatively little attention. These advantages were soon made apparent by the practical application of the electric current in several fields.

It will be recalled that despite the energetic endeavors of such philosophers as Watson, Franklin, Galvani, and many others, the field of practical application of electricity was very limited at the close of the eighteenth century. The lightning-rod had come into general use, to be sure, and its value as an invention can hardly be overestimated. But while it was the result of extensive electrical discoveries, and is a most practical instrument, it can hardly be called one that puts electricity to

practical use, but simply acts as a means of warding off the evil effects of a natural manifestation of electricity. The invention, however, had all the effects of a mechanism which turned electricity to practical account. But with the advent of the new kind of electricity the age of practical application began.

DAVY AND ELECTRIC LIGHT

Volta's announcement of his pile was scarcely two months old when two Englishmen, Messrs. Nicholson and Carlisle, made the discovery that the current from the galvanic battery had a decided effect upon certain chemicals, among other things decomposing water into its elements, hydrogen and oxygen. On May 7, 1800, these investigators arranged the ends of two brass wires connected with the poles of a voltaic pile, composed of alternate silver and zinc plates, so that the current coming from the pile was discharged through a small quantity of "New River water." "A fine stream of minute bubbles immediately began to flow from the point of the lower wire in the tube which communicated with the silver," wrote Nicholson, "and the opposite point of the upper wire became tarnished, first deep orange and then black. . . ." The product of gas during two hours and a half was two- thirtieths of a cubic inch. "It was then mixed with an equal quantity of common air," continues Nicholson, "and exploded by the application of a lighted waxen thread."

This demonstration was the beginning of the very important science of electro-chemistry.

The importance of this discovery was at once recognized by Sir Humphry Davy, who began experimenting immediately in this new field. He constructed a series of batteries in various combinations, with which he attacked the "fixed alkalies," the composition of which was then unknown. Very shortly he was able to decompose potash into bright metallic globules, resembling quicksilver. This new substance he named "potassium." Then in rapid succession the elementary substances sodium, calcium, strontium, and magnesium were isolated.

It was soon discovered, also, that the new electricity, like the old, possessed heating power under certain conditions, even to the fusing of pieces of wire. This observation was probably first made by Frommsdorff, but it was elaborated by Davy, who constructed a battery of two thousand cells with which he produced a bright light from points of carbon--the prototype of the modern arc lamp. He made this demonstration before the members of the Royal Institution in 1810. But the practical utility of such a light for illuminating purposes was still a thing of the future. The expense of constructing and maintaining such an elaborate battery, and the rapid internal destruction of its plates, together with the constant polarization, rendered its use in practical illumination out of the question. It was not until another method of generating electricity was discovered that Davy's demonstration could be turned to practical account.

In Davy's own account of his experiment he says:

"When pieces of charcoal about an inch long and one-sixth of an inch in diameter were brought near each other (within the thirtieth or fortieth of an inch), a bright spark was produced, and more than half the volume of the charcoal became ignited to whiteness; and, by withdrawing the points from each other, a constant discharge took place through the heated air, in a space equal to at least four inches, producing a most brilliant ascending arch of light, broad and conical in form in the

middle. When any substance was introduced into this arch, it instantly became ignited; platina melted as readily in it as wax in a common candle; quartz, the sapphire, magnesia, lime, all entered into fusion; fragments of diamond and points of charcoal and plumbago seemed to evaporate in it, even when the connection was made in the receiver of an air-pump; but there was no evidence of their having previously undergone fusion. When the communication between the points positively and negatively electrified was made in the air rarefied in the receiver of the air-pump, the distance at which the discharge took place increased as the exhaustion was made; and when the atmosphere in the vessel supported only one- fourth of an inch of mercury in the barometrical gauge, the sparks passed through a space of nearly half an inch; and, by withdrawing the points from each other, the discharge was made through six or seven inches, producing a most brilliant coruscation of purple light; the charcoal became intensely ignited, and some platina wire attached to it fused with brilliant scintillations and fell in large globules upon the plate of the pump. All the phenomena of chemical decomposition were produced with intense rapidity by this combination."[1]

But this experiment demonstrated another thing besides the possibility of producing electric light and chemical decomposition, this being the heating power capable of being produced by the electric current. Thus Davy's experiment of fusing substances laid the foundation of the modern electric furnaces, which are of paramount importance in several great commercial industries.

While some of the results obtained with Davy's batteries were practically as satisfactory as could be obtained with modern cell batteries, the batteries themselves were anything but satisfactory. They were expensive, required constant care and attention, and, what was more important from an experimental standpoint at least, were not constant in their action except for a very limited period of time, the current soon "running down." Numerous experimenters, therefore, set about devising a satisfactory battery, and when, in 1836, John Frederick Daniell produced the cell that bears his name, his invention was epoch- making in the history of electrical progress. The Royal Society considered it of sufficient importance to bestow the Copley medal upon the inventor, whose device is the direct parent of all modern galvanic cells. From the time of the advent of the Daniell cell experiments in electricity were rendered comparatively easy. In the mean while, however, another great discovery was made.

ELECTRICITY AND MAGNETISM

For many years there had been a growing suspicion, amounting in many instances to belief in the close relationship existing between electricity and magnetism. Before the winter of 1815, however, it was a belief that was surmised but not demonstrated. But in that year it occurred to Jean Christian Oersted, of Denmark, to pass a current of electricity through a wire held parallel with, but not quite touching, a suspended magnetic needle. The needle was instantly deflected and swung out of its position.

"The first experiments in connection with the subject which I am undertaking to explain," wrote Oersted, "were made during the course of lectures which I held last winter on electricity and magnetism. From those experiments it appeared that the magnetic needle could be moved from its position by means of a galvanic battery--one with a closed galvanic circuit. Since, however, those experiments were made with an apparatus of small power, I undertook to repeat and increase them with a large galvanic battery.

"Let us suppose that the two opposite ends of the galvanic apparatus are joined by a metal wire. This I shall always call the conductor for the sake of brevity. Place a rectilinear piece of this conductor in a horizontal position over an ordinary magnetic needle so that it is parallel to it. The magnetic needle will be set in motion and will deviate towards the west under that part of the conductor which comes from the negative pole of the galvanic battery. If the wire is not more than four-fifths of an inch distant from the middle of this needle, this deviation will be about forty-five degrees. At a greater distance the angle of deviation becomes less. Moreover, the deviation varies according to the strength of the battery. The conductor can be moved towards the east or west, so long as it remains parallel to the needle, without producing any other result than to make the deviation smaller.

"The conductor can consist of several combined wires or metal coils. The nature of the metal does not alter the result except, perhaps, to make it greater or less. We have used wires of platinum, gold, silver, brass, and iron, and coils of lead, tin, and quicksilver with the same result. If the conductor is interrupted by water, all effect is not cut off, unless the stretch of water is several inches long.

"The conductor works on the magnetic needle through glass, metals, wood, water, and resin, through clay vessels and through stone, for when we placed a glass plate, a metal plate, or a board between the conductor and the needle the effect was not cut off; even the three together seemed hardly to weaken the effect, and the same was the case with an earthen vessel, even when it was full of water. Our experiments also demonstrated that the said effects were not altered when we used a magnetic needle which was in a brass case full of water.

"When the conductor is placed in a horizontal plane under the magnetic needle all the effects we have described take place in precisely the same way, but in the opposite direction to what took place when the conductor was in a horizontal plane above the needle.

"If the conductor is moved in a horizontal plane so that it gradually makes ever-increasing angles with the magnetic meridian, the deviation of the magnetic needle from the magnetic meridian is increased when the wire is turned towards the place of the needle; it decreases, on the other hand, when it is turned away from that place.

"A needle of brass which is hung in the same way as the magnetic needle is not set in motion by the influence of the conductor. A needle of glass or rubber likewise remains static under similar experiments. Hence the electrical conductor affects only the magnetic parts of a substance. That the electrical current is not confined to the conducting wire, but is comparatively widely diffused in the surrounding space, is sufficiently demonstrated from the foregoing observations."[2]

The effect of Oersted's demonstration is almost incomprehensible. By it was shown the close relationship between magnetism and electricity. It showed the way to the establishment of the science of electrodynamics; although it was by the French savant Andre Marie Ampere (1775-1836) that the science was actually created, and this within the space of one week after hearing of Oersted's experiment in deflecting the needle. Ampere first received the news of Oersted's experiment on September 11, 1820, and on the 18th of the same month he announced to the

Academy the fundamental principles of the science of electro-dynamics-- seven days of rapid progress perhaps unequalled in the history of science.

Ampere's distinguished countryman, Arago, a few months later, gave the finishing touches to Oersted's and Ampere's discoveries, by demonstrating conclusively that electricity not only influenced a magnet, but actually produced magnetism under proper circumstances --a complemental fact most essential in practical mechanics

Some four years after Arago's discovery, Sturgeon made the first "electro-magnet" by winding a soft iron core with wire through which a current of electricity was passed. This study of electro-magnets was taken up by Professor Joseph Henry, of Albany, New York, who succeeded in making magnets of enormous lifting power by winding the iron core with several coils of wire. One of these magnets, excited by a single galvanic cell of less than half a square foot of surface, and containing only half a pint of dilute acids, sustained a weight of six hundred and fifty pounds.

Thus by Oersted's great discovery of the intimate relationship of magnetism and electricity, with further elaborations and discoveries by Ampere, Volta, and Henry, and with the invention of Daniell's cell, the way was laid for putting electricity to practical use. Soon followed the invention and perfection of the electro-magnetic telegraph and a host of other but little less important devices.

FARADAY AND ELECTRO-MAGNETIC INDUCTION

With these great discoveries and inventions at hand, electricity became no longer a toy or a "plaything for philosophers," but of enormous and growing importance commercially. Still, electricity generated by chemical action, even in a very perfect cell, was both feeble and expensive, and, withal, only applicable in a comparatively limited field. Another important scientific discovery was necessary before such things as electric traction and electric lighting on a large scale were to become possible; but that discovery was soon made by Sir Michael Faraday.

Faraday, the son of a blacksmith and a bookbinder by trade, had interested Sir Humphry Davy by his admirable notes on four of Davy's lectures, which he had been able to attend. Although advised by the great scientist to "stick to his bookbinding" rather than enter the field of science, Faraday became, at twenty-two years of age, Davy's assistant in the Royal Institution. There, for several years, he devoted all his spare hours to scientific investigations and experiments, perfecting himself in scientific technique.

A few years later he became interested, like all the scientists of the time, in Arago's experiment of rotating a copper disk underneath a suspended compass- needle. When this disk was rotated rapidly, the needle was deflected, or even rotated about its axis, in a manner quite inexplicable. Faraday at once conceived the idea that the cause of this rotation was due to electricity, induced in the revolving disk--not only conceived it, but put his belief in writing. For several years, however, he was unable to demonstrate the truth of his assumption, although he made repeated experiments to prove it. But in 1831 he began a series of experiments that established forever the fact of electro-magnetic induction.

In his famous paper, read before the Royal Society in 1831, Faraday describes the method by which he first demonstrated electro-magnetic induction, and then explained the phenomenon of Arago's revolving disk.

"About twenty-six feet of copper wire, one-twentieth of an inch in diameter, were wound round a cylinder of wood as a helix," he said, "the different spires of which were prevented from touching by a thin interposed twine. This helix was covered with calico, and then a second wire applied in the same manner. In this way twelve helices were "superposed, each containing an average length of wire of twenty-seven feet, and all in the same direction. The first, third, fifth, seventh, ninth, and eleventh of these helices were connected at their extremities end to end so as to form one helix; the others were connected in a similar manner; and thus two principal helices were produced, closely interposed, having the same direction, not touching anywhere, and each containing one hundred and fifty-five feet in length of wire.

One of these helices was connected with a galvanometer, the other with a voltaic battery of ten pairs of plates four inches square, with double coppers and well charged; yet not the slightest sensible deflection of the galvanometer needle could be observed.

"A similar compound helix, consisting of six lengths of copper and six of soft iron wire, was constructed. The resulting iron helix contained two hundred and eight feet; but whether the current from the trough was passed through the copper or the iron helix, no effect upon the other could be perceived at the galvanometer.

"In these and many similar experiments no difference in action of any kind appeared between iron and other metals.

"Two hundred and three feet of copper wire in one length were passed round a large block of wood; other two hundred and three feet of similar wire were interposed as a spiral between the turns of the first, and metallic contact everywhere prevented by twine. One of these helices was connected with a galvanometer and the other with a battery of a hundred pairs of plates four inches square, with double coppers and well charged. When the contact was made, there was a sudden and very slight effect at the galvanometer, and there was also a similar slight effect when the contact with the battery was broken. But whilst the voltaic current was continuing to pass through the one helix, no galvanometrical appearances of any effect like induction upon the other helix could be perceived, although the active power of the battery was proved to be great by its heating the whole of its own helix, and by the brilliancy of the discharge when made through charcoal.

"Repetition of the experiments with a battery of one hundred and twenty pairs of plates produced no other effects; but it was ascertained, both at this and at the former time, that the slight deflection of the needle occurring at the moment of completing the connection was always in one direction, and that the equally slight deflection produced when the contact was broken was in the other direction; and, also, that these effects occurred when the first helices were used.

"The results which I had by this time obtained with magnets led me to believe that the battery current through one wire did, in reality, induce a similar current through the other wire, but that it continued for an instant only, and partook more of the nature of the electrical wave passed through

from the shock of a common Leyden jar than of that from a voltaic battery, and, therefore, might magnetize a steel needle although it scarcely affected the galvanometer.

"This expectation was confirmed; for on substituting a small hollow helix, formed round a glass tube, for the galvanometer, introducing a steel needle, making contact as before between the battery and the inducing wire, and then removing the needle before the battery contact was broken, it was found magnetized.

"When the battery contact was first made, then an unmagnetized needle introduced, and lastly the battery contact broken, the needle was found magnetized to an equal degree apparently with the first; but the poles were of the contrary kinds."[3]

To Faraday these experiments explained the phenomenon of Arago's rotating disk, the disk inducing the current from the magnet, and, in reacting, deflecting the needle. To prove this, he constructed a disk that revolved between the poles of an electro-magnet, connecting the axis and the edge of the disk with a galvanometer. ". . . A disk of copper, twelve inches in diameter, fixed upon a brass axis," he says, "was mounted in frames so as to be revolved either vertically or horizontally, its edge being at the same time introduced more or less between the magnetic poles. The edge of the plate was well amalgamated for the purpose of obtaining good but movable contact; a part round the axis was also prepared in a similar manner.

"Conductors or collectors of copper and lead were constructed so as to come in contact with the edge of the copper disk, or with other forms of plates hereafter to be described. These conductors we're about four inches long, one-third of an inch wide, and one-fifth of an inch thick; one end of each was slightly grooved, to allow of more exact adaptation to the somewhat convex edge of the plates, and then amalgamated. Copper wires, one-sixteenth of an inch in thickness, attached in the ordinary manner by convolutions to the other ends of these conductors, passed away to the galvanometer.

"All these arrangements being made, the copper disk was adjusted, the small magnetic poles being about one-half an inch apart, and the edge of the plate inserted about half their width between them. One of the galvanometer wires was passed twice or thrice loosely round the brass axis of the plate, and the other attached to a conductor, which itself was retained by the hand in contact with the amalgamated edge of the disk at the part immediately between the magnetic poles. Under these circumstances all was quiescent, and the galvanometer exhibited no effect. But the instant the plate moved the galvanometer was influenced, and by revolving the plate quickly the needle could be deflected ninety degrees or more."[4]

This rotating disk was really a dynamo electric machine in miniature, the first ever constructed, but whose direct descendants are the ordinary dynamos. Modern dynamos range in power from little machines operating machinery requiring only fractions of a horsepower to great dynamos operating street-car lines and lighting cities; but all are built on the same principle as Faraday's rotating disk. By this discovery the use of electricity as a practical and economical motive power became possible.

STORAGE BATTERIES

When the discoveries of Faraday of electro-magnetic induction had made possible the means of easily generating electricity, the next natural step was to find a means of storing it or accumulating it. This, however, proved no easy matter, and as yet a practical storage or secondary battery that is neither too cumbersome, too fragile, nor too weak in its action has not been invented. If a satisfactory storage battery could be made, it is obvious that its revolutionary effects could scarcely be overestimated. In the single field of aeronautics, it would probably solve the question of aerial navigation. Little wonder, then, that inventors have sought so eagerly for the invention of satisfactory storage batteries. As early as 1803 Ritter had attempted to make such a secondary battery. In 1843 Grove also attempted it. But it was not until 1859, when Gaston Planche produced his invention, that anything like a reasonably satisfactory storage battery was made. Planche discovered that sheets of lead immersed in dilute sulphuric acid were very satisfactory for the production of polarization effects. He constructed a battery of sheets of lead immersed in sulphuric acid, and, after charging these for several hours from the cells of an ordinary Bunsen battery, was able to get currents of great strength and considerable duration. This battery, however, from its construction of lead, was necessarily heavy and cumbersome. Faure improved it somewhat by coating the lead plates with red-lead, thus increasing the capacity of the cell. Faure's invention gave a fresh impetus to inventors, and shortly after the market was filled with storage batteries of various kinds, most of them modifications of Planche's or Faure's. The ardor of enthusiastic inventors soon flagged, however, for all these storage batteries proved of little practical account in the end, as compared with other known methods of generating power.

Three methods of generating electricity are in general use: static or frictional electricity is generated by "plate" or "static" machines; galvanic, generated by batteries based on Volta's discovery; and induced, or faradic, generated either by chemical or mechanical action. There is still another kind, thermo-electricity, that may be generated in a most simple manner. In 1821 Seebecle, of Berlin, discovered that when a circuit was formed of two wires of different metals, if there be a difference in temperature at the juncture of these two metals an electrical current will be established. In this way heat may be transmitted directly into the energy of the current without the interposition of the steam-engine. Batteries constructed in this way are of low resistance, however, although by arranging several of them in "series," currents of considerable strength can be generated. As yet, however, they are of little practical importance.

About the middle of the century Clerk-Maxwell advanced the idea that light waves were really electro- magnetic waves. If this were true and light proved to be simply one form of electrical energy, then the same would be true of radiant heat. Maxwell advanced this theory, but failed to substantiate it by experimental confirmation. But Dr. Heinrich Hertz, a few years later, by a series of experiments, demonstrated the correctness of Maxwell's surmises. What are now called "Hertzian waves" are waves apparently identical with light waves, but of much lower pitch or period. In his experiments Hertz showed that, under proper conditions, electric sparks between polished balls were attended by ether waves of the same nature as those of light, but of a pitch of several millions of vibrations per second. These waves could be dealt with as if they were light waves--reflected, refracted, and polarized. These are the waves that are utilized in wireless telegraphy.

ROENTGEN RAYS, OR X-RAYS

In December of 1895 word came out of Germany of a scientific discovery that startled the world. It came first as a rumor, little credited; then as a pronounced report; at last as a demonstration. It told of a new manifestation of energy, in virtue of which the interior of opaque objects is made visible to human eyes. One had only to look into a tube containing a screen of a certain composition, and directed towards a peculiar electrical apparatus, to acquire clairvoyant vision more wonderful than the discredited second-sight of the medium. Coins within a purse, nails driven into wood, spectacles within a leather case, became clearly visible when subjected to the influence of this magic tube; and when a human hand was held before the tube, its bones stood revealed in weird simplicity, as if the living, palpitating flesh about them were but the shadowy substance of a ghost.

Not only could the human eye see these astounding revelations, but the impartial evidence of inanimate chemicals could be brought forward to prove that the mind harbored no illusion. The photographic film recorded the things that the eye might see, and ghostly pictures galore soon gave a quietus to the doubts of the most sceptical. Within a month of the announcement of Professor Roentgen's experiments comment upon the "X-ray" and the "new photography" had become a part of the current gossip of all Christendom.

It is hardly necessary to say that such a revolutionary thing as the discovery of a process whereby opaque objects became transparent, or translucent, was not achieved at a single bound with no intermediate discoveries. In 1859 the German physicist Julius Plucker (1801-1868) noticed that when there was an electrical discharge through an exhausted tube at a low pressure, on the surrounding walls of the tube near the negative pole, or cathode, appeared a greenish phosphorescence. This discovery was soon being investigated by a number of other scientists, among others Hittorf, Goldstein, and Professor (now Sir William) Crookes. The explanations given of this phenomenon by Professor Crookes concern us here more particularly, inasmuch as his views did not accord exactly with those held by the other two scientists, and as his researches were more directly concerned in the discovery of the Roentgen rays. He held that the heat and phosphorescence produced in a low-pressure tube were caused by streams of particles, projected from the cathode with great velocity, striking the sides of the glass tube. The composition of the glass seemed to enter into this phosphorescence also, for while lead glass produced blue phosphorescence, soda glass produced a yellowish green. The composition of the glass seemed to be changed by a long-continued pelting of these particles, the phosphorescence after a time losing its initial brilliancy, caused by the glass becoming "tired," as Professor Crookes said. Thus when some opaque substance, such as iron, is placed between the cathode and the sides of the glass tube so that it casts a shadow in a certain spot on the glass for some little time, it is found on removing the opaque substance or changing its position that the area of glass at first covered by the shadow now responded to the rays in a different manner from the surrounding glass.

The peculiar ray's, now known as the cathode rays, not only cast a shadow, but are deflected by a magnet, so that the position of the phosphorescence on the sides of the tube may be altered by the proximity of a powerful magnet. From this it would seem that the rays are composed of particles charged with negative electricity, and Professor J. J. Thomson has modified the experiment of Perrin to show that negative electricity is actually associated with the rays. There is reason for believing, therefore, that the cathode rays are rapidly moving charges of negative electricity. It is

possible, also, to determine the velocity at which these particles are moving by measuring the deflection produced by the magnetic field.

From the fact that opaque substances cast a shadow in these rays it was thought at first that all solids were absolutely opaque to them. Hertz, however, discovered that a small amount of phosphorescence occurred on the glass even when such opaque substances as gold-leaf or aluminium foil were interposed between the cathode and the sides of the tube. Shortly afterwards Lenard discovered that the cathode rays can be made to pass from the inside of a discharge tube to the outside air. For convenience these rays outside the tube have since been known as "Lenard rays."

In the closing days of December, 1895, Professor Wilhelm Konrad Roentgen, of Wurzburg, announced that he had made the discovery of the remarkable effect arising from the cathode rays to which reference was made above. He found that if a plate covered with a phosphorescent substance is placed near a discharge tube exhausted so highly that the cathode rays produced a green phosphorescence, this plate is made to glow in a peculiar manner. The rays producing this glow were not the cathode rays, although apparently arising from them, and are what have since been called the Roentgen rays, or X-rays.

Roentgen found that a shadow is thrown upon the screen by substances held between it and the exhausted tube, the character of the shadow depending upon the density of the substance. Thus metals are almost completely opaque to the rays; such substances as bone much less so, and ordinary flesh hardly so at all. If a coin were held in the hand that had been interposed between the tube and the screen the picture formed showed the coin as a black shadow; and the bones of the hand, while casting a distinct shadow, showed distinctly lighter; while the soft tissues produced scarcely any shadow at all. The value of such a discovery was obvious from the first; and was still further enhanced by the discovery made shortly that, photographic plates are affected by the rays, thus making it possible to make permanent photographic records of pictures through what we know as opaque substances.

What adds materially to the practical value of Roentgen's discovery is the fact that the apparatus for producing the X-rays is now so simple and relatively inexpensive that it is within the reach even of amateur scientists. It consists essentially of an induction coil attached either to cells or a street-current plug for generating the electricity, a focus tube, and a phosphorescence screen. These focus tubes are made in various shapes, but perhaps the most popular are in the form of a glass globe, not unlike an ordinary small-sized water-bottle, this tube being closed and exhausted, and having the two poles (anode and cathode) sealed into the glass walls, but protruding at either end for attachment to the conducting wires from the induction coil. This tube may be mounted on a stand at a height convenient for manipulation. The phosphorescence screen is usually a plate covered with some platino-cyanide and mounted in the end of a box of convenient size, the opposite end of which is so shaped that it fits the contour of the face, shutting out the light and allowing the eyes of the observer to focalize on the screen at the end. For making observations the operator has simply to turn on the current of electricity and apply the screen to his eyes, pointing it towards the glowing tube, when the shadow of any substance interposed between the tube and the screen will appear upon the phosphorescence plate.

The wonderful shadow pictures produced on the phosphorescence screen, or the photographic plate, would seem to come from some peculiar form of light, but the exact nature of these rays is still an open question. Whether the Roentgen rays are really a form of light--that is, a form of "electro-magnetic disturbance propagated through ether," is not fully determined. Numerous experiments have been undertaken to determine this, but as yet no proof has been found that the rays are a form of light, although there appears to be nothing in their properties inconsistent with their being so. For the moment most investigators are content to admit that the term X-ray virtually begs the question as to the intimate nature of the form of energy involved.

VIII. THE CONSERVATION OF ENERGY

As we have seen, it was in 1831 that Faraday opened up the field of magneto-electricity. Reversing the experiments of his predecessors, who had found that electric currents may generate magnetism, he showed that magnets have power under certain circumstances to generate electricity; he proved, indeed, the interconvertibility of electricity and magnetism. Then he showed that all bodies are more or less subject to the influence of magnetism, and that even light may be affected by magnetism as to its phenomena of polarization. He satisfied himself completely of the true identity of all the various forms of electricity, and of the convertibility of electricity and chemical action. Thus he linked together light, chemical affinity, magnetism, and electricity. And, moreover, he knew full well that no one of these can be produced in indefinite supply from another. "Nowhere," he says, "is there a pure creation or production of power without a corresponding exhaustion of something to supply it."

When Faraday wrote those words in 1840 he was treading on the very heels of a greater generalization than any which he actually formulated; nay, he had it fairly within his reach. He saw a great truth without fully realizing its import; it was left for others, approaching the same truth along another path, to point out its full significance.

The great generalization which Faraday so narrowly missed is the truth which since then has become familiar as the doctrine of the conservation of energy--the law that in transforming energy from one condition to another we can never secure more than an equivalent quantity; that, in short, "to create or annihilate energy is as impossible as to create or annihilate matter; and that all the phenomena of the material universe consist in transformations of energy alone." Some philosophers think this the greatest generalization ever conceived by the mind of man. Be that as it may, it is surely one of the great intellectual landmarks of the nineteenth century. It stands apart, so stupendous and so far-reaching in its implications that the generation which first saw the law developed could little appreciate it; only now, through the vista of half a century, do we begin to see it in its true proportions.

A vast generalization such as this is never a mushroom growth, nor does it usually spring full grown from the mind of any single man. Always a number of minds are very near a truth before any one mind fully grasps it. Pre-eminently true is this of the doctrine of the conservation of energy. Not Faraday alone, but half a dozen different men had an inkling of it before it gained full expression; indeed, every man who advocated the undulatory theory of light and heat was verging

towards the goal. The doctrine of Young and Fresnel was as a highway leading surely on to the wide plain of conservation. The phenomena of electro- magnetism furnished another such highway. But there was yet another road which led just as surely and even more readily to the same goal. This was the road furnished by the phenomena of heat, and the men who travelled it were destined to outstrip their fellow-workers; though, as we have seen, wayfarers on other roads were within hailing distance when the leaders passed the mark.

In order to do even approximate justice to the men who entered into the great achievement, we must recall that just at the close of the eighteenth century Count Rumford and Humphry Davy independently showed that labor may be transformed into heat; and correctly interpreted this fact as meaning the transformation of molar into molecular motion. We can hardly doubt that each of these men of genius realized--vaguely, at any rate--that there must be a close correspondence between the amount of the molar and the molecular motions; hence that each of them was in sight of the law of the mechanical equivalent of heat. But neither of them quite grasped or explicitly stated what each must vaguely have seen; and for just a quarter of a century no one else even came abreast their line of thought, let alone passing it.

But then, in 1824, a French philosopher, Sadi Carnot, caught step with the great Englishmen, and took a long leap ahead by explicitly stating his belief that a definite quantity of work could be transformed into a definite quantity of heat, no more, no less. Carnot did not, indeed, reach the clear view of his predecessors as to the nature of heat, for he still thought it a form of "imponderable" fluid; but he reasoned none the less clearly as to its mutual convertibility with mechanical work. But important as his conclusions seem now that we look back upon them with clearer vision, they made no impression whatever upon his contemporaries. Carnot's work in this line was an isolated phenomenon of historical interest, but it did not enter into the scheme of the completed narrative in any such way as did the work of Rumford and Davy.

The man who really took up the broken thread where Rumford and Davy had dropped it, and wove it into a completed texture, came upon the scene in 1840. His home was in Manchester, England; his occupation that of a manufacturer. He was a friend and pupil of the great Dr. Dalton. His name was James Prescott Joule. When posterity has done its final juggling with the names of the nineteenth century, it is not unlikely that the name of this Manchester philosopher will be a household word, like the names of Aristotle, Copernicus, and Newton.

For Joule's work it was, done in the fifth decade of the century, which demonstrated beyond all cavil that there is a precise and absolute equivalence between mechanical work and heat; that whatever the form of manifestation of molar motion, it can generate a definite and measurable amount of heat, and no more. Joule found, for example, that at the sea-level in Manchester a pound weight falling through seven hundred and seventy-two feet could generate enough heat to raise the temperature of a pound of water one degree Fahrenheit. There was nothing haphazard, nothing accidental, about this; it bore the stamp of unalterable law. And Joule himself saw, what others in time were made to see, that this truth is merely a particular case within a more general law. If heat cannot be in any sense created, but only made manifest as a transformation of another kind of motion, then must not the same thing be true of all those other forms of "force"--light, electricity, magnetism--which had been shown to be so closely associated, so mutually convertible, with heat? All analogy seemed to urge the truth of this inference; all experiment tended to confirm it. The law

of the mechanical equivalent of heat then became the main corner-stone of the greater law of the conservation of energy.

But while this citation is fresh in mind, we must turn our attention with all haste to a country across the Channel--to Denmark, in short--and learn that even as Joule experimented with the transformation of heat, a philosopher of Copenhagen, Colding by name, had hit upon the same idea, and carried it far towards a demonstration. And then, without pausing, we must shift yet again, this time to Germany, and consider the work of three other men, who independently were on the track of the same truth, and two of whom, it must be admitted, reached it earlier than either Joule or Colding, if neither brought it to quite so clear a demonstration. The names of these three Germans are Mohr, Mayer, and Helmholtz. Their share in establishing the great doctrine of conservation must now claim our attention.

As to Karl Friedrich Mohr, it may be said that his statement of the doctrine preceded that of any of his fellows, yet that otherwise it was perhaps least important. In 1837 this thoughtful German had grasped the main truth, and given it expression in an article published in the Zeitschrift fur Physik, etc. But the article attracted no attention whatever, even from Mohr's own countrymen. Still, Mohr's title to rank as one who independently conceived the great truth, and perhaps conceived it before any other man in the world saw it as clearly, even though he did not demonstrate its validity, is not to be disputed.

It was just five years later, in 1842, that Dr. Julius Robert Mayer, practising physician in the little German town of Heilbronn, published a paper in Liebig's Annalen on "The Forces of Inorganic Nature," in which not merely the mechanical theory of heat, but the entire doctrine of the conservation of energy, is explicitly if briefly stated. Two years earlier Dr. Mayer, while surgeon to a Dutch India vessel cruising in the tropics, had observed that the venous blood of a patient seemed redder than venous blood usually is observed to be in temperate climates. He pondered over this seemingly insignificant fact, and at last reached the conclusion that the cause must be the lesser amount of oxidation required to keep up the body temperature in the tropics. Led by this reflection to consider the body as a machine dependent on outside forces for its capacity to act, he passed on into a novel realm of thought, which brought him at last to independent discovery of the mechanical theory of heat, and to the first full and comprehensive appreciation of the great law of conservation. Blood-letting, the modern physician holds, was a practice of very doubtful benefit, as a rule, to the subject; but once, at least, it led to marvellous results. No straw is go small that

it may not point the receptive mind of genius to new and wonderful truths.

MAYER'S PAPER OF 1842

The paper in which Mayer first gave expression to his revolutionary ideas bore the title of "The Forces of Inorganic Nature," and was published in 1842. It is one of the gems of scientific literature, and fortunately it is not too long to be quoted in its entirety. Seldom if ever was a great revolutionary doctrine expounded in briefer compass:

"What are we to understand by 'forces'? and how are different forces related to each other? The term force conveys for the most part the idea of something unknown, unsearchable, and

hypothetical; while the term matter, on the other hand, implies the possession, by the object in question, of such definite properties as weight and extension. An attempt, therefore, to render the idea of force equally exact with that of matter is one which should be welcomed by all those who desire to have their views of nature clear and unencumbered by hypothesis.

"Forces are causes; and accordingly we may make full application in relation to them of the principle causa aequat effectum. If the cause c has the effect e, then $c = e$; if, in its turn, e is the cause of a second effect of f, we have $e = f$, and so on: $c = e = f \ldots = c$. In a series of causes and effects, a term or a part of a term can never, as is apparent from the nature of an equation, become equal to nothing. This first property of all causes we call their indestructibility.

"If the given cause c has produced an effect e equal to itself, it has in that very act ceased to be--c has become e. If, after the production of e, c still remained in the whole or in part, there must be still further effects corresponding to this remaining cause: the total effect of c would thus be $> e$, which would be contrary to the supposition $c = e$. Accordingly, since c becomes e, and e becomes f, etc., we must regard these various magnitudes as different forms under which one and the same object makes its appearance. This capability of assuming various forms is the second essential property of all causes. Taking both properties together, we may say, causes an INDESTRUCTIBLE quantitatively, and quantitatively CONVERTIBLE objects.

"There occur in nature two causes which apparently never pass one into the other," said Mayer. "The first class consists of such causes as possess the properties of weight and impenetrability. These are kinds of matter. The other class is composed of causes which are wanting in the properties just mentioned-- namely, forces, called also imponderables, from the negative property that has been indicated. Forces are therefore INDESTRUCTIBLE, CONVERTIBLE, IMPONDERABLE OBJECTS.

"As an example of causes and effects, take matter: explosive gas, $H + O$, and water, HO, are related to each other as cause and effect; therefore $H + O = HO$. But if $H + O$ becomes HO, heat, cal., makes its appearance as well as water; this heat must likewise have a cause, x, and we have therefore $H + O + X = HO + cal.$ It might be asked, however, whether $H + O$ is really $= HO$, and $x = cal.$, and not perhaps $H + O = cal.$, and $x = HO$, whence the above equation could equally be deduced; and so in many other cases. The phlogistic chemists recognized the equation between cal. and x, or phlogiston as they called it, and in so doing made a great step in advance; but they involved themselves again in a system of mistakes by putting x in place of O. In this way they obtained $H = HO + x$.

"Chemistry teaches us that matter, as a cause, has matter for its effect; but we may say with equal justification that to force as a cause corresponds force as effect. Since $c = e$, and $e = c$, it is natural to call one term of an equation a force, and the other an effect of force, or phenomenon, and to attach different notions to the expression force and phenomenon. In brief, then, if the cause is matter, the effect is matter; if the cause is a force, the effect is also a force.

"The cause that brings about the raising of a weight is a force. The effect of the raised weight is, therefore, also a force; or, expressed in a more general form, SEPARATION IN SPACE OF PONDERABLE OBJECTS IS A FORCE; and since this force causes the fall of bodies, we call it

FALLING FORCE. Falling force and fall, or, still more generally, falling force and motion, are forces related to each other as cause and effect--forces convertible into each other--two different forms of one and the same object. For example, a weight resting on the ground is not a force: it is neither the cause of motion nor of the lifting of another weight. It becomes so, however, in proportion as it is raised above the ground. The cause--that is, the distance between a weight and the earth, and the effect, or the quantity of motion produced, bear to each other, as shown by mechanics, a constant relation.

'Gravity being regarded as the cause of the falling of bodies, a gravitating force is spoken of; and thus the ideas of PROPERTY and of FORCE are confounded with each other. Precisely that which is the essential attribute of every force--that is, the UNION of indestructibility with convertibility--is wanting in every property: between a property and a force, between gravity and motion, it is therefore impossible to establish the equation required for a rightly conceived causal relation. If gravity be called a force, a cause is supposed which produces effects without itself diminishing, and incorrect conceptions of the causal connections of things are thereby fostered. In order that a body may fall, it is just as necessary that it be lifted up as that it should be heavy or possess gravity. The fall of bodies, therefore, ought not to be ascribed to their gravity alone. The problem of mechanics is to develop the equations which subsist between falling force and motion, motion and falling force, and between different motions. Here is a case in point: The magnitude of the falling force v is directly proportional (the earth's radius being assumed--oo) to the magnitude of the mass m, and the height d, to which it is raised--that is, $v = md$. If the height $d = l$, to which the mass m is raised, is transformed into the final velocity $c = l$ of this mass, we have also $v = mc$; but from the known relations existing between d and c, it results that, for other values of d or of c, the measure of the force v is mc squared; accordingly v = md = mcsquared. The law of the conservation of vis viva is thus found to be based on the general law of the indestructibility of causes.

"In many cases we see motion cease without having caused another motion or the lifting of a weight. But a force once in existence cannot be annihilated--it can only change its form. And the question therefore arises, what other forms is force, which we have become acquainted with as falling force and motion, capable of assuming? Experience alone can lead us to a conclusion on this point. That we may experiment to advantage, we must select implements which, besides causing a real cessation of motion, are as little as possible altered by the objects to be examined. For example, if we rub together two metal plates, we see motion disappear, and heat, on the other hand, make its appearance, and there remains to be determined only whether MOTION is the cause of heat. In order to reach a decision on this point, we must discuss the question whether, in the numberless cases in which the expenditure of motion is accompanied by the appearance of heat, the motion has not some other effect than the production of heat, and the heat some other cause than the motion.

"A serious attempt to ascertain the effects of ceasing motion has never been made. Without wishing to exclude a priori the hypothesis which it may be possible to establish, therefore, we observe only that, as a rule, this effect cannot be supposed to be an alteration in the state of aggregation of the moved (that is, rubbing, etc.) bodies. If we assume that a certain quantity of motion v is expended in the conversion of a rubbing substance m into n, we must then have $m + v - n$, and $n = m + v$; and when n is reconverted into m, v must appear again in some form or other.

By the friction of two metallic plates continued for a very long time, we can gradually cause the cessation of an immense quantity of movement; but would it ever occur to us to look for even the smallest trace of the force which has disappeared in the metallic dust that we could collect, and to try to regain it thence? We repeat, the motion cannot have been annihilated; and contrary, or positive and negative, motions cannot be regarded as = o any more than contrary motions can come out of nothing, or a weight can raise itself.

"Without the recognition of a causal relation between motion and heat, it is just as difficult to explain the production of heat as it is to give any account of the motion that disappears. The heat cannot be derived from the diminution of the volume of the rubbing substances. It is well known that two pieces of ice may be melted by rubbing them together in vacuo; but let any one try to convert ice into water by pressure, however enormous. The author has found that water undergoes a rise of temperature when shaken violently. The water so heated (from twelve to thirteen degrees centigrade) has a greater bulk after being shaken than it had before. Whence now comes this quantity of heat, which by repeated shaking may be called into existence in the same apparatus as often as we please? The vibratory hypothesis of heat is an approach towards the doctrine of heat being the effect of motion, but it does not favor the admission of this causal relation in its full generality. It rather lays the chief stress on restless oscillations.

"If it be considered as now established that in many cases no other effect of motion can be traced except heat, and that no other cause than motion can be found for the heat that is produced, we prefer the assumption that heat proceeds from motion to the assumption of a cause without effect and of an effect without a cause. Just as the chemist, instead of allowing oxygen and hydrogen to disappear without further investigation, and water to be produced in some inexplicable manner, establishes a connection between oxygen and hydrogen on the one hand, and water on the other.

"We may conceive the natural connection existing between falling force, motion, and heat as follows: We know that heat makes its appearance when the separate particles of a body approach nearer to each other; condensation produces heat. And what applies to the smallest particles of matter, and the smallest intervals between them, must also apply to large masses and to measurable distances. The falling of a weight is a diminution of the bulk of the earth, and must therefore without doubt be related to the quantity of heat thereby developed; this quantity of heat must be proportional to the greatness of the weight and its distance from the ground. From this point of view we are easily led to the equations between falling force, motion, and heat that have already been discussed.

"But just as little as the connection between falling force and motion authorizes the conclusion that the essence of falling force is motion, can such a conclusion be adopted in the case of heat. We are, on the contrary, rather inclined to infer that, before it can become heat, motion must cease to exist as motion, whether simple, or vibratory, as in the case of light and radiant heat, etc.

"If falling force and motion are equivalent to heat, heat must also naturally be equivalent to motion and falling force. Just as heat appears as an EFFECT of the diminution of bulk and of the cessation of motion, so also does heat disappear as a CAUSE when its effects are produced in the shape of motion, expansion, or raising of weight.

"In water-mills the continual diminution in bulk which the earth undergoes, owing to the fall of the water, gives rise to motion, which afterwards disappears again, calling forth unceasingly a great quantity of heat; and, inversely, the steam-engine serves to decompose heat again into motion or the raising of weights. A locomotive with its train may be compared to a distilling apparatus; the heat applied under the boiler passes off as motion, and this is deposited again as heat at the axles of the wheels."

Mayer then closes his paper with the following deduction: "The solution of the equations subsisting between falling force and motion requires that the space fallen through in a given time--e. g., the first second-- should be experimentally determined. In like manner, the solution of the equations subsisting between falling force and motion on the one hand and heat on the other requires an answer to the question, How great is the quantity of heat which corresponds to a given quantity of motion or falling force? For instance, we must ascertain how high a given weight requires to be raised above the ground in order that its falling force maybe equivalent to the raising of the temperature of an equal weight of water from 0 degrees to 1 degrees centigrade. The attempt to show that such an equation is the expression of a physical truth may be regarded as the substance of the foregoing remarks.

"By applying the principles that have been set forth to the relations subsisting between the temperature and the volume of gases, we find that the sinking of a mercury column by which a gas is compressed is equivalent to the quantity of heat set free by the compression; and hence it follows, the ratio between the capacity for heat of air under constant pressure and its capacity under constant volume being taken as = 1.421, that the warming of a given weight of water from 0 degrees to 1 degrees centigrade corresponds to the fall of an equal weight from the height of about three hundred and sixty-five metres. If we compare with this result the working of our best steam-engines, we see how small a part only of the heat applied under the boiler is really transformed into motion or the raising of weights; and this may serve as justification for the attempts at the profitable production of motion by some other method than the expenditure of the chemical difference between carbon and oxygen--more particularly by the transformation into motion of electricity obtained by chemical means."[1]

MAYER AND HELMHOLTZ

Here, then, was this obscure German physician, leading the humdrum life of a village practitioner, yet seeing such visions as no human being in the world had ever seen before.

The great principle he had discovered became the dominating thought of his life, and filled all his leisure hours. He applied it far and wide, amid all the phenomena of the inorganic and organic worlds. It taught him that both vegetables and animals are machines, bound by the same laws that hold sway over inorganic matter, transforming energy, but creating nothing. Then his mind reached out into space and met a universe made up of questions. Each star that blinked down at him as he rode in answer to a night-call seemed an interrogation-point asking, How do I exist? Why have I not long since burned out if your theory of conservation be true? No one had hitherto even tried to answer that question; few had so much as realized that it demanded an answer. But the Heilbronn physician understood the question and found an answer. His meteoric hypothesis, published in

1848, gave for the first time a tenable explanation of the persistent light and heat of our sun and the myriad other suns--an explanation to which we shall recur in another connection.

All this time our isolated philosopher, his brain aflame with the glow of creative thought, was quite unaware that any one else in the world was working along the same lines. And the outside world was equally heedless of the work of the Heilbronn physician. There was no friend to inspire enthusiasm and give courage, no kindred spirit to react on this masterful but lonely mind. And this is the more remarkable because there are few other cases where a master-originator in science has come upon the scene except as the pupil or friend of some other master-originator. Of the men we have noticed in the present connection, Young was the friend and confrere of Davy; Davy, the protege of Rumford; Faraday, the pupil of Davy; Fresnel, the co-worker with Arago; Colding, the confrere of Oersted; Joule, the pupil of Dalton. But Mayer is an isolated phenomenon--one of the lone mountain-peak intellects of the century. That estimate may be exaggerated which has called him the Galileo of the nineteenth century, but surely no lukewarm praise can do him justice.

Yet for a long time his work attracted no attention whatever. In 1847, when another German physician, Hermann von Helmholtz, one of the most massive and towering intellects of any age, had been independently led to comprehension of the doctrine of the conservation of energy and published his treatise on the subject, he had hardly heard of his countryman Mayer. When he did hear of him, however, he hastened to renounce all claim to the doctrine of conservation, though the world at large gives him credit of independent even though subsequent discovery.

JOULE'S PAPER OF 1843

Meantime, in England, Joule was going on from one experimental demonstration to another, oblivious of his German competitors and almost as little noticed by his own countrymen. He read his first paper before the chemical section of the British Association for the Advancement of Science in 1843, and no one heeded it in the least. It is well worth our while, however, to consider it at length. It bears the title, "On the Calorific Effects of Magneto-Electricity, and the Mechanical Value of Heat." The full text, as published in the Report of the British Association, is as follows:

"Although it has been long known that fine platinum wire can be ignited by magneto-electricity, it still remained a matter of doubt whether heat was evolved by the COILS in which the magneto-electricity was generated; and it seemed indeed not unreasonable to suppose that COLD was produced there in order to make up for the heat evolved by the other part of the circuit. The author therefore has endeavored to clear up this uncertainty by experiment. His apparatus consisted of a small compound electro-magnet, immersed in water, revolving between the poles of a powerful stationary magnet. The magneto-electricity developed in the coils of the revolving electro-magnet was measured by an accurate galvanometer; and the temperature of the water was taken before and after each experiment by a very delicate thermometer. The influence of the temperature of the surrounding atmospheric air was guarded against by covering the revolving tube with flannel, etc., and by the adoption of a system of interpolation. By an extensive series of experiments with the above apparatus the author succeeded in proving that heat is evolved by the coils of the magneto-electrical machine, as well as by any other part of the circuit, in proportion to the resistance to conduction of the wire and the square of the current; the magneto having, under comparable circumstances, the same calorific power as the voltaic electricity.

"Professor Jacobi, of St. Petersburg, bad shown that the motion of an electro-magnetic machine generates magneto-electricity in opposition to the voltaic current of the battery. The author had observed the same phenomenon on arranging his apparatus as an electro-magnetic machine; but had found that no additional heat was evolved on account of the conflict of forces in the coil of the electro-magnet, and that the heat evolved by the coil remained, as before, proportional to the square of the current. Again, by turning the machine contrary to the direction of the attractive forces, so as to increase the intensity of the voltaic current by the assistance of the magneto-electricity, he found that the evolution of heat was still proportional to the square of the current. The author discovered, therefore, that the heat evolved by the voltaic current is invariably proportional to the square of the current, however the intensity of the current may be varied by magnetic induction. But Dr. Faraday has shown that the chemical effects of the current are simply as its quantity. Therefore he concluded that in the electro- magnetic engine a part of the heat due to the chemical actions of the battery is lost by the circuit, and converted into mechanical power; and that when the electro-magnetic engine is turned CONTRARY to the direction of the attractive forces, a greater quantity of heat is evolved by the circuit than is due to the chemical reactions of the battery, the over-plus quantity being produced by the conversion of the mechanical force exerted in turning the machine. By a dynamometrical apparatus attached to his machine, the author has ascertained that, in all the above cases, a quantity of heat, capable of increasing the temperature of a pound of water by one degree of Fahrenheit's scale, is equal to the mechanical force capable of raising a weight of about eight hundred and thirty pounds to the height of one foot."[2]

JOULE OR MAYER?

Two years later Joule wished to read another paper, but the chairman hinted that time was limited, and asked him to confine himself to a brief verbal synopsis of the results of his experiments. Had the chairman but known it, he was curtailing a paper vastly more important than all the other papers of the meeting put together. However, the synopsis was given, and one man was there to hear it who had the genius to appreciate its importance. This was William Thomson, the present Lord Kelvin, now known to all the world as among the greatest of natural philosophers, but then only a novitiate in science. He came to Joule's aid, started rolling the ball of controversy, and subsequently associated himself with the Manchester experimenter in pursuing his investigations.

But meantime the acknowledged leaders of British science viewed the new doctrine askance. Faraday, Brewster, Herschel--those were the great names in physics at that day, and no one of them could quite accept the new views regarding energy. For several years no older physicist, speaking with recognized authority, came forward in support of the doctrine of conservation. This culminating thought of the first half of the nineteenth century came silently into the world, unheralded and unopposed. The fifth decade of the century had seen it elaborated and substantially demonstrated in at least three different countries, yet even the leaders of thought did not so much as know of its existence. In 1853 Whewell, the historian of the inductive sciences, published a second edition of his history, and, as Huxley has pointed out, he did not so much as refer to the revolutionizing thought which even then was a full decade old.

By this time, however, the battle was brewing. The rising generation saw the importance of a law which their elders could not appreciate, and soon it was noised abroad that there were more than

one claimant to the honor of discovery. Chiefly through the efforts of Professor Tyndall, the work of Mayer became known to the British public, and a most regrettable controversy ensued between the partisans of Mayer and those of Joule--a bitter controversy, in which Davy's contention that science knows no country was not always regarded, and which left its scars upon the hearts and minds of the great men whose personal interests were involved.

And so to this day the question who is the chief discoverer of the law of the conservation of energy is not susceptible of a categorical answer that would satisfy all philosophers. It is generally held that the first choice lies between Joule and Mayer. Professor Tyndall has expressed the belief that in future each of these men will be equally remembered in connection with this work. But history gives us no warrant for such a hope. Posterity in the long run demands always that its heroes shall stand alone. Who remembers now that Robert Hooke contested with Newton the discovery of the doctrine of universal gravitation? The judgment of posterity is unjust, but it is inexorable. And so we can little doubt that a century from now one name will be mentioned as that of the originator of the great doctrine of the conservation of energy. The man whose name is thus remembered will perhaps be spoken of as the Galileo, the Newton, of the nineteenth century; but whether the name thus dignified by the final verdict of history will be that of Colding, Mohr, Mayer, Helmholtz, or Joule, is not as, yet decided.

LORD KELVIN AND THE DISSIPATION OF ENERGY

The gradual permeation of the field by the great doctrine of conservation simply repeated the history of the introduction of every novel and revolutionary thought. Necessarily the elder generation, to whom all forms of energy were imponderable fluids, must pass away before the new conception could claim the field. Even the word energy, though Young had introduced it in 1807, did not come into general use till some time after the middle of the century. To the generality of philosophers (the word physicist was even less in favor at this time) the various forms of energy were still subtile fluids, and never was idea relinquished with greater unwillingness than this. The experiments of Young and Fresnel had convinced a large number of philosophers that light is a vibration and not a substance; but so great an authority as Biot clung to the old emission idea to the end of his life, in 1862, and held a following.

Meantime, however, the company of brilliant young men who had just served their apprenticeship when the doctrine of conservation came upon the scene had grown into authoritative positions, and were battling actively for the new ideas. Confirmatory evidence that energy is a molecular motion and not an "imponderable" form of matter accumulated day by day. The experiments of two Frenchmen, Hippolyte L. Fizeau and Leon Foucault, served finally to convince the last lingering sceptics that light is an undulation; and by implication brought heat into the same category, since James David Forbes, the Scotch physicist, had shown in 1837 that radiant heat conforms to the same laws of polarization and double refraction that govern light. But, for that matter, the experiments that had established the mechanical equivalent of heat hardly left room for doubt as to the immateriality of this "imponderable." Doubters had indeed, expressed scepticism as to the validity of Joule's experiments, but the further researches, experimental and mathematical, of such workers as Thomson (Lord Kelvin), Rankine, and Tyndall in Great Britain, of Helmholtz and Clausius in Germany, and of Regnault in France, dealing with various manifestations of heat, placed the evidence beyond the reach of criticism.

Out of these studies, just at the middle of the century, to which the experiments of Mayer and Joule had led, grew the new science of thermo-dynamics. Out of them also grew in the mind of one of the investigators a new generalization, only second in importance to the doctrine of conservation itself. Professor William Thomson (Lord Kelvin) in his studies in thermodynamics was early impressed with the fact that whereas all the molar motion developed through labor or gravity could be converted into heat, the process is not fully reversible. Heat can, indeed, be converted into molar motion or work, but in the process a certain amount of the heat is radiated into space and lost. The same thing happens whenever any other form of energy is converted into molar motion. Indeed, every transmutation of energy, of whatever character, seems complicated by a tendency to develop heat, part of which is lost. This observation led Professor Thomson to his doctrine of the dissipation of energy, which he formulated before the Royal Society of Edinburgh in 1852, and published also in the Philosophical Magazine the same year, the title borne being, "On a Universal Tendency in Nature to the Dissipation of Mechanical Energy."

From the principle here expressed Professor Thomson drew the startling conclusion that, "since any restoration of this mechanical energy without more than an equivalent dissipation is impossible," the universe, as known to us, must be in the condition of a machine gradually running down; and in particular that the world we live on has been within a finite time unfit for human habitation, and must again become so within a finite future. This thought seems such a commonplace to-day that it is difficult to realize how startling it appeared half a century ago. A generation trained, as ours has been, in the doctrines of the conservation and dissipation of energy as the very alphabet of physical science can but ill appreciate the mental attitude of a generation which for the most part had not even thought it problematical whether the sun could continue to give out heat and light forever. But those advance thinkers who had grasped the import of the doctrine of conservation could at once appreciate the force of Thomson's doctrine of dissipation, and realize the complementary character of the two conceptions.

Here and there a thinker like Rankine did, indeed, attempt to fancy conditions under which the energy lost through dissipation might be restored to availability, but no such effort has met with success, and in time Professor Thomson's generalization and his conclusions as to the consequences of the law involved came to be universally accepted.

The introduction of the new views regarding the nature of energy followed, as I have said, the course of every other growth of new ideas. Young and imaginative men could accept the new point of view; older philosophers, their minds channelled by preconceptions, could not get into the new groove. So strikingly true is this in the particular case now before us that it is worth while to note the ages at the time of the revolutionary experiments of the men whose work has been mentioned as entering into the scheme of evolution of the idea that energy is merely a manifestation of matter in motion. Such a list will tell the story better than a volume of commentary.

Observe, then, that Davy made his epochal experiment of melting ice by friction when he was a youth of twenty. Young was no older when he made his first communication to the Royal Society, and was in his twenty-seventh year when he first actively espoused the undulatory theory. Fresnel was twenty-six when he made his first important discoveries in the same field; and Arago, who at

once became his champion, was then but two years his senior, though for a decade he had been so famous that one involuntarily thinks of him as belonging to an elder generation.

Forbes was under thirty when he discovered the polarization of heat, which pointed the way to Mohr, then thirty-one, to the mechanical equivalent. Joule was twenty-two in 1840, when his great work was begun; and Mayer, whose discoveries date from the same year, was then twenty-six, which was also the age of Helmholtz when he published his independent discovery of the same law. William Thomson was a youth just past his majority when he came to the aid of Joule before the British Society, and but seven years older when he formulated his own doctrine of the dissipation of energy. And Clausius and Rankine, who are usually mentioned with Thomson as the great developers of thermo-dynamics, were both far advanced with their novel studies before they were thirty. With such a list in mind, we may well agree with the father of inductive science that "the man who is young in years may be old in hours."

Yet we must not forget that the shield has a reverse side. For was not the greatest of observing astronomers, Herschel, past thirty-five before he ever saw a telescope, and past fifty before he discovered the heat rays of the spectrum? And had not Faraday reached middle life before he turned his attention especially to electricity? Clearly, then, to make this phrase complete, Bacon should have added that "the man who is old in years may be young in imagination." Here, however, even more appropriate than in the other case --more's the pity--would have been the application of his qualifying clause: "but that happeneth rarely."

THE FINAL UNIFICATION

There are only a few great generalizations as yet thought out in any single field of science. Naturally, then, after a great generalization has found definitive expression, there is a period of lull before another forward move. In the case of the doctrines of energy, the lull has lasted half a century. Throughout this period, it is true, a multitude of workers have been delving in the field, and to the casual observer it might seem as if their activity had been boundless, while the practical applications of their ideas--as exemplified, for example, in the telephone, phonograph, electric light, and so on --have been little less than revolutionary. Yet the most competent of living authorities, Lord Kelvin, could assert in 1895 that in fifty years he had learned nothing new regarding the nature of energy.

This, however, must not be interpreted as meaning that the world has stood still during these two generations. It means rather that the rank and file have been moving forward along the road the leaders had already travelled. Only a few men in the world had the range of thought regarding the new doctrine of energy that Lord Kelvin had at the middle of the century. The few leaders then saw clearly enough that if one form of energy is in reality merely an undulation or vibration among the particles of "ponderable" matter or of ether, all other manifestations of energy must be of the same nature. But the rank and file were not even within sight of this truth for a long time after they had partly grasped the meaning of the doctrine of conservation. When, late in the fifties, that marvellous young Scotchman, James Clerk-Maxwell, formulating in other words an idea of Faraday's, expressed his belief that electricity and magnetism are but manifestations of various conditions of stress and motion in the ethereal medium (electricity a displacement of strain, magnetism a whirl in the ether), the idea met with no immediate popularity. And even less cordial was the reception

given the same thinker's theory, put forward in 1863, that the ethereal undulations producing the phenomenon we call light differ in no respect except in their wave-length from the pulsations of electro-magnetism.

At about the same time Helmholtz formulated a somewhat similar electro-magnetic theory of light; but even the weight of this combined authority could not give the doctrine vogue until very recently, when the experiments of Heinrich Hertz, the pupil of Helmholtz, have shown that a condition of electrical strain may be developed into a wave system by recurrent interruptions of the electric state in the generator, and that such waves travel through the ether with the rapidity of light. Since then the electro-magnetic theory of light has been enthusiastically referred to as the greatest generalization of the century; but the sober thinker must see that it is really only what Hertz himself called it--one pier beneath the great arch of conservation. It is an interesting detail of the architecture, but the part cannot equal the size of the whole.

More than that, this particular pier is as yet by no means a very firm one. It has, indeed, been demonstrated that waves of electro-magnetism pass through space with the speed of light, but as yet no one has developed electric waves even remotely approximating the shortness of the visual rays. The most that can positively be asserted, therefore, is that all the known forms of radiant energy-heat, light, electro-magnetism-- travel through space at the same rate of speed, and consist of traverse vibrations--"lateral quivers," as Fresnel said of light--known to differ in length, and not positively known to differ otherwise. It has, indeed, been suggested that the newest form of radiant energy, the famous X-ray of Professor Roentgen's discovery, is a longitudinal vibration, but this is a mere surmise. Be that as it may, there is no one now to question that all forms of radiant energy, whatever their exact affinities, consist essentially of undulatory motions of one uniform medium.

A full century of experiment, calculation, and controversy has thus sufficed to correlate the "imponderable fluids" of our forebears, and reduce them all to manifestations of motion among particles of matter. At first glimpse that seems an enormous change of view. And yet, when closely considered, that change in thought is not so radical as the change in phrase might seem to imply. For the nineteenth-century physicist, in displacing the "imponderable fluids" of many kinds--one each for light, heat, electricity, magnetism--has been obliged to substitute for them one all-pervading fluid, whose various quivers, waves, ripples, whirls or strains produce the manifestations which in popular parlance are termed forms of force. This all-pervading fluid the physicist terms the ether, and he thinks of it as having no weight. In effect, then, the physicist has dispossessed the many imponderables in favor of a single imponderable--though the word imponderable has been banished from his vocabulary. In this view the ether--which, considered as a recognized scientific verity, is essentially a nineteenth- century discovery--is about the most interesting thing in the universe. Something more as to its properties, real or assumed, we shall have occasion to examine as we turn to the obverse side of physics, which demands our attention in the next chapter.

IX. THE ETHER AND PONDERABLE MATTER

"Whatever difficulties we may have in forming a consistent idea of the constitution of the ether, there can be no doubt that the interplanetary and interstellar spaces are not empty, but are occupied

by a material substance or body which is certainly the largest and probably the most uniform body of which we have any knowledge."

Such was the verdict pronounced some thirty years ago by James Clerk-Maxwell, one of the very greatest of nineteenth-century physicists, regarding the existence of an all-pervading plenum in the universe, in which every particle of tangible matter is immersed. And this verdict may be said to express the attitude of the entire philosophical world of our day. Without exception, the authoritative physicists of our time accept this plenum as a verity, and reason about it with something of the same confidence they manifest in speaking of "ponderable" matter or of, energy. It is true there are those among them who are disposed to deny that this all-pervading plenum merits the name of matter. But that it is a something, and a vastly important something at that, all are agreed. Without it, they allege, we should know nothing of light, of radiant heat, of electricity or magnetism; without it there would probably be no such thing as gravitation; nay, they even hint that without this strange something, ether, there would be no such thing as matter in the universe. If these contentions of the modern physicist are justified, then this intangible ether is incomparably the most important as well as the "largest and most uniform substance or body" in the universe. Its discovery may well be looked upon as one of the most important feats of the nineteenth century.

For a discovery of that century it surely is, in the sense that all the known evidences of its existence were gathered in that epoch. True dreamers of all ages have, for metaphysical reasons, imagined the existence of intangible fluids in space--they had, indeed, peopled space several times over with different kinds of ethers, as Maxwell remarks--but such vague dreamings no more constituted the discovery of the modern ether than the dream of some pre-Columbian visionary that land might lie beyond the unknown waters constituted the discovery of America. In justice it must be admitted that Huyghens, the seventeenth-century originator of the undulatory theory of light, caught a glimpse of the true ether; but his contemporaries and some eight generations of his successors were utterly deaf to his claims; so he bears practically the same relation to the nineteenth-century discoverers of ether that the Norseman bears to Columbus.

The true Columbus of the ether was Thomas Young. His discovery was consummated in the early days of the nineteenth century, when he brought forward the first, conclusive proofs of the undulatory theory of light. To say that light consists of undulations is to postulate something that undulates; and this something could not be air, for air exists only in infinitesimal quantity, if at all, in the interstellar spaces, through which light freely penetrates. But if not air, what then? Why, clearly, something more intangible than air; something supersensible, evading all direct efforts to detect it, yet existing everywhere in seemingly vacant space, and also interpenetrating the substance of all transparent liquids and solids, if not, indeed, of all tangible substances. This intangible something Young rechristened the Luminiferous Ether.

In the early days of his discovery Young thought of the undulations which produce light and radiant heat as being longitudinal--a forward and backward pulsation, corresponding to the pulsations of sound--and as such pulsations can be transmitted by a fluid medium with the properties of ordinary fluids, he was justified in thinking of the ether as being like a fluid in its properties, except for its extreme intangibility. But about 1818 the experiments of Fresnel and Arago with polarization of light made it seem very doubtful whether the theory of longitudinal vibrations is sufficient, and it was suggested by Young, and independently conceived and demonstrated by Fresnel, that the

luminiferous undulations are not longitudinal, but transverse; and all the more recent experiments have tended to confirm this view. But it happens that ordinary fluids-- gases and liquids--cannot transmit lateral vibrations; only rigid bodies are capable of such a vibration. So it became necessary to assume that the luminiferous ether is a body possessing elastic rigidity--a familiar property of tangible solids, but one quite unknown among fluids.

The idea of transverse vibrations carried with it another puzzle. Why does not the ether, when set aquiver with the vibration which gives us the sensation we call light, have produced in its substance subordinate quivers, setting out at right angles from the path of the original quiver? Such perpendicular vibrations seem not to exist, else we might see around a corner; how explain their absence? The physicist could think of but one way: they must assume that the ether is incompressible. It must fill all space--at any rate, all space with which human knowledge deals--perfectly full.

These properties of the ether, incompressibility and elastic rigidity, are quite conceivable by themselves; but difficulties of thought appear when we reflect upon another quality which the ether clearly must possess-- namely, frictionlessness. By hypothesis this rigid, incompressible body pervades all space, imbedding every particle of tangible matter; yet it seems not to retard the movements of this matter in the slightest degree. This is undoubtedly the most difficult to comprehend of the alleged properties of the ether. The physicist explains it as due to the perfect elasticity of the ether, in virtue of which it closes in behind a moving particle with a push exactly counterbalancing the stress required to penetrate it in front.

To a person unaccustomed to think of seemingly solid matter as really composed of particles relatively wide apart, it is hard to understand the claim that ether penetrates the substance of solids--of glass, for example--and, to use Young's expression, which we have previously quoted, moves among them as freely as the wind moves through a grove of trees. This thought, however, presents few difficulties to the mind accustomed to philosophical speculation. But the question early arose in the mind of Fresnel whether the ether is not considerably affected by contact with the particles of solids. Some of his experiments led him to believe that a portion of the ether which penetrates among the molecules of tangible matter is held captive, so to speak, and made to move along with these particles. He spoke of such portions of the ether as "bound" ether, in contradistinction to the great mass of "free" ether. Half a century after Fresnel's death, when the ether hypothesis had become an accepted tenet of science, experiments were undertaken by Fizeau in France, and by Clerk-Maxwell in England, to ascertain whether any portion of ether is really thus bound to particles of matter; but the results of the experiments were negative, and the question is still undetermined.

While the undulatory theory of light was still fighting its way, another kind of evidence favoring the existence of an ether was put forward by Michael Faraday, who, in the course of his experiments in electrical and magnetic induction, was led more and more to perceive definite lines or channels of force in the medium subject to electro-magnetic influence. Faraday's mind, like that of Newton and many other philosophers, rejected the idea of action at a distance, and he felt convinced that the phenomena of magnetism and of electric induction told strongly for the existence of an invisible plenum everywhere in space, which might very probably be the same plenum that carries the undulations of light and radiant heat.

Then, about the middle of the century, came that final revolution of thought regarding the nature of energy which we have already outlined in the preceding chapter, and with that the case for ether was considered to be fully established. The idea that energy is merely a "mode of motion" (to adopt Tyndall's familiar phrase), combined with the universal rejection of the notion of action at a distance, made the acceptance of a plenum throughout space a necessity of thought--so, at any rate, it has seemed to most physicists of recent decades. The proof that all known forms of radiant energy move through space at the same rate of speed is regarded as practically a demonstration that but one plenum--one ether--is concerned in their transmission. It has, indeed, been tentatively suggested, by Professor J. Oliver Lodge, that there may be two ethers, representing the two opposite kinds of electricity, but even the author of this hypothesis would hardly claim for it a high degree of probability.

The most recent speculations regarding the properties of the ether have departed but little from the early ideas of Young and Fresnel. It is assumed on all sides that the ether is a continuous, incompressible body, possessing rigidity and elasticity. Lord Kelvin has even calculated the probable density of this ether, and its coefficient of rigidity. As might be supposed, it is all but infinitely tenuous as compared with any tangible solid, and its rigidity is but infinitesimal as compared with that of steel. In a word, it combines properties of tangible matter in a way not known in any tangible substance. Therefore we cannot possibly conceive its true condition correctly. The nearest approximation, according to Lord Kelvin, is furnished by a mould of transparent jelly. It is a crude, inaccurate analogy, of course, the density and resistance of jelly in particular being utterly different from those of the ether; but the quivers that run through the jelly when it is shaken, and the elastic tension under which it is placed when its mass is twisted about, furnish some analogy to the quivers and strains in the ether, which are held to constitute radiant energy, magnetism, and electricity.

The great physicists of the day being at one regarding the existence of this all-pervading ether, it would be a manifest presumption for any one standing without the pale to challenge so firmly rooted a belief. And, indeed, in any event, there seems little ground on which to base such a challenge. Yet it may not be altogether amiss to reflect that the physicist of to-day is no more certain of his ether than was his predecessor of the eighteenth century of the existence of certain alleged substances which he called phlogiston, caloric, corpuscles of light, and magnetic and electric fluids. It would be but the repetition of history should it chance that before the close of another century the ether should have taken its place along with these discarded creations of the scientific imagination of earlier generations. The philosopher of to-day feels very sure that an ether exists; but when he says there is "no doubt" of its existence he speaks incautiously, and steps beyond the bounds of demonstration. He does not KNOW that action cannot take place at a distance; he does not KNOW that empty space itself may not perform the functions which he ascribes to his space-filling ether.

Meantime, however, the ether, be it substance or be it only dream-stuff, is serving an admirable purpose in furnishing a fulcrum for modern physics. Not alone to the student of energy has it proved invaluable, but to the student of matter itself as well. Out of its hypothetical mistiness has been reared the most tenable theory of the constitution of ponderable matter which has yet been suggested--or, at any rate, the one that will stand as the definitive nineteenth-century guess at this

"riddle of the ages." I mean, of course, the vortex theory of atoms--that profound and fascinating doctrine which suggests that matter, in all its multiform phases, is neither more nor less than ether in motion.

The author of this wonderful conception is Lord Kelvin. The idea was born in his mind of a happy union of mathematical calculations with concrete experiments. The mathematical calculations were largely the work of Hermann von Helmholtz, who, about the year 1858, had undertaken to solve some unique problems in vortex motions. Helmholtz found that a vortex whirl, once established in a frictionless medium, must go on, theoretically, unchanged forever. In a limited medium such a whirl may be V-shaped, with its ends at the surface of the medium. We may imitate such a vortex by drawing the bowl of a spoon quickly through a cup of water. But in a limitless medium the vortex whirl must always be a closed ring, which may take the simple form of a hoop or circle, or which may be indefinitely contorted, looped, or, so to speak, knotted. Whether simple or contorted, this endless chain of whirling matter (the particles revolving about the axis of the loop as the particles of a string revolve when the string is rolled between the fingers) must, in a frictionless medium, retain its form and whirl on with undiminished speed forever.

While these theoretical calculations of Helmholtz were fresh in his mind, Lord Kelvin (then Sir William Thomson) was shown by Professor P. G. Tait, of Edinburgh, an apparatus constructed for the purpose of creating vortex rings in air. The apparatus, which any one may duplicate, consisted simply of a box with a hole bored in one side, and a piece of canvas stretched across the opposite side in lieu of boards. Fumes of chloride of ammonia are generated within the box, merely to render the air visible. By tapping with the band on the canvas side of the box, vortex rings of the clouded air are driven out, precisely similar in appearance to those smoke-rings which some expert tobacco-smokers can produce by tapping on their cheeks, or to those larger ones which we sometimes see blown out from the funnel of a locomotive.

The advantage of Professor Tait's apparatus is its manageableness and the certainty with which the desired result can be produced. Before Lord Kelvin's interested observation it threw out rings of various sizes, which moved straight across the room at varying rates of speed, according to the initial impulse, and which behaved very strangely when coming in contact with one another. If, for example, a rapidly moving ring overtook another moving in the same path, the one in advance seemed to pause, and to spread out its periphery like an elastic band, while the pursuer seemed to contract, till it actually slid through the orifice of the other, after which each ring resumed its original size, and continued its course as if nothing had happened. When, on the other hand, two rings moving in slightly different directions came near each other, they seemed to have an attraction for each other; yet if they impinged, they bounded away, quivering like elastic solids. If an effort were made to grasp or to cut one of these rings, the subtle thing shrank from the contact, and slipped away as if it were alive.

And all the while the body which thus conducted itself consisted simply of a whirl in the air, made visible, but not otherwise influenced, by smoky fumes. Presently the friction of the surrounding air wore the ring away, and it faded into the general atmosphere-- often, however, not until it had persisted for many seconds, and passed clear across a large room. Clearly, if there were no friction, the ring's inertia must make it a permanent structure. Only the frictionless medium was lacking to fulfil all the conditions of Helmholtz's indestructible vortices. And at once Lord Kelvin bethought

him of the frictionless medium which physicists had now begun to accept--the all-pervading ether. What if vortex rings were started in this ether, must they not have the properties which the vortex rings in air had exhibited--inertia, attraction, elasticity? And are not these the properties of ordinary tangible matter? Is it not probable, then, that what we call matter consists merely of aggregations of infinitesimal vortex rings in the ether?

Thus the vortex theory of atoms took form in Lord Kelvin's mind, and its expression gave the world what many philosophers of our time regard as the most plausible conception of the constitution of matter hitherto formulated. It is only a theory, to be sure; its author would be the last person to claim finality for it. "It is only a dream," Lord Kelvin said to me, in referring to it not long ago. But it has a basis in mathematical calculation and in analogical experiment such as no other theory of matter can lay claim to, and it has a unifying or monistic tendency that makes it, for the philosophical mind, little less than fascinating. True or false, it is the definitive theory of matter of the twentieth century.

Quite aside from the question of the exact constitution of the ultimate particles of matter, questions as to the distribution of such particles, their mutual relations, properties, and actions, came in for a full share of attention during the nineteenth century, though the foundations for the modern speculations were furnished in a previous epoch. The most popular eighteenth- century speculation as to the ultimate constitution of matter was that of the learned Italian priest, Roger Joseph Boscovich, published in 1758, in his Theoria Philosophiae Naturalis. "In this theory," according to an early commentator, "the whole mass of which the bodies of the universe are composed is supposed to consist of an exceedingly great yet finite number of simple, indivisible, inextended atoms. These atoms are endued by the Creator with REPULSIVE and ATTRACTIVE forces, which vary according to the distance. At very small distances the particles of matter repel each other; and this repulsive force increases beyond all limits as the distances are diminished, and will consequently forever prevent actual contact. When the particles of matter are removed to sensible distances, the repulsive is exchanged for an attractive force, which decreases in inverse ratio with the squares of the distances, and extends beyond the spheres of the most remote comets."

This conception of the atom as a mere centre of force was hardly such as could satisfy any mind other than the metaphysical. No one made a conspicuous attempt to improve upon the idea, however, till just at the close of the century, when Humphry Davy was led, in the course of his studies of heat, to speculate as to the changes that occur in the intimate substance of matter under altered conditions of temperature. Davy, as we have seen, regarded heat as a manifestation of motion among the particles of matter. As all bodies with which we come in contact have some temperature, Davy inferred that the intimate particles of every substance must be perpetually in a state of vibration. Such vibrations, he believed, produced the "repulsive force" which (in common with Boscovich) he admitted as holding the particles of matter at a distance from one another. To heat a substance means merely to increase the rate of vibration of its particles; thus also, plainly, increasing the repulsive forces and expanding the bulk of the mass as a whole. If the degree of heat applied be sufficient, the repulsive force may become strong enough quite to overcome the attractive force, and the particles will separate and tend to fly away from one another, the solid then becoming a gas.

Not much attention was paid to these very suggestive ideas of Davy, because they were founded on the idea that heat is merely a motion, which the scientific world then repudiated; but half a century later, when the new theories of energy had made their way, there came a revival of practically the same ideas of the particles of matter (molecules they were now called) which Davy had advocated. Then it was that Clausius in Germany and Clerk-Maxwell in England took up the investigation of what came to be known as the kinetic theory of gases--the now familiar conception that all the phenomena of gases are due to the helter- skelter flight of the showers of widely separated molecules of which they are composed. The specific idea that the pressure or "spring" of gases is due to such molecular impacts was due to Daniel Bournelli, who advanced it early in the eighteenth century. The idea, then little noticed, had been revived about a century later by William Herapath, and again with some success by J. J. Waterston, of Bombay, about 1846; but it gained no distinct footing until taken in hand by Clausius in 1857 and by Clerk-Maxwell in 1859.

The considerations that led Clerk-Maxwell to take up the computations may be stated in his own words, as formulated in a paper "On the Motions and Collisions of Perfectly Elastic Spheres."

"So many of the properties of matter, especially when in the gaseous form," he says, "can be deduced from the hypothesis that their minute parts are in rapid motion, the velocity increasing with the temperature, that the precise nature of this motion becomes a subject of rational curiosity. Daniel Bournelli, Herapath, Joule, Kronig, Clausius, etc., have shown that the relations between pressure, temperature, and density in a perfect gas can be explained by supposing the particles to move with uniform velocities in straight lines, striking against the sides of the containing vessel and thus producing pressure. It is not necessary to suppose each particle to travel to any great distance in the same straight line; for the effect in producing pressure will be the same if the particles strike against each other; so that the straight line described may be very short. M. Clausius has determined the mean length of path in terms of the average of the particles, and the distance between the centres of two particles when the collision takes place. We have at present no means of ascertaining either of these distances; but certain phenomena, such as the internal friction of gases, the conduction of heat through a gas, and the diffusion of one gas through another, seem to indicate the possibility of determining accurately the mean length of path which a particle describes between two successive collisions. In order to lay the foundation of such investigations on strict mechanical principles, I shall demonstrate the laws of motion of an indefinite number of small, hard, and perfectly elastic spheres acting on one another only during impact. If the properties of such a system of bodies are found to correspond to those of gases, an important physical analogy will be established, which may lead to more accurate knowledge of the properties of matter. If experiments on gases are inconsistent with the hypothesis of these propositions, then our theory, though consistent with itself, is proved to be incapable of explaining the phenomena of gases. In either case it is necessary to follow out these consequences of the hypothesis.

"Instead of saying that the particles are hard, spherical, and elastic, we may, if we please, say the particles are centres of force, of which the action is insensible except at a certain very small distance, when it suddenly appears as a repulsive force of very great intensity. It is evident that either assumption will lead to the same results. For the sake of avoiding the repetition of a long phrase about these repulsive bodies, I shall proceed upon the assumption of perfectly elastic spherical bodies. If we suppose those aggregate molecules which move together to have a bounding surface which is not spherical, then the rotatory motion of the system will close up a certain

proportion of the whole vis viva, as has been shown by Clausius, and in this way we may account for the value of the specific heat being greater than on the more simple hypothesis."[1]

The elaborate investigations of Clerk-Maxwell served not merely to substantiate the doctrine, but threw a flood of light upon the entire subject of molecular dynamics. Soon the physicists came to feel as certain of the existence of these showers of flying molecules making up a gas as if they could actually see and watch their individual actions. Through study of the viscosity of gases--that is to say, of the degree of frictional opposition they show to an object moving through them or to another current of gas--an idea was gained, with the aid of mathematics, of the rate of speed at which the particles of the gas are moving, and the number of collisions which each particle must experience in a given time, and of the length of the average free path traversed by the molecule between collisions, These measurements were confirmed by study of the rate of diffusion at which different gases mix together, and also by the rate of diffusion of heat through a gas, both these phenomena being chiefly due to the helter-skelter flight of the molecules.

It is sufficiently astonishing to be told that such measurements as these have been made at all, but the astonishment grows when one hears the results. It appears from Clerk-Maxwell's calculations that the mean free path, or distance traversed by the molecules between collisions in ordinary air, is about one-half-millionth of an inch; while the speed of the molecules is such that each one experiences about eight billions of collisions per second! It would be hard, perhaps, to cite an illustration showing the refinements of modern physics better than this; unless, indeed, one other result that followed directly from these calculations be considered such--the feat, namely, of measuring the size of the molecules themselves. Clausius was the first to point out how this might be done from a knowledge of the length of free path; and the calculations were made by Loschmidt in Germany and by Lord Kelvin in England, independently.

The work is purely mathematical, of course, but the results are regarded as unassailable; indeed, Lord Kelvin speaks of them as being absolutely demonstrative within certain limits of accuracy. This does not mean, however, that they show the exact dimensions of the molecule; it means an estimate of the limits of size within which the actual size of the molecule may lie. These limits, Lord Kelvin estimates, are about the one- ten-millionth of a centimetre for the maximum, and the one-one-hundred-millionth of a centimetre for the minimum. Such figures convey no particular meaning to our blunt senses, but Lord Kelvin has given a tangible illustration that aids the imagination to at least a vague comprehension of the unthinkable smallness of the molecule. He estimates that if a ball, say of water or glass, about "as large as a football, were to be magnified up to the size of the earth, each constituent molecule being magnified in the same proportion, the magnified structure would be more coarse-grained than a heap of shot, but probably less coarse-grained than a heap of footballs."

Several other methods have been employed to estimate the size of molecules. One of these is based upon the phenomena of contact electricity; another upon the wave-theory of light; and another upon capillary attraction, as shown in the tense film of a soap-bubble! No one of these methods gives results more definite than that due to the kinetic theory of gases, just outlined; but the important thing is that the results obtained by these different methods (all of them due to Lord Kelvin) agree with one another in fixing the dimensions of the molecule at somewhere about the limits already mentioned. We may feel very sure indeed, therefore, that the molecules of matter are not the

unextended, formless points which Boscovich and his followers of the eighteenth century thought them. But all this, it must be borne in mind, refers to the molecule, not to the ultimate particle of matter, about which we shall have more to say in another connection. Curiously enough, we shall find that the latest theories as to the final term of the series are not so very far afield from the dreamings of the eighteenth-century philosophers; the electron of J. J. Thompson shows many points of resemblance to the formless centre of Boscovich.

Whatever the exact form of the molecule, its outline is subject to incessant variation; for nothing in molecular science is regarded as more firmly established than that the molecule, under all ordinary circumstances, is in a state of intense but variable vibration. The entire energy of a molecule of gas, for example, is not measured by its momentum, but by this plus its energy of vibration and rotation, due to the collisions already referred to. Clausius has even estimated the relative importance of these two quantities, showing that the translational motion of a molecule of gas accounts for only three-fifths of its kinetic energy. The total energy of the molecule (which we call "heat") includes also another factor--namely, potential energy, or energy of position, due to the work that has been done on expanding, in overcoming external pressure, and internal attraction between the molecules themselves. This potential energy (which will be recovered when the gas contracts) is the "latent heat" of Black, which so long puzzled the philosophers. It is latent in the same sense that the energy of a ball thrown into the air is latent at the moment when the ball poises at its greatest height before beginning to fall.

It thus appears that a variety of motions, real and potential, enter into the production of the condition we term heat. It is, however, chiefly the translational motion which is measurable as temperature; and this, too, which most obviously determines the physical state of the substance that the molecules collectively compose--whether, that is to say, it shall appear to our blunt perceptions as a gas, a liquid, or a solid. In the gaseous state, as we have seen, the translational motion of the molecules is relatively enormous, the molecules being widely separated. It does not follow, as we formerly supposed, that this is evidence of a repulsive power acting between the molecules. The physicists of to-day, headed by Lord Kelvin, decline to recognize any such power. They hold that the molecules of a gas fly in straight lines by virtue of their inertia, quite independently of one another, except at times of collision, from which they rebound by virtue of their elasticity; or on an approach to collision, in which latter case, coming within the range of mutual attraction, two molecules may circle about each other, as a comet circles about the sun, then rush apart again, as the comet rushes from the sun.

It is obvious that the length of the mean free path of the molecules of a gas may be increased indefinitely by decreasing the number of the molecules themselves in a circumscribed space. It has been shown by Professors Tait and Dewar that a vacuum may be produced artificially of such a degree of rarefaction that the mean free path of the remaining molecules is measurable in inches. The calculation is based on experiments made with the radiometer of Professor Crookes, an instrument which in itself is held to demonstrate the truth of the kinetic theory of gases. Such an attenuated gas as this is considered by Professor Crookes as constituting a fourth state of matter, which he terms ultra- gaseous.

If, on the other hand, a gas is subjected to pressure, its molecules are crowded closer together, and the length of their mean free path is thus lessened. Ultimately, the pressure being sufficient, the

molecules are practically in continuous contact. Meantime the enormously increased number of collisions has set the molecules more and more actively vibrating, and the temperature of the gas has increased, as, indeed, necessarily results in accordance with the law of the conservation of energy. No amount of pressure, therefore, can suffice by itself to reduce the gas to a liquid state. It is believed that even at the centre of the sun, where the pressure is almost inconceivably great, all matter is to be regarded as really gaseous, though the molecules must be so packed together that the consistency is probably more like that of a solid.

If, however, coincidently with the application of pressure, opportunity be given for the excess of heat to be dissipated to a colder surrounding medium, the molecules, giving off their excess of energy, become relatively quiescent, and at a certain stage the gas becomes a liquid. The exact point at which this transformation occurs, however, differs enormously for different substances. In the case of water, for example, it is a temperature more than four hundred degrees above zero, centigrade; while for atmospheric air it is one hundred and ninety-four degrees centigrade below zero, or more than a hundred and fifty degrees below the point at which mercury freezes.

Be it high or low, the temperature above which any substance is always a gas, regardless of pressure, is called the critical temperature, or absolute boiling- point, of that substance. It does not follow, however, that below this point the substance is necessarily a liquid. This is a matter that will be determined by external conditions of pressure. Even far below the critical temperature the molecules have an enormous degree of activity, and tend to fly asunder, maintaining what appears to be a gaseous, but what technically is called a vaporous, condition--the distinction being that pressure alone suffices to reduce the vapor to the liquid state. Thus water may change from the gaseous to the liquid state at four hundred degrees above zero, but under conditions of ordinary atmospheric pressure it does not do so until the temperature is lowered three hundred degrees further. Below four hundred degrees, however, it is technically a vapor, not a gas; but the sole difference, it will be understood, is in the degree of molecular activity.

It thus appeared that the prevalence of water in a vaporous and liquid rather than in a "permanently" gaseous condition here on the globe is a mere incident of telluric evolution. Equally incidental is the fact that the air we breathe is "permanently" gaseous and not liquid or solid, as it might be were the earth's surface temperature to be lowered to a degree which, in the larger view, may be regarded as trifling. Between the atmospheric temperature in tropical and in arctic regions there is often a variation of more than one hundred degrees; were the temperature reduced another hundred, the point would be reached at which oxygen gas becomes a vapor, and under increased pressure would be a liquid. Thirty-seven degrees more would bring us to the critical temperature of nitrogen.

Nor is this a mere theoretical assumption; it is a determination of experimental science, quite independent of theory. The physicist in the laboratory has produced artificial conditions of temperature enabling him to change the state of the most persistent gases. Some fifty years since, when the kinetic theory was in its infancy, Faraday liquefied carbonic-acid gas, among others, and the experiments thus inaugurated have been extended by numerous more recent investigators, notably by Cailletet in Switzerland, by Pictet in France, and by Dr. Thomas. Andrews and Professor James Dewar in England. In the course of these experiments not only has air been liquefied, but hydrogen also, the most subtle of gases; and it has been made more and more apparent that gas and liquid are, as Andrews long ago asserted, "only distant stages of a long series

of continuous physical changes." Of course, if the temperature be lowered still further, the liquid becomes a solid; and this change also has been effected in the case of some of the most "permanent" gases, including air.

The degree of cold--that is, of absence of heat-- thus produced is enormous, relatively to anything of which we have experience in nature here at the earth now, yet the molecules of solidified air, for example, are not absolutely quiescent. In other words, they still have a temperature, though so very low. But it is clearly conceivable that a stage might be reached at which the molecules became absolutely quiescent, as regards either translational or vibratory motion. Such a heatless condition has been approached, but as yet not quite attained, in laboratory experiments. It is called the absolute zero of temperature, and is estimated to be equivalent to two hundred and seventy- three degrees Centigrade below the freezing-point of water, or ordinary zero.

A temperature (or absence of temperature) closely approximating this is believed to obtain in the ethereal ocean of interplanetary and interstellar space, which transmits, but is thought not to absorb, radiant energy. We here on the earth's surface are protected from exposure to this cold, which would deprive every organic thing of life almost instantaneously, solely by the thin blanket of atmosphere with which the globe is coated. It would seem as if this atmosphere, exposed to such a temperature at its surface, must there be incessantly liquefied, and thus fall back like rain to be dissolved into gas again while it still is many miles above the earth's surface. This may be the reason why its scurrying molecules have not long ago wandered off into space and left the world without protection.

But whether or not such liquefaction of the air now occurs in our outer atmosphere, there can be no question as to what must occur in its entire depth were we permanently shut off from the heating influence of the sun, as the astronomers threaten that we may be in a future age. Each molecule, not alone of the atmosphere, but of the entire earth's substance, is kept aquiver by the energy which it receives, or has received, directly or indirectly, from the sun. Left to itself, each molecule would wear out its energy and fritter it off into the space about it, ultimately running completely down, as surely as any human-made machine whose power is not from time to time restored. If, then, it shall come to pass in some future age that the sun's rays fail us, the temperature of the globe must gradually sink towards the absolute zero. That is to say, the molecules of gas which now fly about at such inconceivable speed must drop helpless to the earth; liquids must in turn become solids; and solids themselves, their molecular quivers utterly stilled, may perhaps take on properties the nature of which we cannot surmise.

Yet even then, according to the current hypothesis, the heatless molecule will still be a thing instinct with life. Its vortex whirl will still go on, uninfluenced by the dying-out of those subordinate quivers that produced the transitory effect which we call temperature. For those transitory thrills, though determining the physical state of matter as measured by our crude organs of sense, were no more than non-essential incidents; but the vortex whirl is the essence of matter itself. Some estimates as to the exact character of this intramolecular motion, together with recent theories as to the actual structure of the molecule, will claim our attention in a later volume. We shall also have occasion in another connection to make fuller inquiry as to the phenomena of low temperature.

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END OF VOL. III

A HISTORY OF SCIENCE

BY

Henry Smith Williams, M.D., LL.D.

Assisted by Edward H. Williams, M.D.

IN FIVE VOLUMES

VOLUME IV. MODERN DEVELOPMENT OF THE CHEMICAL AND BIOLOGICAL SCIENCES

As regards chronology, the epoch covered in the present volume is identical with that viewed in the preceding one. But now as regards subject matter we pass on to those diverse phases of the physical world which are the field of the chemist, and to those yet more intricate processes which have to do with living organisms. So radical are the changes here that we seem to be entering new worlds; and yet, here as before, there are intimations of the new discoveries away back in the Greek days. The solution of the problem of respiration will remind us that Anaxagoras half guessed the secret; and in those diversified studies which tell us of the Daltonian atom in its wonderful transmutations, we shall be reminded again of the Clazomenian philosopher and his successor Democritus.

Yet we should press the analogy much too far were we to intimate that the Greek of the elder day or any thinker of a more recent period had penetrated, even in the vaguest way, all of the mysteries that the nineteenth century has revealed in the fields of chemistry and biology. At the very most the insight of those great Greeks and of the wonderful seventeenth-century philosophers who so often seemed on the verge of our later discoveries did no more than vaguely anticipate their successors of this later century. To gain an accurate, really specific knowledge of the properties of elementary bodies was reserved for the chemists of a recent epoch. The vague Greek questionings as to organic evolution were world-wide from the precise inductions of a Darwin. If the mediaeval Arabian endeavored to dull the knife of the surgeon with the use of drugs, his results hardly merit to be termed even an anticipation of modern anaesthesia. And when we speak of preventive medicine--of bacteriology in all its phases--we have to do with a marvellous field of which no previous generation of men had even the slightest inkling.

All in all, then, those that lie before us are perhaps the most wonderful and the most fascinating of all the fields of science. As the chapters of the preceding book carried us out into a macrocosm of inconceivable magnitude, our present studies are to reveal a microcosm of equally inconceivable smallness. As the studies of the physicist attempted to reveal the very nature of matter and of energy, we have now to seek the solution of the yet more inscrutable problems of life and of mind.

I. THE PHLOGISTON THEORY IN CHEMISTRY

The development of the science of chemistry from the "science" of alchemy is a striking example of the complete revolution in the attitude of observers in the field of science. As has been pointed out in a preceding chapter, the alchemist, having a preconceived idea of how things should be, made all his experiments to prove his preconceived theory; while the chemist reverses this attitude of mind and bases his conceptions on the results of his laboratory experiments. In short, chemistry is what alchemy never could be, an inductive science. But this transition from one point of view to an exactly opposite one was necessarily a very slow process. Ideas that have held undisputed sway over the minds of succeeding generations for hundreds of years cannot be overthrown in a moment, unless the agent of such an overthrow be so obvious that it cannot be challenged. The rudimentary chemistry that overthrew alchemy had nothing so obvious and palpable.

The great first step was the substitution of the one principle, phlogiston, for the three principles, salt, sulphur, and mercury. We have seen how the experiment of burning or calcining such a metal as lead "destroyed" the lead as such, leaving an entirely different substance in its place, and how the original metal could be restored by the addition of wheat to the calcined product. To the alchemist this was "mortification" and "revivification" of the metal. For, as pointed out by Paracelsus, "anything that could be killed by man could also be revivified by him, although this was not possible to the things killed by God." The burning of such substances as wood, wax, oil, etc., was also looked upon as the same "killing" process, and the fact that the alchemist was unable to revivify them was regarded as simply the lack of skill on his part, and in no wise affecting the theory itself.

But the iconoclastic spirit, if not the acceptance of all the teachings, of the great Paracelsus had been gradually taking root among the better class of alchemists, and about the middle of the seventeenth century Robert Boyle (1626-1691) called attention to the possibility of making a wrong deduction from the phenomenon of the calcination of the metals, because of a very important factor, the action of the air, which was generally overlooked. And he urged his colleagues of the laboratories to give greater heed to certain other phenomena that might pass unnoticed in the ordinary calcinating process. In his work, The Sceptical Chemist, he showed the reasons for doubting the threefold constitution of matter; and in his General History of the Air advanced some novel and carefully studied theories as to the composition of the atmosphere. This was an important step, and although Boyle is not directly responsible for the phlogiston theory, it is probable that his experiments on the atmosphere influenced considerably the real founders, Becker and Stahl.

Boyle gave very definitely his idea of how he thought air might be composed. "I conjecture that the atmospherical air consists of three different kinds of corpuscles," he says; "the first, those numberless particles which, in the form of vapors or dry exhalations, ascend from the earth, water, minerals, vegetables, animals, etc.; in a word, whatever substances are elevated by the celestial or subterraneal heat, and thence diffused into the atmosphere. The second may be yet more subtle, and consist of those exceedingly minute atoms, the magnetical effluvia of the earth, with other innumerable particles sent out from the bodies of the celestial luminaries, and causing, by their influence, the idea of light in us. The third sort is its characteristic and essential property, I mean permanently elastic parts. Various hypotheses may be framed relating to the structure of these later particles of the air. They might be resembled to the springs of watches, coiled up and endeavoring to restore themselves; to wool, which, being compressed, has an elastic force; to slender wires of different substances, consistencies, lengths, and thickness; in greater curls or less, near to, or remote from each other, etc., yet all continuing springy, expansible, and compressible. Lastly, they may also be compared to the thin shavings of different kinds of wood, various in their lengths, breadth, and thickness. And this, perhaps, will seem the most eligible hypothesis, because it, in some measure, illustrates the production of the elastic particles we are considering. For no art or curious instruments are required to make these shavings whose curls are in no wise uniform, but seemingly casual; and what is more remarkable, bodies that before seemed unelastic, as beams and blocks, will afford them."[1]

Although this explanation of the composition of the air is most crude, it had the effect of directing attention to the fact that the atmosphere is not "mere nothingness," but a "something" with a definite composition, and this served as a good foundation for future investigations. To be sure,

Boyle was neither the first nor the only chemist who had suspected that the air was a mixture of gases, and not a simple one, and that only certain of these gases take part in the process of calcination. Jean Rey, a French physician, and John Mayow, an Englishman, had preformed experiments which showed conclusively that the air was not a simple substance; but Boyle's work was better known, and in its effect probably more important. But with all Boyle's explanations of the composition of air, he still believed that there was an inexplicable something, a "vital substance," which he was unable to fathom, and which later became the basis of Stahl's phlogiston theory. Commenting on this mysterious substance, Boyle says: "The, difficulty we find in keeping flame and fire alive, though but for a little time, without air, renders it suspicious that there be dispersed through the rest of the atmosphere some odd substance, either of a solar, astral, or other foreign nature; on account of which the air is so necessary to the substance of flame!" It was this idea that attracted the attention of George Ernst Stahl (1660-1734), a professor of medicine in the University of Halle, who later founded his new theory upon it. Stahl's theory was a development of an earlier chemist, Johann Joachim Becker (1635-1682), in whose footsteps he followed and whose experiments he carried further.

In many experiments Stahl had been struck with the fact that certain substances, while differing widely, from one another in many respects, were alike in combustibility. From this he argued that all combustible substances must contain a common principle, and this principle he named phlogiston. This phlogiston he believed to be intimately associated in combination with other substances in nature, and in that condition not perceivable by the senses; but it was supposed to escape as a substance burned, and become apparent to the senses as fire or flame. In other words, phlogiston was something imprisoned in a combustible structure (itself forming part of the structure), and only liberated when this structure was destroyed. Fire, or flame, was FREE phlogiston, while the imprisoned phlogiston was called COMBINED PHLOGISTON, or combined fire. The peculiar quality of this strange substance was that it disliked freedom and was always striving to conceal itself in some combustible substance. Boyle's tentative suggestion that heat was simply motion was apparently not accepted by Stahl, or perhaps it was unknown to him.

According to the phlogistic theory, the part remaining after a substance was burned was simply the original substance deprived of phlogiston. To restore the original combustible substance, it was necessary to heat the residue of the combustion with something that burned easily, so that the freed phlogiston might again combine with the ashes. This was explained by the supposition that the more combustible a substance was the more phlogiston it contained, and since free phlogiston sought always to combine with some suitable substance, it was only necessary to mix the phlogisticating agents, such as charcoal, phosphorus, oils, fats, etc., with the ashes of the original substance, and heat the mixture, the phlogiston thus freed uniting at once with the ashes. This theory fitted very nicely as applied to the calcined lead revivified by the grains of wheat, although with some other products of calcination it did not seem to apply at all.

It will be seen from this that the phlogistic theory was a step towards chemistry and away from alchemy. It led away from the idea of a "spirit" in metals that could not be seen, felt, or appreciated by any of the senses, and substituted for it a principle which, although a falsely conceived one, was still much more tangible than the "spirit," since it could be seen and felt as free phlogiston and weighed and measured as combined phlogiston. The definiteness of the statement that a metal, for example, was composed of phlogiston and an element was much less enigmatic, even if wrong,

than the statement of the alchemist that "metals are produced by the spiritual action of the three principles, salt, mercury, sulphur"--particularly when it is explained that salt, mercury, and sulphur were really not what their names implied, and that there was no universally accepted belief as to what they really were.

The metals, which are now regarded as elementary bodies, were considered compounds by the phlogistians, and they believed that the calcining of a metal was a process of simplification. They noted, however, that the remains of calcination weighed more than the original product, and the natural inference from this would be that the metal must have taken in some substance rather than have given off anything. But the phlogistians had not learned the all-important significance of weights, and their explanation of variation in weight was either that such gain or loss was an unimportant "accident" at best, or that phlogiston, being light, tended to lighten any substance containing it, so that driving it out of the metal by calcination naturally left the residue heavier.

At first the phlogiston theory seemed to explain in an indisputable way all the known chemical phenomena. Gradually, however, as experiments multiplied, it became evident that the plain theory as stated by Stahl and his followers failed to explain satisfactorily certain laboratory reactions. To meet these new conditions, certain modifications were introduced from time to time, giving the theory a flexibility that would allow it to cover all cases. But as the number of inexplicable experiments continued to increase, and new modifications to the theory became necessary, it was found that some of these modifications were directly contradictory to others, and thus the simple theory became too cumbersome from the number of its modifications. Its supporters disagreed among themselves, first as to the explanation of certain phenomena that did not seem to accord with the phlogistic theory, and a little later as to the theory itself. But as yet there was no satisfactory substitute for this theory, which, even if unsatisfactory, seemed better than anything that had gone before or could be suggested.

But the good effects of the era of experimental research, to which the theory of Stahl had given such an impetus, were showing in the attitude of the experimenters. The works of some of the older writers, such as Boyle and Hooke, were again sought out in their dusty corners and consulted, and their surmises as to the possible mixture of various gases in the air were more carefully considered. Still the phlogiston theory was firmly grounded in the minds of the philosophers, who can hardly be censured for adhering to it, at least until some satisfactory substitute was offered. The foundation for such a theory was finally laid, as we shall see presently, by the work of Black, Priestley, Cavendish, and Lavoisier, in the eighteenth century, but the phlogiston theory cannot be said to have finally succumbed until the opening years of the nineteenth century.

II. THE BEGINNINGS OF MODERN CHEMISTRY THE "PNEUMATIC" CHEMISTS

Modern chemistry may be said to have its beginning with the work of Stephen Hales (1677-1761), who early in the eighteenth century began his important study of the elasticity of air. Departing from the point of view of most of the scientists of the time, be considered air to be "a fine elastic fluid, with particles of very different nature floating in it" ; and he showed that these "particles"

could be separated. He pointed out, also, that various gases, or "airs," as he called them, were contained in many solid substances. The importance of his work, however, lies in the fact that his general studies were along lines leading away from the accepted doctrines of the time, and that they gave the impetus to the investigation of the properties of gases by such chemists as Black, Priestley, Cavendish, and Lavoisier, whose specific discoveries are the foundation-stones of modern chemistry.

JOSEPH BLACK

The careful studies of Hales were continued by his younger confrere, Dr. Joseph Black (1728-1799), whose experiments in the weights of gases and other chemicals were first steps in quantitative chemistry. But even more important than his discoveries of chemical properties in general was his discovery of the properties of carbonic-acid gas.

Black had been educated for the medical profession in the University of Glasgow, being a friend and pupil of the famous Dr. William Cullen. But his liking was for the chemical laboratory rather than for the practice of medicine. Within three years after completing his medical course, and when only twenty-three years of age, he made the discovery of the properties of carbonic acid, which he called by the name of "fixed air." After discovering this gas, Black made a long series of experiments, by which he was able to show how widely it was distributed throughout nature. Thus, in 1757, be discovered that the bubbles given off in the process of brewing, where there was vegetable fermentation, were composed of it. To prove this, he collected the contents of these bubbles in a bottle containing lime-water. When this bottle was shaken violently, so that the lime-water and the carbonic acid became thoroughly mixed, an insoluble white powder was precipitated from the solution, the carbonic acid having combined chemically with the lime to form the insoluble calcium carbonate, or chalk. This experiment suggested another. Fixing a piece of burning charcoal in the end of a bellows, he arranged a tube so that the gas coming from the charcoal would pass through the lime-water, and, as in the case of the bubbles from the brewer's vat, he found that the white precipitate was thrown down; in short, that carbonic acid was given off in combustion. Shortly after, Black discovered that by blowing through a glass tube inserted into lime-water, chalk was precipitated, thus proving that carbonic acid was being constantly thrown off in respiration.

The effect of Black's discoveries was revolutionary, and the attitude of mind of the chemists towards gases, or "airs," was changed from that time forward. Most of the chemists, however, attempted to harmonize the new facts with the older theories--to explain all the phenomena on the basis of the phlogiston theory, which was still dominant. But while many of Black's discoveries could not be made to harmonize with that theory, they did not directly overthrow it. It required the additional discoveries of some of Black's fellow-scientists to complete its downfall, as we shall see.

HENRY CAVENDISH

This work of Black's was followed by the equally important work of his former pupil, Henry Cavendish (1731-1810), whose discovery of the composition of many substances, notably of nitric acid and of water, was of great importance, adding another link to the important chain of evidence

against the phlogiston theory. Cavendish is one of the most eccentric figures in the history of science, being widely known in his own time for his immense wealth and brilliant intellect, and also for his peculiarities and his morbid sensibility, which made him dread society, and probably did much in determining his career. Fortunately for him, and incidentally for the cause of science, he was able to pursue laboratory investigations without being obliged to mingle with his dreaded fellow-mortals, his every want being provided for by the immense fortune inherited from his father and an uncle.

When a young man, as a pupil of Dr. Black, he had become imbued with the enthusiasm of his teacher, continuing Black's investigations as to the properties of carbonic-acid gas when free and in combination. One of his first investigations was reported in 1766, when he communicated to the Royal Society his experiments for ascertaining the properties of carbonic-acid and hydrogen gas, in which he first showed the possibility of weighing permanently elastic fluids, although Torricelli had before this shown the relative weights of a column of air and a column of mercury. Other important experiments were continued by Cavendish, and in 1784 he announced his discovery of the composition of water, thus robbing it of its time-honored position as an "element." But his claim to priority in this discovery was at once disputed by his fellow-countryman James Watt and by the Frenchman Lavoisier. Lavoisier's claim was soon disallowed even by his own countrymen, but for many years a bitter controversy was carried on by the partisans of Watt and Cavendish. The two principals, however, seem. never to have entered into this controversy with anything like the same ardor as some of their successors, as they remained on the best of terms.[1] It is certain, at any rate, that Cavendish announced his discovery officially before Watt claimed that the announcement had been previously made by him, "and, whether right or wrong, the honor of scientific discoveries seems to be accorded naturally to the man who first publishes a demonstration of his discovery." Englishmen very generally admit the justness of Cavendish's claim, although the French scientist Arago, after reviewing the evidence carefully in 1833, decided in favor of Watt.

It appears that something like a year before Cavendish made known his complete demonstration of the composition of water, Watt communicated to the Royal Society a suggestion that water was composed of "dephlogisticated air (oxygen) and phlogiston (hydrogen) deprived of part of its latent heat." Cavendish knew of the suggestion, but in his experiments refuted the idea that the hydrogen lost any of its latent heat. Furthermore, Watt merely suggested the possible composition without proving it, although his idea was practically correct, if we can rightly interpret the vagaries of the nomenclature then in use. But had Watt taken the steps to demonstrate his theory, the great "Water Controversy" would have been avoided. Cavendish's report of his discovery to the Royal Society covers something like forty pages of printed matter. In this he shows how, by passing an electric spark through a closed jar containing a mixture of hydrogen gas and oxygen, water is invariably formed, apparently by the union of the two gases. The experiment was first tried with hydrogen and common air, the oxygen of the air uniting with the hydrogen to form water, leaving the nitrogen of the air still to be accounted for. With pure oxygen and hydrogen, however, Cavendish found that pure water was formed, leaving slight traces of any other, substance which might not be interpreted as being Chemical impurities. There was only one possible explanation of this phenomenon--that hydrogen and oxygen, when combined, form water.

"By experiments with the globe it appeared," wrote Cavendish, "that when inflammable and common air are exploded in a proper proportion, almost all the inflammable air, and near one-fifth

the common air, lose their elasticity and are condensed into dew. And by this experiment it appears that this dew is plain water, and consequently that almost all the inflammable air is turned into pure water.

"In order to examine the nature of the matter condensed on firing a mixture of dephlogisticated and inflammable air, I took a glass globe, holding 8800 grain measures, furnished with a brass cock and an apparatus for firing by electricity. This globe was well exhausted by an air-pump, and then filled with a mixture of inflammable and dephlogisticated air by shutting the cock, fastening the bent glass tube into its mouth, and letting up the end of it into a glass jar inverted into water and containing a mixture of 19,500 grain measures of dephlogisticated air, and 37,000 of inflammable air; so that, upon opening the cock, some of this mixed air rushed through the bent tube and filled the globe. The cock was then shut and the included air fired by electricity, by means of which almost all of it lost its elasticity (was condensed into water vapors). The cock was then again opened so as to let in more of the same air to supply the place of that destroyed by the explosion, which was again fired, and the operation continued till almost the whole of the mixture was let into the globe and exploded. By this means, though the globe held not more than a sixth part of the mixture, almost the whole of it was exploded therein without any fresh exhaustion of the globe."

At first this condensed matter was "acid to the taste and contained two grains of nitre," but Cavendish, suspecting that this was due to impurities, tried another experiment that proved conclusively that his opinions were correct. "I therefore made another experiment," he says, "with some more of the same air from plants in which the proportion of inflammable air was greater, so that the burnt air was almost completely phlogisticated, its standard being one-tenth. The condensed liquor was then not at all acid, but seemed pure water."

From these experiments he concludes "that when a mixture of inflammable and dephlogisticated air is exploded, in such proportions that the burnt air is not much phlogisticated, the condensed liquor contains a little acid which is always of the nitrous kind, whatever substance the dephlogisticated air is procured from; but if the proportion be such that the burnt air is almost entirely phlogisticated, the condensed liquor is not at all acid, but seems pure water, without any addition whatever."[2]

These same experiments, which were undertaken to discover the composition of water, led him to discover also the composition of nitric acid. He had observed that, in the combustion of hydrogen gas with common air, the water was slightly tinged with acid, but that this was not the case when pure oxygen gas was used. Acting upon this observation, he devised an experiment to determine the nature of this acid. He constructed an apparatus whereby an electric spark was passed through a vessel containing common air. After this process had been carried on for several weeks a small amount of liquid was formed. This liquid combined with a solution of potash to form common nitre, which "detonated with charcoal, sparkled when paper impregnated with it was burned, and gave out nitrous fumes when sulphuric acid was poured on it." In other words, the liquid was shown to be nitric acid. Now, since nothing but pure air had been used in the initial experiment, and since air is composed of nitrogen and oxygen, there seemed no room to doubt that nitric acid is a combination of nitrogen and oxygen.

This discovery of the nature of nitric acid seems to have been about the last work of importance that Cavendish did in the field of chemistry, although almost to the hour of his death he was

constantly occupied with scientific observations. Even in the last moments of his life this habit asserted itself, according to Lord Brougham. "He died on March 10, 1810, after a short illness, probably the first, as well as the last, which he ever suffered. His habit of curious observation continued to the end. He was desirous of marking the progress of the disease and the gradual extinction of the vital powers. With these ends in view, that he might not be disturbed, he desired to be left alone. His servant, returning sooner than he had wished, was ordered again to leave the chamber of death, and when be came back a second time he found his master had expired.[3]

JOSEPH PRIESTLEY

While the opulent but diffident Cavendish was making his important discoveries, another Englishman, a poor country preacher named Joseph Priestley (1733-1804) was not only rivalling him, but, if anything, outstripping him in the pursuit of chemical discoveries. In 1761 this young minister was given a position as tutor in a nonconformist academy at Warrington, and here, for six years, he was able to pursue his studies in chemistry and electricity. In 1766, while on a visit to London, he met Benjamin Franklin, at whose suggestion he published his History of Electricity. From this time on he made steady progress in scientific investigations, keeping up his ecclesiastical duties at the same time. In 1780 he removed to Birmingham, having there for associates such scientists as James Watt, Boulton, and Erasmus Darwin.

Eleven years later, on the anniversary of the fall of the Bastile in Paris, a fanatical mob, knowing Priestley's sympathies with the French revolutionists, attacked his house and chapel, burning both and destroying a great number of valuable papers and scientific instruments. Priestley and his family escaped violence by flight, but his most cherished possessions were destroyed; and three years later he quitted England forever, removing to the United States, whose struggle for liberty he had championed. The last ten years of his life were spent at Northumberland, Pennsylvania, where he continued his scientific researches.

Early in his scientific career Priestley began investigations upon the "fixed air" of Dr. Black, and, oddly enough, he was stimulated to this by the same thing that had influenced Black--that is, his residence in the immediate neighborhood of a brewery. It was during the course of a series of experiments on this and other gases that he made his greatest discovery, that of oxygen, or "dephlogisticated air," as he called it. The story of this important discovery is probably best told in Priestley's own words:

"There are, I believe, very few maxims in philosophy that have laid firmer hold upon the mind than that air, meaning atmospheric air, is a simple elementary substance, indestructible and unalterable, at least as much so as water is supposed to be. In the course of my inquiries I was, however, soon satisfied that atmospheric air is not an unalterable thing; for that, according to my first hypothesis, the phlogiston with which it becomes loaded from bodies burning in it, and the animals breathing it, and various other chemical processes, so far alters and depraves it as to render it altogether unfit for inflammation, respiration, and other purposes to which it is subservient; and I had discovered that agitation in the water, the process of vegetation, and probably other natural processes, restore it to its original purity....

"Having procured a lens of twelve inches diameter and twenty inches local distance, I proceeded with the greatest alacrity, by the help of it, to discover what kind of air a great variety of substances would yield, putting them into the vessel, which I filled with quicksilver, and kept inverted in a basin of the same .... With this apparatus, after a variety of experiments .... on the 1st of August, 1774, I endeavored to extract air from mercurius calcinatus per se; and I presently found that, by means of this lens, air was expelled from it very readily. Having got about three or four times as much as the bulk of my materials, I admitted water to it, and found that it was not imbibed by it. But what surprised me more than I can express was that a candle burned in this air with a remarkably vigorous flame, very much like that enlarged flame with which a candle burns in nitrous oxide, exposed to iron or liver of sulphur; but as I had got nothing like this remarkable appearance from any kind of air besides this particular modification of vitrous air, and I knew no vitrous acid was used in the preparation of mercurius calcinatus, I was utterly at a loss to account for it."[4]

The "new air" was, of course, oxygen. Priestley at once proceeded to examine it by a long series of careful experiments, in which, as will be seen, he discovered most of the remarkable qualities of this gas. Continuing his description of these experiments, he says:

"The flame of the candle, besides being larger, burned with more splendor and heat than in that species of nitrous air; and a piece of red-hot wood sparkled in it, exactly like paper dipped in a solution of nitre, and it consumed very fast; an experiment that I had never thought of trying with dephlogisticated nitrous air.

". . . I had so little suspicion of the air from the mercurius calcinatus, etc., being wholesome, that I had not even thought of applying it to the test of nitrous air; but thinking (as my reader must imagine I frequently must have done) on the candle burning in it after long agitation in water, it occurred to me at last to make the experiment; and, putting one measure of nitrous air to two measures of this air, I found not only that it was diminished, but that it was diminished quite as much as common air, and that the redness of the mixture was likewise equal to a similar mixture of nitrous and common air.... The next day I was more surprised than ever I had been before with finding that, after the above-mentioned mixture of nitrous air and the air from mercurius calcinatus had stood all night, . . . a candle burned in it, even better than in common air."

A little later Priestley discovered that "dephlogisticated air . . . is a principal element in the composition of acids, and may be extracted by means of heat from many substances which contain them.... It is likewise produced by the action of light upon green vegetables; and this seems to be the chief means employed to preserve the purity of the atmosphere."

This recognition of the important part played by oxygen in the atmosphere led Priestley to make some experiments upon mice and insects, and finally upon himself, by inhalations of the pure gas. "The feeling in my lungs," he said, "was not sensibly different from that of common air, but I fancied that my breathing felt peculiarly light and easy for some time afterwards. Who can tell but that in time this pure air may become a fashionable article in luxury? . . . Perhaps we may from these experiments see that though pure dephlogisticated air might be useful as a medicine, it might not be so proper for us in the usual healthy state of the body."

This suggestion as to the possible usefulness of oxygen as a medicine was prophetic. A century later the use of oxygen had become a matter of routine practice with many physicians. Even in Priestley's own time such men as Dr. John Hunter expressed their belief in its efficacy in certain conditions, as we shall see, but its value in medicine was not fully appreciated until several generations later.

Several years after discovering oxygen Priestley thus summarized its properties: "It is this ingredient in the atmospheric air that enables it to support combustion and animal life. By means of it most intense heat may be produced, and in the purest of it animals will live nearly five times as long as in an equal quantity of atmospheric air. In respiration, part of this air, passing the membranes of the lungs, unites with the blood and imparts to it its florid color, while the remainder, uniting with phlogiston exhaled from venous blood, forms mixed air. It is dephlogisticated air combined with water that enables fishes to live in it."[5]

KARL WILHELM SCHEELE

The discovery of oxygen was the last but most important blow to the tottering phlogiston theory, though Priestley himself would not admit it. But before considering the final steps in the overthrow of Stahl's famous theory and the establishment of modern chemistry, we must review the work of another great chemist, Karl Wilhelm Scheele (1742-1786), of Sweden, who discovered oxygen quite independently, although later than Priestley. In the matter of brilliant discoveries in a brief space of time Scheele probably eclipsed all his great contemporaries. He had a veritable genius for interpreting chemical reactions and discovering new substances, in this respect rivalling Priestley himself. Unlike Priestley, however, he planned all his experiments along the lines of definite theories from the beginning, the results obtained being the logical outcome of a predetermined plan.

Scheele was the son of a merchant of Stralsund, Pomerania, which then belonged to Sweden. As a boy in school he showed so little aptitude for the study of languages that he was apprenticed to an apothecary at the age of fourteen. In this work he became at once greatly interested, and, when not attending to his duties in the dispensary, he was busy day and night making experiments or studying books on chemistry. In 1775, still employed as an apothecary, he moved to Stockholm, and soon after he sent to Bergman, the leading chemist of Sweden, his first discovery--that of tartaric acid, which he had isolated from cream of tartar. This was the beginning of his career of discovery, and from that time on until his death he sent forth accounts of new discoveries almost uninterruptedly. Meanwhile he was performing the duties of an ordinary apothecary, and struggling against poverty. His treatise upon Air and Fire appeared in 1777. In this remarkable book he tells of his discovery of oxygen--"empyreal" or "fire-air," as he calls it--which he seems to have made independently and without ever having heard of the previous discovery by Priestley. In this book, also, he shows that air is composed chiefly of oxygen and nitrogen gas.

Early in his experimental career Scheele undertook the solution of the composition of black oxide of manganese, a substance that had long puzzled the chemists. He not only succeeded in this, but incidentally in the course of this series of experiments he discovered oxygen, baryta, and chlorine, the last of far greater importance, at least commercially, than the real object of his search. In speaking of the experiment in which the discovery was made he says:

"When marine (hydrochloric) acid stood over manganese in the cold it acquired a dark reddish-brown color. As manganese does not give any colorless solution without uniting with phlogiston [probably meaning hydrogen], it follows that marine acid can dissolve it without this principle. But such a solution has a blue or red color. The color is here more brown than red, the reason being that the very finest portions of the manganese, which do not sink so easily, swim in the red solution; for without these fine particles the solution is red, and red mixed with black is brown. The manganese has here attached itself so loosely to acidum salis that the water can precipitate it, and this precipitate behaves like ordinary manganese. When, now, the mixture of manganese and spiritus salis was set to digest, there arose an effervescence and smell of aqua regis."[6]

The "effervescence" he refers to was chlorine, which he proceeded to confine in a suitable vessel and examine more fully. He described it as having a "quite characteristically suffocating smell," which was very offensive. He very soon noted the decolorizing or bleaching effects of this now product, finding that it decolorized flowers, vegetables, and many other substances.

Commercially this discovery of chlorine was of enormous importance, and the practical application of this new chemical in bleaching cloth soon supplanted the, old process of crofting--that is, bleaching by spreading the cloth upon the grass. But although Scheele first pointed out the bleaching quality of his newly discovered gas, it was the French savant, Berthollet, who, acting upon Scheele's discovery that the new gas would decolorize vegetables and flowers, was led to suspect that this property might be turned to account in destroying the color of cloth. In 1785 he read a paper before the Academy of Sciences of Paris, in which he showed that bleaching by chlorine was entirely satisfactory, the color but not the substance of the cloth being affected. He had experimented previously and found that the chlorine gas was soluble in water and could thus be made practically available for bleaching purposes. In 1786 James Watt examined specimens of the bleached cloth made by Berthollet, and upon his return to England first instituted the process of practical bleaching. His process, however, was not entirely satisfactory, and, after undergoing various modifications and improvements, it was finally made thoroughly practicable by Mr. Tennant, who hit upon a compound of chlorine and lime--the chloride of lime--which was a comparatively cheap chemical product, and answered the purpose better even than chlorine itself.

To appreciate how momentous this discovery was to cloth manufacturers, it should be remembered that the old process of bleaching consumed an entire summer for the whitening of a single piece of linen; the new process reduced the period to a few hours. To be sure, lime had been used with fair success previous to Tennant's discovery, but successful and practical bleaching by a solution of chloride of lime was first made possible by him and through Scheele's discovery of chlorine.

Until the time of Scheele the great subject of organic chemistry had remained practically unexplored, but under the touch of his marvellous inventive genius new methods of isolating and studying animal and vegetable products were introduced, and a large number of acids and other organic compounds prepared that had been hitherto unknown. His explanations of chemical phenomena were based on the phlogiston theory, in which, like Priestley, he always, believed. Although in error in this respect, he was, nevertheless, able to make his discoveries with extremely accurate interpretations. A brief epitome of the list of some of his more important discoveries conveys some idea, of his fertility of mind as well as his industry. In 1780 he discovered lactic

acid,[7] and showed that it was the substance that caused the acidity of sour milk; and in the same year he discovered mucic acid. Next followed the discovery of tungstic acid, and in 1783 he added to his list of useful discoveries that of glycerine. Then in rapid succession came his announcements of the new vegetable products citric, malic, oxalic, and gallic acids. Scheele not only made the discoveries, but told the world how he had made them--how any chemist might have made them if he chose--for he never considered that he had really discovered any substance until he had made it, decomposed it, and made it again.

His experiments on Prussian blue are most interesting, not only because of the enormous amount of work involved and the skill he displayed in his experiments, but because all the time the chemist was handling, smelling, and even tasting a compound of one of the most deadly poisons, ignorant of the fact that the substance was a dangerous one to handle. His escape from injury seems almost miraculous; for his experiments, which were most elaborate, extended over a considerable period of time, during which he seems to have handled this chemical with impunity.

While only forty years of age and just at the zenith of his fame, Scheele was stricken by a fatal illness, probably induced by his ceaseless labor and exposure. It is gratifying to know, however, that during the last eight or nine years of his life he had been less bound down by pecuniary difficulties than before, as Bergman had obtained for him an annual grant from the Academy. But it was characteristic of the man that, while devoting one-sixth of the amount of this grant to his personal wants, the remaining five-sixths was devoted to the expense of his experiments.

LAVOISIER AND THE FOUNDATION OF MODERN CHEMISTRY

The time was ripe for formulating the correct theory of chemical composition: it needed but the master hand to mould the materials into the proper shape. The discoveries in chemistry during the eighteenth century had been far-reaching and revolutionary in character. A brief review of these discoveries shows how completely they had subverted the old ideas of chemical elements and chemical compounds. Of the four substances earth, air, fire, and water, for many centuries believed to be elementary bodies, not one has stood the test of the eighteenth-century chemists. Earth had long since ceased to be regarded as an element, and water and air had suffered the same fate in this century. And now at last fire itself, the last of the four "elements" and the keystone to the phlogiston arch, was shown to be nothing more than one of the manifestations of the new element, oxygen, and not "phlogiston" or any other intangible substance.

In this epoch of chemical discoveries England had produced such mental giants and pioneers in science as Black, Priestley, and Cavendish; Sweden had given the world Scheele and Bergman, whose work, added to that of their English confreres, had laid the broad base of chemistry as a science; but it was for France to produce a man who gave the final touches to the broad but rough workmanship of its foundation, and establish it as the science of modern chemistry. It was for Antoine Laurent Lavoisier (1743-1794) to gather together, interpret correctly, rename, and classify the wealth of facts that his immediate predecessors and contemporaries had given to the world.

The attitude of the mother-countries towards these illustrious sons is an interesting piece of history. Sweden honored and rewarded Scheele and Bergman for their efforts; England received the

intellectuality of Cavendish with less appreciation than the Continent, and a fanatical mob drove Priestley out of the country; while France, by sending Lavoisier to the guillotine, demonstrated how dangerous it was, at that time at least, for an intelligent Frenchman to serve his fellowman and his country well.

"The revolution brought about by Lavoisier in science," says Hoefer, "coincides by a singular act of destiny with another revolution, much greater indeed, going on then in the political and social world. Both happened on the same soil, at the same epoch, among the same people; and both marked the commencement of a new era in their respective spheres."[8]

Lavoisier was born in Paris, and being the son of an opulent family, was educated under the instruction of the best teachers of the day. With Lacaille he studied mathematics and astronomy; with Jussieu, botany; and, finally, chemistry under Rouelle. His first work of importance was a paper on the practical illumination of the streets of Paris, for which a prize had been offered by M. de Sartine, the chief of police. This prize was not awarded to Lavoisier, but his suggestions were of such importance that the king directed that a gold medal be bestowed upon the young author at the public sitting of the Academy in April, 1776. Two years later, at the age of thirty-five, Lavoisier was admitted a member of the Academy.

In this same year he began to devote himself almost exclusively to chemical inquiries, and established a laboratory in his home, fitted with all manner of costly apparatus and chemicals. Here he was in constant communication with the great men of science of Paris, to all of whom his doors were thrown open. One of his first undertakings in this laboratory was to demonstrate that water could not be converted into earth by repeated distillations, as was generally advocated; and to show also that there was no foundation to the existing belief that it was possible to convert water into a gas so "elastic" as to pass through the pores of a vessel. He demonstrated the fallaciousness of both these theories in 1768-1769 by elaborate experiments, a single investigation of this series occupying one hundred and one days.

In 1771 he gave the first blow to the phlogiston theory by his experiments on the calcination of metals. It will be recalled that one basis for the belief in phlogiston was the fact that when a metal was calcined it was converted into an ash, giving up its "phlogiston" in the process. To restore the metal, it was necessary to add some substance such as wheat or charcoal to the ash. Lavoisier, in examining this process of restoration, found that there was always evolved a great quantity of "air," which he supposed to be "fixed air" or carbonic acid--the same that escapes in effervescence of alkalies and calcareous earths, and in the fermentation of liquors. He then examined the process of calcination, whereby the phlogiston of the metal was supposed to have been drawn off. But far from finding that phlogiston or any other substance had been driven off, he found that something had been taken on: that the metal "absorbed air," and that the increased weight of the metal corresponded to the amount of air "absorbed." Meanwhile he was within grasp of two great discoveries, that of oxygen and of the composition of the air, which Priestley made some two years later.

The next important inquiry of this great Frenchman was as to the composition of diamonds. With the great lens of Tschirnhausen belonging to the Academy he succeeded in burning up several diamonds, regardless of expense, which, thanks to his inheritance, he could ignore. In this process

he found that a gas was given off which precipitated lime from water, and proved to be carbonic acid. Observing this, and experimenting with other substances known to give off carbonic acid in the same manner, he was evidently impressed with the now well-known fact that diamond and charcoal are chemically the same. But if he did really believe it, he was cautious in expressing his belief fully. "We should never have expected," he says, "to find any relation between charcoal and diamond, and it would be unreasonable to push this analogy too far; it only exists because both substances seem to be properly ranged in the class of combustible bodies, and because they are of all these bodies the most fixed when kept from contact with air."

As we have seen, Priestley, in 1774, had discovered oxygen, or "dephlogisticated air." Four years later Lavoisier first advanced his theory that this element discovered by Priestley was the universal acidifying or oxygenating principle, which, when combined with charcoal or carbon, formed carbonic acid; when combined with sulphur, formed sulphuric (or vitriolic) acid; with nitrogen, formed nitric acid, etc., and when combined with the metals formed oxides, or calcides. Furthermore, he postulated the theory that combustion was not due to any such illusive thing as "phlogiston," since this did not exist, and it seemed to him that the phenomena of combustion heretofore attributed to phlogiston could be explained by the action of the new element oxygen and heat. This was the final blow to the phlogiston theory, which, although it had been tottering for some time, had not been completely overthrown.

In 1787 Lavoisier, in conjunction with Guyon de Morveau, Berthollet, and Fourcroy, introduced the reform in chemical nomenclature which until then had remained practically unchanged since alchemical days. Such expressions as "dephlogisticated" and "phlogisticated" would obviously have little meaning to a generation who were no longer to believe in the existence of phlogiston. It was appropriate that a revolution in chemical thought should be accompanied by a corresponding revolution in chemical names, and to Lavoisier belongs chiefly the credit of bringing about this revolution. In his Elements of Chemistry he made use of this new nomenclature, and it seemed so clearly an improvement over the old that the scientific world hastened to adopt it. In this connection Lavoisier says: "We have, therefore, laid aside the expression metallic calx altogether, and have substituted in its place the word oxide. By this it may be seen that the language we have adopted is both copious and expressive. The first or lowest degree of oxygenation in bodies converts them into oxides; a second degree of additional oxygenation constitutes the class of acids of which the specific names drawn from their particular bases terminate in ous, as in the nitrous and the sulphurous acids. The third degree of oxygenation changes these into the species of acids distinguished by the termination in ic, as the nitric and sulphuric acids; and, lastly, we can express a fourth or higher degree of oxygenation by adding the word oxygenated to the name of the acid, as has already been done with oxygenated muriatic acid."[9]

This new work when given to the world was not merely an epoch-making book; it was revolutionary. It not only discarded phlogiston altogether, but set forth that metals are simple elements, not compounds of "earth" and "phlogiston." It upheld Cavendish's demonstration that water itself, like air, is a compound of oxygen with another element. In short, it was scientific chemistry, in the modern acceptance of the term.

Lavoisier's observations on combustion are at once important and interesting: "Combustion," he says, ". . . is the decomposition of oxygen produced by a combustible body. The oxygen which

forms the base of this gas is absorbed by and enters into combination with the burning body, while the caloric and light are set free. Every combustion necessarily supposes oxygenation; whereas, on the contrary, every oxygenation does not necessarily imply concomitant combustion; because combustion properly so called cannot take place without disengagement of caloric and light. Before combustion can take place, it is necessary that the base of oxygen gas should have greater affinity to the combustible body than it has to caloric; and this elective attraction, to use Bergman's expression, can only take place at a certain degree of temperature which is different for each combustible substance; hence the necessity of giving the first motion or beginning to every combustion by the approach of a heated body. This necessity of heating any body we mean to burn depends upon certain considerations which have not hitherto been attended to by any natural philosopher, for which reason I shall enlarge a little upon the subject in this place:

"Nature is at present in a state of equilibrium, which cannot have been attained until all the spontaneous combustions or oxygenations possible in an ordinary degree of temperature had taken place.... To illustrate this abstract view of the matter by example: Let us suppose the usual temperature of the earth a little changed, and it is raised only to the degree of boiling water; it is evident that in this case phosphorus, which is combustible in a considerably lower degree of temperature, would no longer exist in nature in its pure and simple state, but would always be procured in its acid or oxygenated state, and its radical would become one of the substances unknown to chemistry. By gradually increasing the temperature of the earth, the same circumstance would successively happen to all the bodies capable of combustion; and, at the last, every possible combustion having taken place, there would no longer exist any combustible body whatever, and every substance susceptible of the operation would be oxygenated and consequently incombustible.

"There cannot, therefore, exist, as far as relates to us, any combustible body but such as are non-combustible at the ordinary temperature of the earth, or, what is the same thing in other words, that it is essential to the nature of every combustible body not to possess the property of combustion unless heated, or raised to a degree of temperature at which its combustion naturally takes place. When this degree is once produced, combustion commences, and the caloric which is disengaged by the decomposition of the oxygen gas keeps up the temperature which is necessary for continuing combustion. When this is not the case--that is, when the disengaged caloric is not sufficient for keeping up the necessary temperature--the combustion ceases. This circumstance is expressed in the common language by saying that a body burns ill or with difficulty."[10]

It needed the genius of such a man as Lavoisier to complete the refutation of the false but firmly grounded phlogiston theory, and against such a book as his Elements of Chemistry the feeble weapons of the supporters of the phlogiston theory were hurled in vain.

But while chemists, as a class, had become converts to the new chemistry before the end of the century, one man, Dr. Priestley, whose work had done so much to found it, remained unconverted. In this, as in all his life-work, he showed himself to be a most remarkable man. Davy said of him, a generation later, that no other person ever discovered so many new and curious substances as he; yet to the last he was only an amateur in science, his profession, as we know, being the ministry. There is hardly another case in history of a man not a specialist in science accomplishing so much in original research as did this chemist, physiologist, electrician; the mathematician, logician, and moralist; the theologian, mental philosopher, and political economist. He took all knowledge for his

field; but how he found time for his numberless researches and multifarious writings, along with his every-day duties, must ever remain a mystery to ordinary mortals.

That this marvellously receptive, flexible mind should have refused acceptance to the clearly logical doctrines of the new chemistry seems equally inexplicable. But so it was. To the very last, after all his friends had capitulated, Priestley kept up the fight. From America he sent out his last defy to the enemy, in 1800, in a brochure entitled "The Doctrine of Phlogiston Upheld," etc. In the mind of its author it was little less than a paean of victory; but all the world beside knew that it was the swan-song of the doctrine of phlogiston. Despite the defiance of this single warrior the battle was really lost and won, and as the century closed "antiphlogistic" chemistry had practical possession of the field.

III. CHEMISTRY SINCE THE TIME OF DALTON

JOHN DALTON AND THE ATOMIC THEORY

Small beginnings as have great endings--sometimes. As a case in point, note what came of the small, original effort of a self-trained back-country Quaker youth named John Dalton, who along towards the close of the eighteenth century became interested in the weather, and was led to construct and use a crude water-gauge to test the amount of the rainfall. The simple experiments thus inaugurated led to no fewer than two hundred thousand recorded observations regarding the weather, which formed the basis for some of the most epochal discoveries in meteorology, as we have seen. But this was only a beginning. The simple rain-gauge pointed the way to the most important generalization of the nineteenth century in a field of science with which, to the casual observer, it might seem to have no alliance whatever. The wonderful theory of atoms, on which the whole gigantic structure of modern chemistry is founded, was the logical outgrowth, in the mind of John Dalton, of those early studies in meteorology.

The way it happened was this: From studying the rainfall, Dalton turned naturally to the complementary process of evaporation. He was soon led to believe that vapor exists, in the atmosphere as an independent gas. But since two bodies cannot occupy the same space at the same time, this implies that the various atmospheric gases are really composed of discrete particles. These ultimate particles are so small that we cannot see them--cannot, indeed, more than vaguely imagine them--yet each particle of vapor, for example, is just as much a portion of water as if it were a drop out of the ocean, or, for that matter, the ocean itself. But, again, water is a compound substance, for it may be separated, as Cavendish has shown, into the two elementary substances hydrogen and oxygen. Hence the atom of water must be composed of two lesser atoms joined together. Imagine an atom of hydrogen and one of oxygen. Unite them, and we have an atom of water; sever them, and the water no longer exists; but whether united or separate the atoms of hydrogen and of oxygen remain hydrogen and oxygen and nothing else. Differently mixed together or united, atoms produce different gross substances; but the elementary atoms never change their chemical nature--their distinct personality.

It was about the year 1803 that Dalton first gained a full grasp of the conception of the chemical atom. At once he saw that the hypothesis, if true, furnished a marvellous key to secrets of matter hitherto insoluble--questions relating to the relative proportions of the atoms themselves. It is known, for example, that a certain bulk of hydrogen gas unites with a certain bulk of oxygen gas to form water. If it be true that this combination consists essentially of the union of atoms one with another (each single atom of hydrogen united to a single atom of oxygen), then the relative weights of the original masses of hydrogen and of oxygen must be also the relative weights of each of their respective atoms. If one pound of hydrogen unites with five and one-half pounds of oxygen (as, according to Dalton's experiments, it did), then the weight of the oxygen atom must be five and one-half times that of the hydrogen atom. Other compounds may plainly be tested in the same way. Dalton made numerous tests before he published his theory. He found that hydrogen enters into compounds in smaller proportions than any other element known to him, and so, for convenience, determined to take the weight of the hydrogen atom as unity. The atomic weight of oxygen then becomes (as given in Dalton's first table of 1803) 5.5; that of water (hydrogen plus oxygen) being of course 6.5. The atomic weights of about a score of substances are given in Dalton's first paper, which was read before the Literary and Philosophical Society of Manchester, October 21, 1803. I wonder if Dalton himself, great and acute intellect though he had, suspected, when he read that paper, that he was inaugurating one of the most fertile movements ever entered on in the whole history of science?

Be that as it may, it is certain enough that Dalton's contemporaries were at first little impressed with the novel atomic theory. Just at this time, as it chanced, a dispute was waging in the field of chemistry regarding a matter of empirical fact which must necessarily be settled before such a theory as that of Dalton could even hope for a bearing. This was the question whether or not chemical elements unite with one another always in definite proportions. Berthollet, the great co-worker with Lavoisier, and now the most authoritative of living chemists, contended that substances combine in almost indefinitely graded proportions between fixed extremes. He held that solution is really a form of chemical combination--a position which, if accepted, left no room for argument.

But this contention of the master was most actively disputed, in particular by Louis Joseph Proust, and all chemists of repute were obliged to take sides with one or the other. For a time the authority of Berthollet held out against the facts, but at last accumulated evidence told for Proust and his followers, and towards the close of the first decade of our century it came to be generally conceded that chemical elements combine with one another in fixed and definite proportions.

More than that. As the analysts were led to weigh carefully the quantities of combining elements, it was observed that the proportions are not only definite, but that they bear a very curious relation to one another. If element A combines with two different proportions of element B to form two compounds, it appears that the weight of the larger quantity of B is an exact multiple of that of the smaller quantity. This curious relation was noticed by Dr. Wollaston, one of the most accurate of observers, and a little later it was confirmed by Johan Jakob Berzelius, the great Swedish chemist, who was to be a dominating influence in the chemical world for a generation to come. But this combination of elements in numerical proportions was exactly what Dalton had noticed as early as

1802, and what bad led him directly to the atomic weights. So the confirmation of this essential point by chemists of such authority gave the strongest confirmation to the atomic theory.

During these same years the rising authority of the French chemical world, Joseph Louis Gay-Lussac, was conducting experiments with gases, which he had undertaken at first in conjunction with Humboldt, but which later on were conducted independently. In 1809, the next year after the publication of the first volume of Dalton's New System of Chemical Philosophy, Gay-Lussac published the results of his observations, and among other things brought out the remarkable fact that gases, under the same conditions as to temperature and pressure, combine always in definite numerical proportions as to volume. Exactly two volumes of hydrogen, for example, combine with one volume of oxygen to form water. Moreover, the resulting compound gas always bears a simple relation to the combining volumes. In the case just cited, the union of two volumes of hydrogen and one of oxygen results in precisely two volumes of water vapor.

Naturally enough, the champions of the atomic theory seized upon these observations of Gay-Lussac as lending strong support to their hypothesis--all of them, that is, but the curiously self-reliant and self-sufficient author of the atomic theory himself, who declined to accept the observations of the French chemist as valid. Yet the observations of Gay-Lussac were correct, as countless chemists since then have demonstrated anew, and his theory of combination by volumes became one of the foundation-stones of the atomic theory, despite the opposition of the author of that theory.

The true explanation of Gay-Lussac's law of combination by volumes was thought out almost immediately by an Italian savant, Amadeo, Avogadro, and expressed in terms of the atomic theory. The fact must be, said Avogadro, that under similar physical conditions every form of gas contains exactly the same number of ultimate particles in a given volume. Each of these ultimate physical particles may be composed of two or more atoms (as in the case of water vapor), but such a compound atom conducts itself as if it were a simple and indivisible atom, as regards the amount of space that separates it from its fellows under given conditions of pressure and temperature. The compound atom, composed of two or more elementary atoms, Avogadro proposed to distinguish, for purposes of convenience, by the name molecule. It is to the molecule, considered as the unit of physical structure, that Avogadro's law applies.

This vastly important distinction between atoms and molecules, implied in the law just expressed, was published in 1811. Four years later, the famous French physicist Ampere outlined a similar theory, and utilized the law in his mathematical calculations. And with that the law of Avogadro dropped out of sight for a full generation. Little suspecting that it was the very key to the inner mysteries of the atoms for which they were seeking, the chemists of the time cast it aside, and let it fade from the memory of their science.

This, however, was not strange, for of course the law of Avogadro is based on the atomic theory, and in 1811 the atomic theory was itself still being weighed in the balance. The law of multiple proportions found general acceptance as an empirical fact; but many of the leading lights of chemistry still looked askance at Dalton's explanation of this law. Thus Wollaston, though from the first he inclined to acceptance of the Daltonian view, cautiously suggested that it would be well to use the non-committal word "equivalent" instead of "atom"; and Davy, for a similar reason, in his

book of 1812, speaks only of "proportions," binding himself to no theory as to what might be the nature of these proportions.

At least two great chemists of the time, however, adopted the atomic view with less reservation. One of these was Thomas Thomson, professor at Edinburgh, who, in 1807, had given an outline of Dalton's theory in a widely circulated book, which first brought the theory to the general attention of the chemical world. The other and even more noted advocate of the atomic theory was Johan Jakob Berzelius. This great Swedish chemist at once set to work to put the atomic theory to such tests as might be applied in the laboratory. He was an analyst of the utmost skill, and for years be devoted himself to the determination of the combining weights, "equivalents" or "proportions," of the different elements. These determinations, in so far as they were accurately made, were simple expressions of empirical facts, independent of any theory; but gradually it became more and more plain that these facts all harmonize with the atomic theory of Dalton. So by common consent the proportionate combining weights of the elements came to be known as atomic weights--the name Dalton had given them from the first--and the tangible conception of the chemical atom as a body of definite constitution and weight gained steadily in favor.

From the outset the idea had had the utmost tangibility in the mind of Dalton. He had all along represented the different atoms by geometrical symbols--as a circle for oxygen, a circle enclosing a dot for hydrogen, and the like--and had represented compounds by placing these symbols of the elements in juxtaposition. Berzelius proposed to improve upon this method by substituting for the geometrical symbol the initial of the Latin name of the element represented--O for oxygen, H for hydrogen, and so on--a numerical coefficient to follow the letter as an indication of the number of atoms present in any given compound. This simple system soon gained general acceptance, and with slight modifications it is still universally employed. Every school-boy now is aware that H2O is the chemical way of expressing the union of two atoms of hydrogen with one of oxygen to form a molecule of water. But such a formula would have had no meaning for the wisest chemist before the day of Berzelius.

The universal fame of the great Swedish authority served to give general currency to his symbols and atomic weights, and the new point of view thus developed led presently to two important discoveries which removed the last lingering doubts as to the validity of the atomic theory. In 1819 two French physicists, Dulong and Petit, while experimenting with heat, discovered that the specific heats of solids (that is to say, the amount of heat required to raise the temperature of a given mass to a given degree) vary inversely as their atomic weights. In the same year Eilhard Mitscherlich, a German investigator, observed that compounds having the same number of atoms to the molecule are disposed to form the same angles of crystallization--a property which he called isomorphism.

Here, then, were two utterly novel and independent sets of empirical facts which harmonize strangely with the supposition that substances are composed of chemical atoms of a determinate weight. This surely could not be coincidence--it tells of law. And so as soon as the claims of Dulong and Petit and of Mitscherlich had been substantiated by other observers, the laws of the specific heat of atoms, and of isomorphism, took their place as new levers of chemical science. With the aid of these new tools an impregnable breastwork of facts was soon piled about the atomic

theory. And John Dalton, the author of that theory, plain, provincial Quaker, working on to the end in semi-retirement, became known to all the world and for all time as a master of masters.

HUMPHRY DAVY AND ELECTRO-CHEMISTRY

During those early years of the nineteenth century, when Dalton was grinding away at chemical fact and theory in his obscure Manchester laboratory, another Englishman held the attention of the chemical world with a series of the most brilliant and widely heralded researches. This was Humphry Davy, a young man who had conic to London in 1801, at the instance of Count Rumford, to assume the chair of chemical philosophy in the Royal Institution, which the famous American had just founded.

Here, under Davy's direction, the largest voltaic battery yet constructed had been put in operation, and with its aid the brilliant young experimenter was expected almost to perform miracles. And indeed he scarcely disappointed the expectation, for with the aid of his battery he transformed so familiar a substance as common potash into a metal which was not only so light that it floated on water, but possessed the seemingly miraculous property of bursting into flames as soon as it came in contact with that fire-quenching liquid. If this were not a miracle, it had for the popular eye all the appearance of the miraculous.

What Davy really had done was to decompose the potash, which hitherto had been supposed to be elementary, liberating its oxygen, and thus isolating its metallic base, which he named potassium. The same thing was done with soda, and the closely similar metal sodium was discovered--metals of a unique type, possessed of a strange avidity for oxygen, and capable of seizing on it even when it is bound up in the molecules of water. Considered as mere curiosities, these discoveries were interesting, but aside from that they were of great theoretical importance, because they showed the compound nature of some familiar chemicals that had been regarded as elements. Several other elementary earths met the same fate when subjected to the electrical influence; the metals barium, calcium, and strontium being thus discovered. Thereafter Davy always referred to the supposed elementary substances (including oxygen, hydrogen, and the rest) as "unde-compounded" bodies. These resist all present efforts to decompose them, but how can one know what might not happen were they subjected to an influence, perhaps some day to be discovered, which exceeds the battery in power as the battery exceeds the blowpipe?

Another and even more important theoretical result that flowed from Davy's experiments during this first decade of the century was the proof that no elementary substances other than hydrogen and oxygen are produced when pure water is decomposed by the electric current. It was early noticed by Davy and others that when a strong current is passed through water, alkalies appear at one pole of the battery and acids at the other, and this though the water used were absolutely pure. This seemingly told of the creation of elements--a transmutation but one step removed from the creation of matter itself--under the influence of the new "force." It was one of Davy's greatest triumphs to prove, in the series of experiments recorded in his famous Bakerian lecture of 1806, that the alleged creation of elements did not take place, the substances found at the poles of the battery having been dissolved from the walls of the vessels in which the water experimented upon had been placed. Thus the same implement which had served to give a certain philosophical

warrant to the fading dreams of alchemy banished those dreams peremptorily from the domain of present science.

"As early as 1800," writes Davy, "I had found that when separate portions of distilled water, filling two glass tubes, connected by moist bladders, or any moist animal or vegetable substances, were submitted to the electrical action of the pile of Volta by means of gold wires, a nitro-muriatic solution of gold appeared in the tube containing the positive wire, or the wire transmitting the electricity, and a solution of soda in the opposite tube; but I soon ascertained that the muriatic acid owed its existence to the animal or vegetable matters employed; for when the same fibres of cotton were made use of in successive experiments, and washed after every process in a weak solution of nitric acid, the water in the apparatus containing them, though acted on for a great length of time with a very strong power, at last produced no effects upon nitrate of silver.

"In cases when I had procured much soda, the glass at its point of contact with the wire seemed considerably corroded; and I was confirmed in my idea of referring the production of the alkali principally to this source, by finding that no fixed saline matter could be obtained by electrifying distilled water in a single agate cup from two points of platina with the Voltaic battery.

"Mr. Sylvester, however, in a paper published in Mr. Nicholson's journal for last August, states that though no fixed alkali or muriatic acid appears when a single vessel is employed, yet that they are both formed when two vessels are used. And to do away with all objections with regard to vegetable substances or glass, he conducted his process in a vessel made of baked tobacco-pipe clay inserted in a crucible of platina. I have no doubt of the correctness of his results; but the conclusion appears objectionable. He conceives, that he obtained fixed alkali, because the fluid after being heated and evaporated left a matter that tinged turmeric brown, which would have happened had it been lime, a substance that exists in considerable quantities in all pipe-clay; and even allowing the presence of fixed alkali, the materials employed for the manufacture of tobacco-pipes are not at all such as to exclude the combinations of this substance.

"I resumed the inquiry; I procured small cylindrical cups of agate of the capacity of about one-quarter of a cubic inch each. They were boiled for some hours in distilled water, and a piece of very white and transparent amianthus that had been treated in the same way was made then to connect together; they were filled with distilled water and exposed by means of two platina wires to a current of electricity, from one hundred and fifty pairs of plates of copper and zinc four inches square, made active by means of solution of alum. After forty-eight hours the process was examined: Paper tinged with litmus plunged into the tube containing the transmitting or positive wire was immediately strongly reddened. Paper colored by turmeric introduced into the other tube had its color much deepened; the acid matter gave a very slight degree of turgidness to solution of nitrate of soda. The fluid that affected turmeric retained this property after being strongly boiled; and it appeared more vivid as the quantity became reduced by evaporation; carbonate of ammonia was mixed with it, and the whole dried and exposed to a strong heat; a minute quantity of white matter remained, which, as far as my examinations could go, had the properties of carbonate of soda. I compared it with similar minute portions of the pure carbonates of potash, and similar minute portions of the pure carbonates of potash and soda. It was not so deliquescent as the former of these bodies, and it formed a salt with nitric acid, which, like nitrate of soda, soon attracted moisture from a damp atmosphere and became fluid.

"This result was unexpected, but it was far from convincing me that the substances which were obtained were generated. In a similar process with glass tubes, carried on under exactly the same circumstances and for the same time, I obtained a quantity of alkali which must have been more than twenty times greater, but no traces of muriatic acid. There was much probability that the agate contained some minute portion of saline matter, not easily detected by chemical analysis, either in combination or intimate cohesion in its pores. To determine this, I repeated this a second, a third, and a fourth time. In the second experiment turbidness was still produced by a solution of nitrate of silver in the tube containing the acid, but it was less distinct; in the third process it was barely perceptible; and in the fourth process the two fluids remained perfectly clear after the mixture. The quantity of alkaline matter diminished in every operation; and in the last process, though the battery had been kept in great activity for three days, the fluid possessed, in a very slight degree, only the power of acting on paper tinged with turmeric; but its alkaline property was very sensible to litmus paper slightly reddened, which is a much more delicate test; and after evaporation and the process by carbonate of ammonia, a barely perceptible quantity of fixed alkali was still left. The acid matter in the other tube was abundant; its taste was sour; it smelled like water over which large quantities of nitrous gas have been long kept; it did not effect solution of muriate of barytes; and a drop of it placed upon a polished plate of silver left, after evaporation, a black stain, precisely similar to that produced by extremely diluted nitrous acid.

"After these results I could no longer doubt that some saline matter existing in the agate tubes had been the source of the acid matter capable of precipitating nitrate of silver and much of the alkali. Four additional repetitions of the process, however, convinced me that there was likewise some other cause for the presence of this last substance; for it continued to appear to the last in quantities sufficiently distinguishable, and apparently equal in every case. I had used every precaution, I had included the tube in glass vessels out of the reach of the circulating air; all the acting materials had been repeatedly washed with distilled water; and no part of them in contact with the fluid had been touched by the fingers.

"The only substance that I could now conceive as furnishing the fixed alkali was the water itself. This water appeared pure by the tests of nitrate of silver and muriate of barytes; but potash of soda, as is well known, rises in small quantities in rapid distillation; and the New River water which I made use of contains animal and vegetable impurities, which it was easy to conceive might furnish neutral salts capable of being carried over in vivid ebullition."[1] Further experiment proved the correctness of this inference, and the last doubt as to the origin of the puzzling chemical was dispelled.

Though the presence of the alkalies and acids in the water was explained, however, their respective migrations to the negative and positive poles of the battery remained to be accounted for. Davy's classical explanation assumed that different elements differ among themselves as to their electrical properties, some being positively, others negatively, electrified. Electricity and "chemical affinity," he said, apparently are manifestations of the same force, acting in the one case on masses, in the other on particles. Electro-positive particles unite with electro-negative particles to form chemical compounds, in virtue of the familiar principle that opposite electricities attract one another. When compounds are decomposed by the battery, this mutual attraction is overcome by the stronger attraction of the poles of the battery itself.

This theory of binary composition of all chemical compounds, through the union of electro-positive and electro-negative atoms or molecules, was extended by Berzelius, and made the basis of his famous system of theoretical chemistry. This theory held that all inorganic compounds, however complex their composition, are essentially composed of such binary combinations. For many years this view enjoyed almost undisputed sway. It received what seemed strong confirmation when Faraday showed the definite connection between the amount of electricity employed and the amount of decomposition produced in the so-called electrolyte. But its claims were really much too comprehensive, as subsequent discoveries proved.

ORGANIC CHEMISTRY AND THE IDEA OF THE MOLECULE

When Berzelius first promulgated his binary theory he was careful to restrict its unmodified application to the compounds of the inorganic world. At that time, and for a long time thereafter, it was supposed that substances of organic nature had some properties that kept them aloof from the domain of inorganic chemistry. It was little doubted that a so-called "vital force" operated here, replacing or modifying the action of ordinary "chemical affinity." It was, indeed, admitted that organic compounds are composed of familiar elements--chiefly carbon, oxygen, hydrogen, and nitrogen; but these elements were supposed to be united in ways that could not be imitated in the domain of the non-living. It was regarded almost as an axiom of chemistry that no organic compound whatever could be put together from its elements--synthesized--in the laboratory. To effect the synthesis of even the simplest organic compound, it was thought that the "vital force" must be in operation.

Therefore a veritable sensation was created in the chemical world when, in the year 1828, it was announced that the young German chemist, Friedrich Wohler, formerly pupil of Berzelius, and already known as a coming master, had actually synthesized the well-known organic product urea in his laboratory at Sacrow. The "exception which proves the rule" is something never heard of in the domain of logical science. Natural law knows no exceptions. So the synthesis of a single organic compound sufficed at a blow to break down the chemical barrier which the imagination of the fathers of the science had erected between animate and inanimate nature. Thenceforth the philosophical chemist would regard the plant and animal organisms as chemical laboratories in which conditions are peculiarly favorable for building up complex compounds of a few familiar elements, under the operation of universal chemical laws. The chimera "vital force" could no longer gain recognition in the domain of chemistry.

Now a wave of interest in organic chemistry swept over the chemical world, and soon the study of carbon compounds became as much the fashion as electrochemistry had been in the, preceding generation.

Foremost among the workers who rendered this epoch of organic chemistry memorable were Justus Liebig in Germany and Jean Baptiste Andre Dumas in France, and their respective pupils, Charles Frederic Gerhardt and Augustus Laurent. Wohler, too, must be named in the same breath, as also must Louis Pasteur, who, though somewhat younger than the others, came upon the scene in time to take chief part in the most important of the controversies that grew out of their labors.

Several years earlier than this the way had been paved for the study of organic substances by Gay-Lussac's discovery, made in 1815, that a certain compound of carbon and nitrogen, which he named cyanogen, has a peculiar degree of stability which enables it to retain its identity and enter into chemical relations after the manner of a simple body. A year later Ampere discovered that nitrogen and hydrogen, when combined in certain proportions to form what he called ammonium, have the same property. Berzelius had seized upon this discovery of the compound radical, as it was called, because it seemed to lend aid to his dualistic theory. He conceived the idea that all organic compounds are binary unions of various compound radicals with an atom of oxygen, announcing this theory in 1818. Ten years later, Liebig and Wohler undertook a joint investigation which resulted in proving that compound radicals are indeed very abundant among organic substances. Thus the theory of Berzelius seemed to be substantiated, and organic chemistry came to be defined as the chemistry of compound radicals.

But even in the day of its seeming triumph the dualistic theory was destined to receive a rude shock. This came about through the investigations of Dumas, who proved that in a certain organic substance an atom of hydrogen may be removed and an atom of chlorine substituted in its place without destroying the integrity of the original compound--much as a child might substitute one block for another in its play-house. Such a substitution would be quite consistent with the dualistic theory, were it not for the very essential fact that hydrogen is a powerfully electro-positive element, while chlorine is as strongly electro-negative. Hence the compound radical which united successively with these two elements must itself be at one time electro-positive, at another electro-negative--a seeming inconsistency which threw the entire Berzelian theory into disfavor.

In its place there was elaborated, chiefly through the efforts of Laurent and Gerhardt, a conception of the molecule as a unitary structure, built up through the aggregation of various atoms, in accordance with "elective affinities" whose nature is not yet understood A doctrine of "nuclei" and a doctrine of "types" of molecular structure were much exploited, and, like the doctrine of compound radicals, became useful as aids to memory and guides for the analyst, indicating some of the plans of molecular construction, though by no means penetrating the mysteries of chemical affinity. They are classifications rather than explanations of chemical unions. But at least they served an important purpose in giving definiteness to the idea of a molecular structure built of atoms as the basis of all substances. Now at last the word molecule came to have a distinct meaning, as distinct from "atom," in the minds of the generality of chemists, as it had had for Avogadro a third of a century before. Avogadro's hypothesis that there are equal numbers of these molecules in equal volumes of gases, under fixed conditions, was revived by Gerhardt, and a little later, under the championship of Cannizzaro, was exalted to the plane of a fixed law. Thenceforth the conception of the molecule was to be as dominant a thought in chemistry as the idea of the atom had become in a previous epoch.

CHEMICAL AFFINITY

Of course the atom itself was in no sense displaced, but Avogadro's law soon made it plain that the atom had often usurped territory that did not really belong to it. In many cases the chemists had supposed themselves dealing with atoms as units where the true unit was the molecule. In the case

of elementary gases, such as hydrogen and oxygen, for example, the law of equal numbers of molecules in equal spaces made it clear that the atoms do not exist isolated, as had been supposed. Since two volumes of hydrogen unite with one volume of oxygen to form two volumes of water vapor, the simplest mathematics show, in the light of Avogadro's law, not only that each molecule of water must contain two hydrogen atoms (a point previously in dispute), but that the original molecules of hydrogen and oxygen must have been composed in each case of two atoms---else how could one volume of oxygen supply an atom for every molecule of two volumes of water?

What, then, does this imply? Why, that the elementary atom has an avidity for other atoms, a longing for companionship, an "affinity"--call it what you will--which is bound to be satisfied if other atoms are in the neighborhood. Placed solely among atoms of its own kind, the oxygen atom seizes on a fellow oxygen atom, and in all their mad dancings these two mates cling together--possibly revolving about each other in miniature planetary orbits. Precisely the same thing occurs among the hydrogen atoms. But now suppose the various pairs of oxygen atoms come near other pairs of hydrogen atoms (under proper conditions which need not detain us here), then each oxygen atom loses its attachment for its fellow, and flings itself madly into the circuit of one of the hydrogen couplets, and--presto!--there are only two molecules for every three there were before, and free oxygen and hydrogen have become water. The whole process, stated in chemical phraseology, is summed up in the statement that under the given conditions the oxygen atoms had a greater affinity for the hydrogen atoms than for one another.

As chemists studied the actions of various kinds of atoms, in regard to their unions with one another to form molecules, it gradually dawned upon them that not all elements are satisfied with the same number of companions. Some elements ask only one, and refuse to take more; while others link themselves, when occasion offers, with two, three, four, or more. Thus we saw that oxygen forsook a single atom of its own kind and linked itself with two atoms of hydrogen. Clearly, then, the oxygen atom, like a creature with two hands, is able to clutch two other atoms. But we have no proof that under any circumstances it could hold more than two. Its affinities seem satisfied when it has two bonds. But, on the other hand, the atom of nitrogen is able to hold three atoms of hydrogen, and does so in the molecule of ammonium (NH3); while the carbon atom can hold four atoms of hydrogen or two atoms of oxygen.

Evidently, then, one atom is not always equivalent to another atom of a different kind in combining powers. A recognition of this fact by Frankland about 1852, and its further investigation by others (notably A. Kekule and A. S. Couper), led to the introduction of the word equivalent into chemical terminology in a new sense, and in particular to an understanding of the affinities or "valency" of different elements, which proved of the most fundamental importance. Thus it was shown that, of the four elements that enter most prominently into organic compounds, hydrogen can link itself with only a single bond to any other element--it has, so to speak, but a single hand with which to grasp--while oxygen has capacity for two bonds, nitrogen for three (possibly for five), and carbon for four. The words monovalent, divalent, trivalent, tretrava-lent, etc., were coined to express this most important fact, and the various elements came to be known as monads, diads, triads, etc. Just why different elements should differ thus in valency no one as yet knows; it is an empirical fact that they do. And once the nature of any element has been determined as regards its valency, a most important insight into the possible behavior of that element has been secured. Thus a consideration of the fact that hydrogen is monovalent, while oxygen is divalent, makes it plain that we must

expect to find no more than three compounds of these two elements--namely, H--O--(written HO by the chemist, and called hydroxyl); H--O--H (H_2O, or water), and H--O--O--H (H_2O_2, or hydrogen peroxide). It will be observed that in the first of these compounds the atom of oxygen stands, so to speak, with one of its hands free, eagerly reaching out, therefore, for another companion, and hence, in the language of chemistry, forming an unstable compound. Again, in the third compound, though all hands are clasped, yet one pair links oxygen with oxygen; and this also must be an unstable union, since the avidity of an atom for its own kind is relatively weak. Thus the well-known properties of hydrogen peroxide are explained, its easy decomposition, and the eagerness with which it seizes upon the elements of other compounds.

But the molecule of water, on the other hand, has its atoms arranged in a state of stable equilibrium, all their affinities being satisfied. Each hydrogen atom has satisfied its own affinity by clutching the oxygen atom; and the oxygen atom has both its bonds satisfied by clutching back at the two hydrogen atoms. Therefore the trio, linked in this close bond, have no tendency to reach out for any other companion, nor, indeed, any power to hold another should it thrust itself upon them. They form a "stable" compound, which under all ordinary circumstances will retain its identity as a molecule of water, even though the physical mass of which it is a part changes its condition from a solid to a gas from ice to vapor.

But a consideration of this condition of stable equilibrium in the molecule at once suggests a new question: How can an aggregation of atoms, having all their affinities satisfied, take any further part in chemical reactions? Seemingly such a molecule, whatever its physical properties, must be chemically inert, incapable of any atomic readjustments. And so in point of fact it is, so long as its component atoms cling to one another unremittingly. But this, it appears, is precisely what the atoms are little prone to do. It seems that they are fickle to the last degree in their individual attachments, and are as prone to break away from bondage as they are to enter into it. Thus the oxygen atom which has just flung itself into the circuit of two hydrogen atoms, the next moment flings itself free again and seeks new companions. It is for all the world like the incessant change of partners in a rollicking dance. This incessant dissolution and reformation of molecules in a substance which as a whole remains apparently unchanged was first fully appreciated by Ste.-Claire Deville, and by him named dissociation. It is a process which goes on much more actively in some compounds than in others, and very much more actively under some physical conditions (such as increase of temperature) than under others. But apparently no substances at ordinary temperatures, and no temperature above the absolute zero, are absolutely free from its disturbing influence. Hence it is that molecules having all the valency of their atoms fully satisfied do not lose their chemical activity--since each atom is momentarily free in the exchange of partners, and may seize upon different atoms from its former partners, if those it prefers are at hand.

While, however, an appreciation of this ceaseless activity of the atom is essential to a proper understanding of its chemical efficiency, yet from another point of view the "saturated" molecule--that is, the molecule whose atoms have their valency all satisfied--may be thought of as a relatively fixed or stable organism. Even though it may presently be torn down, it is for the time being a completed structure; and a consideration of the valency of its atoms gives the best clew that has hitherto been obtainable as to the character of its architecture. How important this matter of architecture of the molecule--of space relations of the atoms--may be was demonstrated as long ago as 1823, when Liebig and Wohler proved, to the utter bewilderment of the chemical world, that two

substances may have precisely the same chemical constitution--the same number and kind of atoms--and yet differ utterly in physical properties. The word isomerism was coined by Berzelius to express this anomalous condition of things, which seemed to negative the most fundamental truths of chemistry. Naming the condition by no means explained it, but the fact was made clear that something besides the mere number and kind of atoms is important in the architecture of a molecule. It became certain that atoms are not thrown together haphazard to build a molecule, any more than bricks are thrown together at random to form a house.

How delicate may be the gradations of architectural design in building a molecule was well illustrated about 1850, when Pasteur discovered that some carbon compounds--as certain sugars--can only be distinguished from one another, when in solution, by the fact of their twisting or polarizing a ray of light to the left or to the right, respectively. But no inkling of an explanation of these strange variations of molecular structure came until the discovery of the law of valency. Then much of the mystery was cleared away; for it was plain that since each atom in a molecule can hold to itself only a fixed number of other atoms, complex molecules must have their atoms linked in definite chains or groups. And it is equally plain that where the atoms are numerous, the exact plan of grouping may sometimes be susceptible of change without doing violence to the law of valency. It is in such cases that isomerism is observed to occur.

By paying constant heed to this matter of the affinities, chemists are able to make diagrammatic pictures of the plan of architecture of any molecule whose composition is known. In the simple molecule of water (H2O), for example, the two hydrogen atoms must have released each other before they could join the oxygen, and the manner of linking must apparently be that represented in the graphic formula H--O--H. With molecules composed of a large number of atoms, such graphic representation of the scheme of linking is of course increasingly difficult, yet, with the affinities for a guide, it is always possible. Of course no one supposes that such a formula, written in a single plane, can possibly represent the true architecture of the molecule: it is at best suggestive or diagrammatic rather than pictorial. Nevertheless, it affords hints as to the structure of the molecule such as the fathers of chemistry would not have thought it possible ever to attain.

PERIODICITY OF ATOMIC WEIGHTS

These utterly novel studies of molecular architecture may seem at first sight to take from the atom much of its former prestige as the all-important personage of the chemical world. Since so much depends upon the mere position of the atoms, it may appear that comparatively little depends upon the nature of the atoms themselves. But such a view is incorrect, for on closer consideration it will appear that at no time has the atom been seen to renounce its peculiar personality. Within certain limits the character of a molecule may be altered by changing the positions of its atoms (just as different buildings may be constructed of the same bricks), but these limits are sharply defined, and it would be as impossible to exceed them as it would be to build a stone building with bricks. From first to last the brick remains a brick, whatever the style of architecture it helps to construct; it never becomes a stone. And just as closely does each atom retain its own peculiar properties, regardless of its surroundings.

Thus, for example, the carbon atom may take part in the formation at one time of a diamond, again of a piece of coal, and yet again of a particle of sugar, of wood fibre, of animal tissue, or of a gas in the atmosphere; but from first to last--from glass-cutting gem to intangible gas--there is no demonstrable change whatever in any single property of the atom itself. So far as we know, its size, its weight, its capacity for vibration or rotation, and its inherent affinities, remain absolutely unchanged throughout all these varying fortunes of position and association. And the same thing is true of every atom of all of the seventy-odd elementary substances with which the modern chemist is acquainted. Every one appears always to maintain its unique integrity, gaining nothing and losing nothing.

All this being true, it would seem as if the position of the Daltonian atom as a primordial bit of matter, indestructible and non-transmutable, had been put to the test by the chemistry of our century, and not found wanting. Since those early days of the century when the electric battery performed its miracles and seemingly reached its limitations in the hands of Davy, many new elementary substances have been discovered, but no single element has been displaced from its position as an undecomposable body. Rather have the analyses of the chemist seemed to make it more and more certain that all elementary atoms are in truth what John Herschel called them, "manufactured articles"--primordial, changeless, indestructible.

And yet, oddly enough, it has chanced that hand in hand with the experiments leading to such a goal have gone other experiments arid speculations of exactly the opposite tenor. In each generation there have been chemists among the leaders of their science who have refused to admit that the so-called elements are really elements at all in any final sense, and who have sought eagerly for proof which might warrant their scepticism. The first bit of evidence tending to support this view was furnished by an English physician, Dr. William Prout, who in 1815 called attention to a curious relation to be observed between the atomic weight of the various elements. Accepting the figures given by the authorities of the time (notably Thomson and Berzelius), it appeared that a strikingly large proportion of the atomic weights were exact multiples of the weight of hydrogen, and that others differed so slightly that errors of observation might explain the discrepancy. Prout felt that it could not be accidental, and he could think of no tenable explanation, unless it be that the atoms of the various alleged elements are made up of different fixed numbers of hydrogen atoms. Could it be that the one true element--the one primal matter--is hydrogen, and that all other forms of matter are but compounds of this original substance?

Prout advanced this startling idea at first tentatively, in an anonymous publication; but afterwards he espoused it openly and urged its tenability. Coming just after Davy's dissociation of some supposed elements, the idea proved alluring, and for a time gained such popularity that chemists were disposed to round out the observed atomic weights of all elements into whole numbers. But presently renewed determinations of the atomic weights seemed to discountenance this practice, and Prout's alleged law fell into disrepute. It was revived, however, about 1840, by Dumas, whose great authority secured it a respectful hearing, and whose careful redetermination of the weight of carbon, making it exactly twelve times that of hydrogen, aided the cause.

Subsequently Stas, the pupil of Dumas, undertook a long series of determinations of atomic weights, with the expectation of confirming the Proutian hypothesis. But his results seemed to disprove the hypothesis, for the atomic weights of many elements differed from whole numbers by

more, it was thought, than the limits of error of the experiments. It was noteworthy, however, that the confidence of Dumas was not shaken, though he was led to modify the hypothesis, and, in accordance with previous suggestions of Clark and of Marignac, to recognize as the primordial element, not hydrogen itself, but an atom half the weight, or even one-fourth the weight, of that of hydrogen, of which primordial atom the hydrogen atom itself is compounded. But even in this modified form the hypothesis found great opposition from experimental observers.

In 1864, however, a novel relation between the weights of the elements and their other characteristics was called to the attention of chemists by Professor John A. R. Newlands, of London, who had noticed that if the elements are arranged serially in the numerical order of their atomic weights, there is a curious recurrence of similar properties at intervals of eight elements This so-called "law of octaves" attracted little immediate attention, but the facts it connotes soon came under the observation of other chemists, notably of Professors Gustav Hinrichs in America, Dmitri Mendeleeff in Russia, and Lothar Meyer in Germany. Mendeleeff gave the discovery fullest expression, explicating it in 1869, under the title of "the periodic law."

Though this early exposition of what has since been admitted to be a most important discovery was very fully outlined, the generality of chemists gave it little heed till a decade or so later, when three new elements, gallium, scandium, and germanium, were discovered, which, on being analyzed, were quite unexpectedly found to fit into three gaps which Mendeleeff had left in his periodic scale. In effect the periodic law had enabled Mendeleeff to predicate the existence of the new elements years before they were discovered. Surely a system that leads to such results is no mere vagary. So very soon the periodic law took its place as one of the most important generalizations of chemical science.

This law of periodicity was put forward as an expression of observed relations independent of hypothesis; but of course the theoretical bearings of these facts could not be overlooked. As Professor J. H. Gladstone has said, it forces upon us "the conviction that the elements are not separate bodies created without reference to one another, but that they have been originally fashioned, or have been built up, from one another, according to some general plan." It is but a short step from that proposition to the Proutian hypothesis.

NEW WEAPONS--SPECTROSCOPE AND CAMERA

But the atomic weights are not alone in suggesting the compound nature of the alleged elements. Evidence of a totally different kind has contributed to the same end, from a source that could hardly have been imagined when the Proutian hypothesis, was formulated, through the tradition of a novel weapon to the armamentarium of the chemist--the spectroscope. The perfection of this instrument, in the hands of two German scientists, Gustav Robert Kirchhoff and Robert Wilhelm Bunsen, came about through the investigation, towards the middle of the century, of the meaning of the dark lines which had been observed in the solar spectrum by Fraunhofer as early as 1815, and by Wollaston a decade earlier. It was suspected by Stokes and by Fox Talbot in England, but first brought to demonstration by Kirchhoff and Bunsen, that these lines, which were known to occupy definite positions in the spectrum, are really indicative of particular elementary substances. By means of the spectroscope, which is essentially a magnifying lens attached to a prism of glass, it is possible to

locate the lines with great accuracy, and it was soon shown that here was a new means of chemical analysis of the most exquisite delicacy. It was found, for example, that the spectroscope could detect the presence of a quantity of sodium so infinitesimal as the one two-hundred-thousandth of a grain. But what was even more important, the spectroscope put no limit upon the distance of location of the substance it tested, provided only that sufficient light came from it. The experiments it recorded might be performed in the sun, or in the most distant stars or nebulae; indeed, one of the earliest feats of the instrument was to wrench from the sun the secret of his chemical constitution.

To render the utility of the spectroscope complete, however, it was necessary to link with it another new chemical agency--namely, photography. This now familiar process is based on the property of light to decompose certain unstable compounds of silver, and thus alter their chemical composition. Davy and Wedgwood barely escaped the discovery of the value of the photographic method early in the nineteenth century. Their successors quite overlooked it until about 1826, when Louis J. M. Daguerre, the French chemist, took the matter in hand, and after many years of experimentation brought it to relative perfection in 1839, in which year the famous daguerreotype first brought the matter to popular attention. In the same year Mr. Fox Talbot read a paper on the subject before the Royal Society, and soon afterwards the efforts of Herschel and numerous other natural philosophers contributed to the advancement of the new method.

In 1843 Dr. John W. Draper, the famous English-American chemist and physiologist, showed that by photography the Fraunhofer lines in the solar spectrum might be mapped with absolute accuracy; also proving that the silvered film revealed many lines invisible to the unaided eye. The value of this method of observation was recognized at once, and, as soon as the spectroscope was perfected, the photographic method, in conjunction with its use, became invaluable to the chemist. By this means comparisons of spectra may be made with a degree of accuracy not otherwise obtainable; and, in case of the stars, whole clusters of spectra may be placed on record at a single observation.

As the examination of the sun and stars proceeded, chemists were amazed or delighted, according to their various preconceptions, to witness the proof that many familiar terrestrial elements are to be found in the celestial bodies. But what perhaps surprised them most was to observe the enormous preponderance in the sidereal bodies of the element hydrogen. Not only are there vast quantities of this element in the sun's atmosphere, but some other suns appeared to show hydrogen lines almost exclusively in their spectra. Presently it appeared that the stars of which this is true are those white stars, such as Sirius, which had been conjectured to be the hottest; whereas stars that are only red-hot, like our sun, show also the vapors of many other elements, including iron and other metals.

In 1878 Professor J. Norman Lockyer, in a paper before the Royal Society, called attention to the possible significance of this series of observations. He urged that the fact of the sun showing fewer elements than are observed here on the cool earth, while stars much hotter than the sun show chiefly one element, and that one hydrogen, the lightest of known elements, seemed to give color to the possibility that our alleged elements are really compounds, which at the temperature of the hottest stars may be decomposed into hydrogen, the latter "element" itself being also doubtless a compound, which might be resolved under yet more trying conditions.

Here, then, was what might be termed direct experimental evidence for the hypothesis of Prout. Unfortunately, however, it is evidence of a kind which only a few experts are competent to discuss--so very delicate a matter is the spectral analysis of the stars. What is still more unfortunate, the experts do not agree among themselves as to the validity of Professor Lockyer's conclusions. Some, like Professor Crookes, have accepted them with acclaim, hailing Lockyer as "the Darwin of the inorganic world," while others have sought a different explanation of the facts he brings forward. As yet it cannot be said that the controversy has been brought to final settlement. Still, it is hardly to be doubted that now, since the periodic law has seemed to join hands with the spectroscope, a belief in the compound nature of the so-called elements is rapidly gaining ground among chemists. More and more general becomes the belief that the Daltonian atom is really a compound radical, and that back of the seeming diversity of the alleged elements is a single form of primordial matter. Indeed, in very recent months, direct experimental evidence for this view has at last come to hand, through the study of radio-active substances. In a later chapter we shall have occasion to inquire how this came about.

IV. ANATOMY AND PHYSIOLOGY IN THE EIGHTEENTH CENTURY

ALBRECHT VON HALLER

An epoch in physiology was made in the eighteenth century by the genius and efforts of Albrecht von Haller (1708-1777), of Berne, who is perhaps as worthy of the title "The Great" as any philosopher who has been so christened by his contemporaries since the time of Hippocrates. Celebrated as a physician, he was proficient in various fields, being equally famed in his own time as poet, botanist, and statesman, and dividing his attention between art and science.

As a child Haller was so sickly that he was unable to amuse himself with the sports and games common to boys of his age, and so passed most of his time poring over books. When ten years of age he began writing poems in Latin and German, and at fifteen entered the University of Tubingen. At seventeen he wrote learned articles in opposition to certain accepted doctrines, and at nineteen he received his degree of doctor. Soon after this he visited England, where his zeal in dissecting brought him under suspicion of grave-robbery, which suspicion made it expedient for him to return to the Continent. After studying botany in Basel for some time he made an extended botanical journey through Switzerland, finally settling in his native city, Berne, as a practising physician. During this time he did not neglect either poetry or botany, publishing anonymously a collection of poems.

In 1736 he was called to Gottingen as professor of anatomy, surgery, chemistry, and botany. During his labors in the university he never neglected his literary work, sometimes living and sleeping for days and nights together in his library, eating his meals while delving in his books, and sleeping only when actually compelled to do so by fatigue. During all this time he was in correspondence with savants from all over the world, and it is said of him that he never left a letter of any kind unanswered.

Haller's greatest contribution to medical science was his famous doctrine of irritability, which has given him the name of "father of modern nervous physiology," just as Harvey is called "the father of the modern physiology of the blood." It has been said of this famous doctrine of irritability that "it moved all the minds of the century--and not in the departments of medicine alone--in a way of which we of the present day have no satisfactory conception, unless we compare it with our modern Darwinism."[1]

The principle of general irritability had been laid down by Francis Glisson (1597-1677) from deductive studies, but Haller proved by experiments along the line of inductive methods that this irritability was not common to all "fibre as well as to the fluids of the body," but something entirely special, and peculiar only to muscular substance. He distinguished between irritability of muscles and sensibility of nerves. In 1747 he gave as the three forces that produce muscular movements: elasticity, or "dead nervous force"; irritability, or "innate nervous force"; and nervous force in itself. And in 1752 he described one hundred and ninety experiments for determining what parts of the body possess "irritability"--that is, the property of contracting when stimulated. His conclusion that this irritability exists in muscular substance alone and is quite independent of the nerves proceeding to it aroused a controversy that was never definitely settled until late in the nineteenth century, when Haller's theory was found to be entirely correct.

It was in pursuit of experiments to establish his theory of irritability that Haller made his chief discoveries in embryology and development. He proved that in the process of incubation of the egg the first trace of the heart of the chick shows itself in the thirty-eighth hour, and that the first trace of red blood showed in the forty-first hour. By his investigations upon the lower animals he attempted to confirm the theory that since the creation of genus every individual is derived from a preceding individual--the existing theory of preformation, in which he believed, and which taught that "every individual is fully and completely preformed in the germ, simply growing from microscopic to visible proportions, without developing any new parts."

In physiology, besides his studies of the nervous system, Haller studied the mechanism of respiration, refuting the teachings of Hamberger (1697-1755), who maintained that the lungs contract independently. Haller, however, in common with his contemporaries, failed utterly to understand the true function of the lungs. The great physiologist's influence upon practical medicine, while most profound, was largely indirect. He was a theoretical rather than a practical physician, yet he is credited with being the first physician to use the watch in counting the pulse.

BATTISTA MORGAGNI AND MORBID ANATOMY

A great contemporary of Haller was Giovanni Battista Morgagni (1682-1771), who pursued what Sydenham had neglected, the investigation in anatomy, thus supplying a necessary counterpart to the great Englishman's work. Morgagni's investigations were directed chiefly to the study of morbid anatomy--the study of the structure of diseased tissue, both during life and post mortem, in contrast to the normal anatomical structures. This work cannot be said to have originated with him; for as early as 1679 Bonnet had made similar, although less extensive, studies; and later many investigators, such as Lancisi and Haller, had made post-mortem studies. But Morgagni's De

sedibus et causis morborum per anatomen indagatis was the largest, most accurate, and best-illustrated collection of cases that had ever been brought together, and marks an epoch in medical science. From the time of the publication of Morgagni's researches, morbid anatomy became a recognized branch of the medical science, and the effect of the impetus thus given it has been steadily increasing since that time.

WILLIAM HUNTER

William Hunter (1718-1783) must always be remembered as one of the greatest physicians and anatomists of the eighteenth century, and particularly as the first great teacher of anatomy in England; but his fame has been somewhat overshadowed by that of his younger brother John.

Hunter had been intended and educated for the Church, but on the advice of the surgeon William Cullen he turned his attention to the study of medicine. His first attempt at teaching was in 1746, when he delivered a series of lectures on surgery for the Society of Naval Practitioners. These lectures proved so interesting and instructive that he was at once invited to give others, and his reputation as a lecturer was soon established. He was a natural orator and story-teller, and he combined with these attractive qualities that of thoroughness and clearness in demonstrations, and although his lectures were two hours long he made them so full of interest that his pupils seldom tired of listening. He believed that he could do greater good to the world by "publicly teaching his art than by practising it," and even during the last few days of his life, when he was so weak that his friends remonstrated against it, he continued his teaching, fainting from exhaustion at the end of his last lecture, which preceded his death by only a few days.

For many years it was Hunter's ambition to establish a museum where the study of anatomy, surgery, and medicine might be advanced, and in 1765 he asked for a grant of a plot of ground for this purpose, offering to spend seven thousand pounds on its, erection besides endowing it with a professorship of anatomy. Not being able to obtain this grant, however, he built a house, in which were lecture and dissecting rooms, and his museum. In this museum were anatomical preparations, coins, minerals, and natural-history specimens.

Hunter's weakness was his love of controversy and his resentment of contradiction. This brought him into strained relations with many of the leading physicians of his time, notably his own brother John, who himself was probably not entirely free from blame in the matter. Hunter is said to have excused his own irritability on the grounds that being an anatomist, and accustomed to "the passive submission of dead bodies," contradictions became the more unbearable. Many of the physiological researches begun by him were carried on and perfected by his more famous brother, particularly his investigations of the capillaries, but he added much to the anatomical knowledge of several structures of the body, notably as to the structure of cartilages and joints.

JOHN HUNTER

In Abbot Islip's chapel in Westminster Abbey, close to the resting-place of Ben Jonson, rest the remains of John Hunter (1728-1793), famous in the annals of medicine as among the greatest

physiologists and surgeons that the world has ever produced: a man whose discoveries and inventions are counted by scores, and whose field of research was only limited by the outermost boundaries of eighteenth-century science, although his efforts were directed chiefly along the lines of his profession.

Until about twenty years of age young Hunter had shown little aptitude for study, being unusually fond of out-door sports and amusements; but about that time, realizing that some occupation must be selected, he asked permission of his brother William to attempt some dissections in his anatomical school in London. To the surprise of his brother he made this dissection unusually well; and being given a second, he acquitted himself with such skill that his brother at once predicted that he would become a great anatomist. Up to this time he had had no training of any kind to prepare him for his professional career, and knew little of Greek or Latin--languages entirely unnecessary for him, as he proved in all of his life work. Ottley tells the story that, when twitted with this lack of knowledge of the "dead languages" in after life, he said of his opponent, "I could teach him that on the dead body which he never knew in any language, dead or living."

By his second year in dissection he had become so skilful that he was given charge of some of the classes in his brother's school; in 1754 he became a surgeon's pupil in St. George's Hospital, and two years later house-surgeon. Having by overwork brought on symptoms that seemed to threaten consumption, he accepted the position of staff-surgeon to an expedition to Belleisle in 1760, and two years later was serving with the English army at Portugal. During all this time he was constantly engaged in scientific researches, many of which, such as his observations of gun-shot wounds, he put to excellent use in later life. On returning to England much improved in health in 1763, he entered at once upon his career as a London surgeon, and from that time forward his progress was a practically uninterrupted series of successes in his profession.

Hunter's work on the study of the lymphatics was of great service to the medical profession. This important net-work of minute vessels distributed throughout the body had recently been made the object of much study, and various students, including Haller, had made extensive investigations since their discovery by Asellius. But Hunter, in 1758, was the first to discover the lymphatics in the neck of birds, although it was his brother William who advanced the theory that the function of these vessels was that of absorbents. One of John Hunter's pupils, William Hewson (1739-1774), first gave an account, in 1768, of the lymphatics in reptiles and fishes, and added to his teacher's investigations of the lymphatics in birds. These studies of the lymphatics have been regarded, perhaps with justice, as Hunter's most valuable contributions to practical medicine.

In 1767 he met with an accident by which he suffered a rupture of the tendo Achillis--the large tendon that forms the attachment of the muscles of the calf to the heel. From observations of this accident, and subsequent experiments upon dogs, he laid the foundation for the now simple and effective operation for the cure of club feet and other deformities involving the tendons. In 1772 he moved into his residence at Earlscourt, Brompton, where he gathered about him a great menagerie of animals, birds, reptiles, insects, and fishes, which he used in his physiological and surgical experiments. Here he performed a countless number of experiments--more, probably, than "any man engaged in professional practice has ever conducted." These experiments varied in nature from observations of the habits of bees and wasps to major surgical operations performed upon

hedgehogs, dogs, leopards, etc. It is said that for fifteen years he kept a flock of geese for the sole purpose of studying the process of development in eggs.

Hunter began his first course of lectures in 1772, being forced to do this because he had been so repeatedly misquoted, and because he felt that he could better gauge his own knowledge in this way. Lecturing was a sore trial to him, as he was extremely diffident, and without writing out his lectures in advance he was scarcely able to speak at all. In this he presented a marked contrast to his brother William, who was a fluent and brilliant speaker. Hunter's lectures were at best simple readings of the facts as he had written them, the diffident teacher seldom raising his eyes from his manuscript and rarely stopping until his complete lecture had been read through. His lectures were, therefore, instructive rather than interesting, as he used infinite care in preparing them; but appearing before his classes was so dreaded by him that he is said to have been in the habit of taking a half-drachm of laudanum before each lecture to nerve him for the ordeal. One is led to wonder by what name he shall designate that quality of mind that renders a bold and fearless surgeon like Hunter, who is undaunted in the face of hazardous and dangerous operations, a stumbling, halting, and "frightened" speaker before a little band of, at most, thirty young medical students. And yet this same thing is not unfrequently seen among the boldest surgeons.

Hunter's Operation for the Cure of Aneurisms

It should be an object-lesson to those who, ignorantly or otherwise, preach against the painless vivisection as practised to-day, that by the sacrifice of a single deer in the cause of science Hunter discovered a fact in physiology that has been the means of saving thousands of human lives and thousands of human bodies from needless mutilation. We refer to the discovery of the "collateral circulation" of the blood, which led, among other things, to Hunter's successful operation upon aneurisms.

Simply stated, every organ or muscle of the body is supplied by one large artery, whose main trunk distributes the blood into its lesser branches, and thence through the capillaries. Cutting off this main artery, it would seem, should cut off entirely the blood-supply to the particular organ which is supplied by this vessel; and until the time of Hunter's demonstration this belief was held by most physiologists. But nature has made a provision for this possible stoppage of blood-supply from a single source, and has so arranged that some of the small arterial branches coming from the main supply-trunk are connected with other arterial branches coming from some other supply-trunk. Under normal conditions the main arterial trunks supply their respective organs, the little connecting arterioles playing an insignificant part. But let the main supply-trunk be cut off or stopped for whatever reason, and a remarkable thing takes place. The little connecting branches begin at once to enlarge and draw blood from the neighboring uninjured supply-trunk, This enlargement continues until at last a new route for the circulation has been established, the organ no longer depending on the now defunct original arterial trunk, but getting on as well as before by this "collateral" circulation that has been established.

The thorough understanding of this collateral circulation is one of the most important steps in surgery, for until it was discovered amputations were thought necessary in such cases as those involving the artery supplying a leg or arm, since it was supposed that, the artery being stopped, death of the limb and the subsequent necessity for amputation were sure to follow. Hunter solved

this problem by a single operation upon a deer, and his practicality as a surgeon led him soon after to apply this knowledge to a certain class of surgical cases in a most revolutionary and satisfactory manner.

What led to Hunter's far-reaching discovery was his investigation as to the cause of the growth of the antlers of the deer. Wishing to ascertain just what part the blood-supply on the opposite sides of the neck played in the process of development, or, perhaps more correctly, to see what effect cutting off the main blood-supply would have, Hunter had one of the deer of Richmond Park caught and tied, while he placed a ligature around one of the carotid arteries--one of the two principal arteries that supply the head with blood. He observed that shortly after this the antler (which was only half grown and consequently very vascular) on the side of the obliterated artery became cold to the touch--from the lack of warmth-giving blood. There was nothing unexpected in this, and Hunter thought nothing of it until a few days later, when he found, to his surprise, that the antler had become as warm as its fellow, and was apparently increasing in size. Puzzled as to how this could be, and suspecting that in some way his ligature around the artery had not been effective, he ordered the deer killed, and on examination was astonished to find that while his ligature had completely shut off the blood-supply from the source of that carotid artery, the smaller arteries had become enlarged so as to supply the antler with blood as well as ever, only by a different route.

Hunter soon had a chance to make a practical application of the knowledge thus acquired. This was a case of popliteal aneurism, operations for which had heretofore proved pretty uniformly fatal. An aneurism, as is generally understood, is an enlargement of a certain part of an artery, this enlargement sometimes becoming of enormous size, full of palpitating blood, and likely to rupture with fatal results at any time. If by any means the blood can be allowed to remain quiet for even a few hours in this aneurism it will form a clot, contract, and finally be absorbed and disappear without any evil results. The problem of keeping the blood quiet, with the heart continually driving it through the vessel, is not a simple one, and in Hunter's time was considered so insurmountable that some surgeons advocated amputation of any member having an aneurism, while others cut down upon the tumor itself and attempted to tie off the artery above and below. The first of these operations maimed the patient for life, while the second was likely to prove fatal.

In pondering over what he had learned about collateral circulation and the time required for it to become fully established, Hunter conceived the idea that if the blood-supply was cut off from above the aneurism, thus temporarily preventing the ceaseless pulsations from the heart, this blood would coagulate and form a clot before the collateral circulation could become established or could affect it. The patient upon whom he performed his now celebrated operation was afflicted with a popliteal aneurism--that is, the aneurism was located on the large popliteal artery just behind the knee-joint. Hunter, therefore, tied off the femoral, or main supplying artery in the thigh, a little distance above the aneurism. The operation was entirely successful, and in six weeks' time the patient was able to leave the hospital, and with two sound limbs. Naturally the simplicity and success of this operation aroused the attention of Europe, and, alone, would have made the name of Hunter immortal in the annals of surgery. The operation has ever since been called the "Hunterian" operation for aneurism, but there is reason to believe that Dominique Anel (born about 1679) performed a somewhat similar operation several years earlier. It is probable, however, that Hunter had never heard of this work of Anel, and that his operation was the outcome of his own independent reasoning from the facts he had learned about collateral circulation. Furthermore,

Hunter's mode of operation was a much better one than Anel's, and, while Anel's must claim priority, the credit of making it widely known will always be Hunter's.

The great services of Hunter were recognized both at home and abroad, and honors and positions of honor and responsibility were given him. In 1776 he was appointed surgeon-extraordinary to the king; in 1783 he was elected a member of the Royal Society of Medicine and of the Royal Academy of Surgery at Paris; in 1786 he became deputy surgeon-general of the army; and in 1790 he was appointed surgeon-general and inspector-general of hospitals. All these positions he filled with credit, and he was actively engaged in his tireless pursuit of knowledge and in discharging his many duties when in October, 1793, he was stricken while addressing some colleagues, and fell dead in the arms of a fellow-physician.

LAZZARO SPALLANZANI

Hunter's great rival among contemporary physiologists was the Italian Lazzaro Spallanzani (1729-1799), one of the most picturesque figures in the history of science. He was not educated either as a scientist or physician, devoting, himself at first to philosophy and the languages, afterwards studying law, and later taking orders. But he was a keen observer of nature and of a questioning and investigating mind, so that he is remembered now chiefly for his discoveries and investigations in the biological sciences. One important demonstration was his controversion of the theory of abiogenesis, or "spontaneous generation," as propounded by Needham and Buffon. At the time of Needham's experiments it had long been observed that when animal or vegetable matter had lain in water for a little time--long enough for it to begin to undergo decomposition--the water became filled with microscopic creatures, the "infusoria animalculis." This would tend to show, either that the water or the animal or vegetable substance contained the "germs" of these minute organisms, or else that they were generated spontaneously. It was known that boiling killed these animalcules, and Needham agreed, therefore, that if he first heated the meat or vegetables, and also the water containing them, and then placed them in hermetically scaled jars--if he did this, and still the animalcules made their appearance, it would be proof-positive that they had been generated spontaneously. Accordingly be made numerous experiments, always with the same results--that after a few days the water was found to swarm with the microscopic creatures. The thing seemed proven beyond question--providing, of course, that there had been no slips in the experiments.

But Abbe Spallanzani thought that he detected such slips in Needham's experiment. The possibility of such slips might come in several ways: the contents of the jar might not have been boiled for a sufficient length of time to kill all the germs, or the air might not have been excluded completely by the sealing process. To cover both these contingencies, Spallanzani first hermetically sealed the glass vessels and then boiled them for three-quarters of an hour. Under these circumstances no animalcules ever made their appearance--a conclusive demonstration that rendered Needham's grounds for his theory at once untenable.[2]

Allied to these studies of spontaneous generation were Spallanzani's experiments and observations on the physiological processes of generation among higher animals. He experimented with frogs, tortoises, and dogs; and settled beyond question the function of the ovum and spermatozoon. Unfortunately he misinterpreted the part played by the spermatozoa in believing that their

surrounding fluid was equally active in the fertilizing process, and it was not until some forty years later (1824) that Dumas corrected this error.

THE CHEMICAL THEORY OF DIGESTION

Among the most interesting researches of Spallanzani were his experiments to prove that digestion, as carried on in the stomach, is a chemical process. In this he demonstrated, as Rene Reaumur had attempted to demonstrate, that digestion could be carried on outside the walls of the stomach as an ordinary chemical reaction, using the gastric juice as the reagent for performing the experiment. The question as to whether the stomach acted as a grinding or triturating organ, rather than as a receptacle for chemical action, had been settled by Reaumur and was no longer a question of general dispute. Reaumur had demonstrated conclusively that digestion would take place in the stomach in the same manner and the same time if the substance to be digested was protected from the peristalic movements of the stomach and subjected to the action of the gastric juice only. He did this by introducing the substances to be digested into the stomach in tubes, and thus protected so that while the juices of the stomach could act upon them freely they would not be affected by any movements of the organ.

Following up these experiments, he attempted to show that digestion could take place outside the body as well as in it, as it certainly should if it were a purely chemical process. He collected quantities of gastric juice, and placing it in suitable vessels containing crushed grain or flesh, kept the mixture at about the temperature of the body for several hours. After repeated experiments of this kind, apparently conducted with great care, Reaumur reached the conclusion that "the gastric juice has no more effect out of the living body in dissolving or digesting the food than water, mucilage, milk, or any other bland fluid."[3] Just why all of these experiments failed to demonstrate a fact so simple does not appear; but to Spallanzani, at least, they were by no means conclusive, and he proceeded to elaborate upon the experiments of Reaumur. He made his experiments in scaled tubes exposed to a certain degree of heat, and showed conclusively that the chemical process does go on, even when the food and gastric juice are removed from their natural environment in the stomach. In this he was opposed by many physiologists, among them John Hunter, but the truth of his demonstrations could not be shaken, and in later years we find Hunter himself completing Spallanzani's experiments by his studies of the post-mortem action of the gastric juice upon the stomach walls.

That Spallanzani's and Hunter's theories of the action of the gastric juice were not at once universally accepted is shown by an essay written by a learned physician in 1834. In speaking of some of Spallanzani's demonstrations, he writes: "In some of the experiments, in order to give the flesh or grains steeped in the gastric juice the same temperature with the body, the phials were introduced under the armpits. But this is not a fair mode of ascertaining the effects of the gastric juice out of the body; for the influence which life may be supposed to have on the solution of the food would be secured in this case. The affinities connected with life would extend to substances in contact with any part of the system: substances placed under the armpits are not placed at least in the same circumstances with those unconnected with a living animal." But just how this writer reaches the conclusion that "the experiments of Reaumur and Spallanzani give no evidence that the

gastric juice has any peculiar influence more than water or any other bland fluid in digesting the food"[4] is difficult to understand.

The concluding touches were given to the new theory of digestion by John Hunter, who, as we have seen, at first opposed Spallanzani, but who finally became an ardent champion of the chemical theory. Hunter now carried Spallanzani's experiments further and proved the action of the digestive fluids after death. For many years anatomists had been puzzled by pathological lesion of the stomach, found post mortem, when no symptoms of any disorder of the stomach had been evinced during life. Hunter rightly conceived that these lesions were caused by the action of the gastric juice, which, while unable to act upon the living tissue, continued its action chemically after death, thus digesting the walls of the stomach in which it had been formed. And, as usual with his observations, be turned this discovery to practical use in accounting for certain phenomena of digestion. The following account of the stomach being digested after death was written by Hunter at the desire of Sir John Pringle, when he was president of the Royal Society, and the circumstance which led to this is as follows: "I was opening, in his presence, the body of a patient of his own, where the stomach was in part dissolved, which appeared to him very unaccountable, as there had been no previous symptom that could have led him to suspect any disease in the stomach. I took that opportunity of giving him my ideas respecting it, and told him that I had long been making experiments on digestion, and considered this as one of the facts which proved a converting power in the gastric juice. . . . There are a great many powers in nature which the living principle does not enable the animal matter, with which it is combined, to resist--viz., the mechanical and most of the strongest chemical solvents. It renders it, however, capable of resisting the powers of fermentation, digestion, and perhaps several others, which are well known to act on the same matter when deprived of the living principle and entirely to decompose it. "

Hunter concludes his paper with the following paragraph: "These appearances throw considerable light on the principle of digestion, and show that it is neither a mechanical power, nor contractions of the stomach, nor heat, but something secreted in the coats of the stomach, and thrown into its cavity, which there animalizes the food or assimilates it to the nature of the blood. The power of this juice is confined or limited to certain substances, especially of the vegetable and animal kingdoms; and although this menstruum is capable of acting independently of the stomach, yet it is indebted to that viscus for its continuance.[5]

THE FUNCTION OF RESPIRATION

It is a curious commentary on the crude notions of mechanics of previous generations that it should have been necessary to prove by experiment that the thin, almost membranous stomach of a mammal has not the power to pulverize, by mere attrition, the foods that are taken into it. However, the proof was now for the first time forthcoming, and the question of the general character of the function of digestion was forever set at rest. Almost simultaneously with this great advance, corresponding progress was made in an allied field: the mysteries of respiration were at last cleared up, thanks to the new knowledge of chemistry. The solution of the problem followed almost as a matter of course upon the advances of that science in the latter part of the century. Hitherto no one since Mayow, of the previous century, whose flash of insight had been strangely overlooked and forgotten, had even vaguely surmised the true function of the lungs. The great

Boerhaave had supposed that respiration is chiefly important as an aid to the circulation of the blood; his great pupil, Haller, had believed to the day of his death in 1777 that the main purpose of the function is to form the voice. No genius could hope to fathom the mystery of the lungs so long as air was supposed to be a simple element, serving a mere mechanical purpose in the economy of the earth.

But the discovery of oxygen gave the clew, and very soon all the chemists were testing the air that came from the lungs--Dr. Priestley, as usual, being in the van. His initial experiments were made in 1777, and from the outset the problem was as good as solved. Other experimenters confirmed his results in all their essentials--notably Scheele and Lavoisier and Spallanzani and Davy. It was clearly established that there is chemical action in the contact of the air with the tissue of the lungs; that some of the oxygen of the air disappears, and that carbonic-acid gas is added to the inspired air. It was shown, too, that the blood, having come in contact with the air, is changed from black to red in color. These essentials were not in dispute from the first. But as to just what chemical changes caused these results was the subject of controversy. Whether, for example, oxygen is actually absorbed into the blood, or whether it merely unites with carbon given off from the blood, was long in dispute.

Each of the main disputants was biased by his own particular views as to the moot points of chemistry. Lavoisier, for example, believed oxygen gas to be composed of a metal oxygen combined with the alleged element heat; Dr. Priestley thought it a compound of positive electricity and phlogiston; and Humphry Davy, when he entered the lists a little later, supposed it to be a compound of oxygen and light. Such mistaken notions naturally complicated matters and delayed a complete understanding of the chemical processes of respiration. It was some time, too, before the idea gained acceptance that the most important chemical changes do not occur in the lungs themselves, but in the ultimate tissues. Indeed, the matter was not clearly settled at the close of the century. Nevertheless, the problem of respiration had been solved in its essentials. Moreover, the vastly important fact had been established that a process essentially identical with respiration is necessary to the existence not only of all creatures supplied with lungs, but to fishes, insects, and even vegetables--in short, to every kind of living organism.

ERASMUS DARWIN AND VEGETABLE PHYSIOLOGY

Some interesting experiments regarding vegetable respiration were made just at the close of the century by Erasmus Darwin, and recorded in his Botanic Garden as a foot-note to the verse:

"While spread in air the leaves respiring play."

These notes are worth quoting at some length, as they give a clear idea of the physiological doctrines of the time (1799), while taking advance ground as to the specific matter in question:

"There have been various opinions," Darwin says, "concerning the use of the leaves of plants in the vegetable economy. Some have contended that they are perspiratory organs. This does not seem probable from an experiment of Dr. Hales, Vegetable Statics, p. 30. He, found, by cutting off branches of trees with apples on them and taking off the leaves, that an apple exhaled about as

much as two leaves the surfaces of which were nearly equal to the apple; whence it would appear that apples have as good a claim to be termed perspiratory organs as leaves. Others have believed them excretory organs of excrementitious juices, but as the vapor exhaled from vegetables has no taste, this idea is no more probable than the other; add to this that in most weathers they do not appear to perspire or exhale at all.

"The internal surface of the lungs or air-vessels in men is said to be equal to the external surface of the whole body, or almost fifteen square feet; on this surface the blood is exposed to the influence of the respired air through the medium, however, of a thin pellicle; by this exposure to the air it has its color changed from deep red to bright scarlet, and acquires something so necessary to the existence of life that we can live scarcely a minute without this wonderful process.

"The analogy between the leaves of plants and the lungs or gills of animals seems to embrace so many circumstances that we can scarcely withhold our consent to their performing similar offices.

"1. The great surface of leaves compared to that of the trunk and branches of trees is such that it would seem to be an organ well adapted for the purpose of exposing the vegetable juices to the influence of the air; this, however, we shall see afterwards is probably performed only by their upper surfaces, yet even in this case the surface of the leaves in general bear a greater proportion to the surface of the tree than the lungs of animals to their external surfaces.

"2. In the lung of animals the blood, after having been exposed to the air in the extremities of the pulmonary artery, is changed in color from deep red to bright scarlet, and certainly in some of its essential properties it is then collected by the pulmonary vein and returned to the heart. To show a similarity of circumstances in the leaves of plants, the following experiment was made, June 24, 1781. A stalk with leaves and seed-vessels of large spurge (Euphorbia helioscopia) had been several days placed in a decoction of madder (Rubia tinctorum) so that the lower part of the stem and two of the undermost leaves were immersed in it. After having washed the immersed leaves in clear water I could readily discover the color of the madder passing along the middle rib of each leaf. The red artery was beautifully visible on the under and on the upper surface of the leaf; but on the upper side many red branches were seen going from it to the extremities of the leaf, which on the other side were not visible except by looking through it against the light. On this under side a system of branching vessels carrying a pale milky fluid were seen coming from the extremities of the leaf, and covering the whole under side of it, and joining two large veins, one on each side of the red artery in the middle rib of the leaf, and along with it descending to the foot-stalk or petiole. On slitting one of these leaves with scissors, and having a magnifying-glass ready, the milky blood was seen oozing out of the returning veins on each side of the red artery in the middle rib, but none of the red fluid from the artery.

"All these appearances were more easily seen in a leaf of Picris treated in the same manner; for in this milky plant the stems and middle rib of the leaves are sometimes naturally colored reddish, and hence the color of the madder seemed to pass farther into the ramifications of their leaf-arteries, and was there beautifully visible with the returning branches of milky veins on each side."

Darwin now goes on to draw an incorrect inference from his observations:

"3. From these experiments," he says, "the upper surface of the leaf appeared to be the immediate organ of respiration, because the colored fluid was carried to the extremities of the leaf by vessels most conspicuous on the upper surface, and there changed into a milky fluid, which is the blood of the plant, and then returned by concomitant veins on the under surface, which were seen to ooze when divided with scissors, and which, in Picris, particularly, render the under surface of the leaves greatly whiter than the upper one."

But in point of fact, as studies of a later generation were to show, it is the under surface of the leaf that is most abundantly provided with stomata, or "breathing-pores." From the stand-point of this later knowledge, it is of interest to follow our author a little farther, to illustrate yet more fully the possibility of combining correct observations with a faulty inference.

"4. As the upper surface of leaves constitutes the organ of respiration, on which the sap is exposed in the termination of arteries beneath a thin pellicle to the action of the atmosphere, these surfaces in many plants strongly repel moisture, as cabbage leaves, whence the particles of rain lying over their surfaces without touching them, as observed by Mr. Melville (Essays Literary and Philosophical: Edinburgh), have the appearance of globules of quicksilver. And hence leaves with the upper surfaces on water wither as soon as in the dry air, but continue green for many days if placed with the under surface on water, as appears in the experiments of Monsieur Bonnet (Usage des Feuilles). Hence some aquatic plants, as the water-lily (Nymphoea), have the lower sides floating on the water, while the upper surfaces remain dry in the air.

"5. As those insects which have many spiracula, or breathing apertures, as wasps and flies, are immediately suffocated by pouring oil upon them, I carefully covered with oil the surfaces of several leaves of phlomis, of Portugal laurel, and balsams, and though it would not regularly adhere, I found them all die in a day or two.

"It must be added that many leaves are furnished with muscles about their foot-stalks, to turn their surfaces to the air or light, as mimosa or Hedysarum gyrans. From all these analogies I think there can be no doubt but that leaves of trees are their lungs, giving out a phlogistic material to the atmosphere, and absorbing oxygen, or vital air.

"6. The great use of light to vegetation would appear from this theory to be by disengaging vital air from the water which they perspire, and thence to facilitate its union with their blood exposed beneath the thin surface of their leaves; since when pure air is thus applied it is probable that it can be more readily absorbed. Hence, in the curious experiments of Dr. Priestley and Mr. Ingenhouz, some plants purified less air than others--that is, they perspired less in the sunshine; and Mr. Scheele found that by putting peas into water which about half covered them they converted the vital air into fixed air, or carbonic-acid gas, in the same manner as in animal respiration.

"7. The circulation in the lungs or leaves of plants is very similar to that of fish. In fish the blood, after having passed through their gills, does not return to the heart as from the lungs of air-breathing animals, but the pulmonary vein taking the structure of an artery after having received the blood from the gills, which there gains a more florid color, distributes it to the other parts of their bodies. The same structure occurs in the livers of fish, whence we see in those animals two circulations independent of the power of the heart--viz., that beginning at the termination of the

veins of the gills and branching through the muscles, and that which passes through the liver; both which are carried on by the action of those respective arteries and veins."[6]

Darwin is here a trifle fanciful in forcing the analogy between plants and animals. The circulatory system of plants is really not quite so elaborately comparable to that of fishes as he supposed. But the all-important idea of the uniformity underlying the seeming diversity of Nature is here exemplified, as elsewhere in the writings of Erasmus Darwin; and, more specifically, a clear grasp of the essentials of the function of respiration is fully demonstrated.

ZOOLOGY AT THE CLOSE OF THE EIGHTEENTH CENTURY

Several causes conspired to make exploration all the fashion during the closing epoch of the eighteenth century. New aid to the navigator had been furnished by the perfected compass and quadrant, and by the invention of the chronometer; medical science had banished scurvy, which hitherto had been a perpetual menace to the voyager; and, above all, the restless spirit of the age impelled the venturesome to seek novelty in fields altogether new. Some started for the pole, others tried for a northeast or northwest passage to India, yet others sought the great fictitious antarctic continent told of by tradition. All these of course failed of their immediate purpose, but they added much to the world's store of knowledge and its fund of travellers' tales.

Among all these tales none was more remarkable than those which told of strange living creatures found in antipodal lands. And here, as did not happen in every field, the narratives were often substantiated by the exhibition of specimens that admitted no question. Many a company of explorers returned more or less laden with such trophies from the animal and vegetable kingdoms, to the mingled astonishment, delight, and bewilderment of the closet naturalists. The followers of Linnaeus in the "golden age of natural history," a few decades before, had increased the number of known species of fishes to about four hundred, of birds to one thousand, of insects to three thousand, and of plants to ten thousand. But now these sudden accessions from new territories doubled the figure for plants, tripled it for fish and birds, and brought the number of described insects above twenty thousand. Naturally enough, this wealth of new material was sorely puzzling to the classifiers. The more discerning began to see that the artificial system of Linnaeus, wonderful and useful as it had been, must be advanced upon before the new material could be satisfactorily disposed of. The way to a more natural system, based on less arbitrary signs, had been pointed out by Jussieu in botany, but the zoologists were not prepared to make headway towards such a system until they should gain a wider understanding of the organisms with which they had to deal through comprehensive studies of anatomy. Such studies of individual forms in their relations to the entire scale of organic beings were pursued in these last decades of the century, but though two or three most important generalizations were achieved (notably Kaspar Wolff's conception of the cell as the basis of organic life, and Goethe's all-important doctrine of metamorphosis of parts), yet, as a whole, the work of the anatomists of the period was germinative rather than fruit-bearing. Bichat's volumes, telling of the recognition of the fundamental tissues of the body, did not begin to appear till the last year of the century. The announcement by Cuvier of the doctrine of correlation of parts bears the same date, but in general the studies of this great naturalist, which in due time were to stamp him as the successor of Linnaeus, were as yet only fairly begun.

V. ANATOMY AND PHYSIOLOGY IN THE NINETEENTH CENTURY

CUVIER AND THE CORRELATION OF PARTS

We have seen that the focal points of the physiological world towards the close of the eighteenth century were Italy and England, but when Spallanzani and Hunter passed away the scene shifted to France. The time was peculiarly propitious, as the recent advances in many lines of science had brought fresh data for the student of animal life which were in need of classification, and, as several minds capable of such a task were in the field, it was natural that great generalizations should have come to be quite the fashion. Thus it was that Cuvier came forward with a brand-new classification of the animal kingdom, establishing four great types of being, which he called vertebrates, mollusks, articulates, and radiates. Lamarck had shortly before established the broad distinction between animals with and those without a backbone; Cuvier's Classification divided the latter--the invertebrates--into three minor groups. And this division, familiar ever since to all students of zoology, has only in very recent years been supplanted, and then not by revolution, but by a further division, which the elaborate recent studies of lower forms of life seemed to make desirable.

In the course of those studies of comparative anatomy which led to his new classification, Cuvier's attention was called constantly to the peculiar co-ordination of parts in each individual organism. Thus an animal with sharp talons for catching living prey--as a member of the cat tribe--has also sharp teeth, adapted for tearing up the flesh of its victim, and a particular type of stomach, quite different from that of herbivorous creatures. This adaptation of all the parts of the animal to one another extends to the most diverse parts of the organism, and enables the skilled anatomist, from the observation of a single typical part, to draw inferences as to the structure of the entire animal--a fact which was of vast aid to Cuvier in his studies of paleontology. It did not enable Cuvier, nor does it enable any one else, to reconstruct fully the extinct animal from observation of a single bone, as has sometimes been asserted, but what it really does establish, in the hands of an expert, is sufficiently astonishing.

"While the study of the fossil remains of the greater quadrupeds is more satisfactory," he writes, "by the clear results which it affords, than that of the remains of other animals found in a fossil state, it is also complicated with greater and more numerous difficulties. Fossil shells are usually found quite entire, and retaining all the characters requisite for comparing them with the specimens contained in collections of natural history, or represented in the works of naturalists. Even the skeletons of fishes are found more or less entire, so that the general forms of their bodies can, for the most part, be ascertained, and usually, at least, their generic and specific characters are determinable, as these are mostly drawn from their solid parts. In quadrupeds, on the contrary, even when their entire skeletons are found, there is great difficulty in discovering their distinguishing characters, as these are chiefly founded upon their hairs and colors and other marks which have disappeared previous to their incrustation. It is also very rare to find any fossil skeletons of quadrupeds in any degree approaching to a complete state, as the strata for the most part only contain separate bones, scattered confusedly and almost always broken and reduced to fragments,

which are the only means left to naturalists for ascertaining the species or genera to which they have belonged.

"Fortunately comparative anatomy, when thoroughly understood, enables us to surmount all these difficulties, as a careful application of its principles instructs us in the correspondences and dissimilarities of the forms of organized bodies of different kinds, by which each may be rigorously ascertained from almost every fragment of its various parts and organs.

"Every organized individual forms an entire system of its own, all the parts of which naturally correspond, and concur to produce a certain definite purpose, by reciprocal reaction, or by combining towards the same end. Hence none of these separate parts can change their forms without a corresponding change in the other parts of the same animal, and consequently each of these parts, taken separately, indicates all the other parts to which it has belonged. Thus, as I have elsewhere shown, if the viscera of an animal are so organized as only to be fitted for the digestion of recent flesh, it is also requisite that the jaws should be so constructed as to fit them for devouring prey; the claws must be constructed for seizing and tearing it to pieces; the teeth for cutting and dividing its flesh; the entire system of the limbs, or organs of motion, for pursuing and overtaking it; and the organs of sense for discovering it at a distance. Nature must also have endowed the brain of the animal with instincts sufficient for concealing itself and for laying plans to catch its necessary victims. . . . .

"To enable the animal to carry off its prey when seized, a corresponding force is requisite in the muscles which elevate the head, and this necessarily gives rise to a determinate form of the vertebrae to which these muscles are attached and of the occiput into which they are inserted. In order that the teeth of a carnivorous animal may be able to cut the flesh, they require to be sharp, more or less so in proportion to the greater or less quantity of flesh that they have to cut. It is requisite that their roots should be solid and strong, in proportion to the quantity and size of the bones which they have to break to pieces. The whole of these circumstances must necessarily influence the development and form of all the parts which contribute to move the jaws. . . . .

After these observations, it will be easily seen that similar conclusions may be drawn with respect to the limbs of carnivorous animals, which require particular conformations to fit them for rapidity of motion in general; and that similar considerations must influence the forms and connections of the vertebrae and other bones constituting the trunk of the body, to fit them for flexibility and readiness of motion in all directions. The bones also of the nose, of the orbit, and of the ears require certain forms and structures to fit them for giving perfection to the senses of smell, sight, and hearing, so necessary to animals of prey. In short, the shape and structure of the teeth regulate the forms of the condyle, of the shoulder-blade, and of the claws, in the same manner as the equation of a curve regulates all its other properties; and as in regard to any particular curve all its properties may be ascertained by assuming each separate property as the foundation of a particular equation, in the same manner a claw, a shoulder-blade, a condyle, a leg or arm bone, or any other bone separately considered, enables us to discover the description of teeth to which they have belonged; and so also reciprocally we may determine the forms of the other bones from the teeth. Thus commencing our investigations by a careful survey of any one bone by itself, a person who is sufficiently master of the laws of organic structure may, as it were, reconstruct the whole animal to which that bone belonged."[1]

We have already pointed out that no one is quite able to perform the necromantic feat suggested in the last sentence; but the exaggeration is pardonable in the enthusiast to whom the principle meant so much and in whose hands it extended so far.

Of course this entire principle, in its broad outlines, is something with which every student of anatomy had been familiar from the time when anatomy was first studied, but the full expression of the "law of co-ordination," as Cuvier called it, had never been explicitly made before; and, notwithstanding its seeming obviousness, the exposition which Cuvier made of it in the introduction to his classical work on comparative anatomy, which was published during the first decade of the nineteenth century, ranks as a great discovery. It is one of those generalizations which serve as guideposts to other discoveries.

BICHAT AND THE BODILY TISSUES

Much the same thing may be said of another generalization regarding the animal body, which the brilliant young French physician Marie Francois Bichat made in calling attention to the fact that each vertebrate organism, including man, has really two quite different sets of organs--one set under volitional control, and serving the end of locomotion, the other removed from volitional control, and serving the ends of the "vital processes" of digestion, assimilation, and the like. He called these sets of organs the animal system and the organic system, respectively. The division thus pointed out was not quite new, for Grimaud, professor of physiology in the University of Montpellier, had earlier made what was substantially the same classification of the functions into "internal or digestive and external or locomotive"; but it was Bichat's exposition that gave currency to the idea.

Far more important, however, was another classification which Bichat put forward in his work on anatomy, published just at the beginning of the last century. This was the division of all animal structures into what Bichat called tissues, and the pointing out that there are really only a few kinds of these in the body, making up all the diverse organs. Thus muscular organs form one system; membranous organs another; glandular organs a third; the vascular mechanism a fourth, and so on. The distinction is so obvious that it seems rather difficult to conceive that it could have been overlooked by the earliest anatomists; but, in point of fact, it is only obvious because now it has been familiarly taught for almost a century. It had never been given explicit expression before the time of Bichat, though it is said that Bichat himself was somewhat indebted for it to his master, Desault, and to the famous alienist Pinel.

However that may be, it is certain that all subsequent anatomists have found Bichat's classification of the tissues of the utmost value in their studies of the animal functions. Subsequent advances were to show that the distinction between the various tissues is not really so fundamental as Bichat supposed, but that takes nothing from the practical value of the famous classification.

It was but a step from this scientific classification of tissues to a similar classification of the diseases affecting them, and this was one of the greatest steps towards placing medicine on the plane of an exact science. This subject of these branches completely fascinated Bichat, and he

exclaimed, enthusiastically: "Take away some fevers and nervous trouble, and all else belongs to the kingdom of pathological anatomy." But out of this enthusiasm came great results. Bichat practised as he preached, and, believing that it was only possible to understand disease by observing the symptoms carefully at the bedside, and, if the disease terminated fatally, by post-mortem examination, he was so arduous in his pursuit of knowledge that within a period of less than six months he had made over six hundred autopsies--a record that has seldom, if ever, been equalled. Nor were his efforts fruitless, as a single example will suffice to show. By his examinations he was able to prove that diseases of the chest, which had formerly been classed under the indefinite name "peripneumonia," might involve three different structures, the pleural sac covering the lungs, the lung itself, and the bronchial tubes, the diseases affecting these organs being known respectively as pleuritis, pneumonia, and bronchitis, each one differing from the others as to prognosis and treatment. The advantage of such an exact classification needs no demonstration.

LISTER AND THE PERFECTED MICROSCOPE

At the same time when these broad macroscopical distinctions were being drawn there were other workers who were striving to go even deeper into the intricacies of the animal mechanism with the aid of the microscope. This undertaking, however, was beset with very great optical difficulties, and for a long time little advance was made upon the work of preceding generations. Two great optical barriers, known technically as spherical and chromatic aberration--the one due to a failure of the rays of light to fall all in one plane when focalized through a lens, the other due to the dispersive action of the lens in breaking the white light into prismatic colors--confronted the makers of microscopic lenses, and seemed all but insuperable. The making of achromatic lenses for telescopes had been accomplished, it is true, by Dolland in the previous century, by the union of lenses of crown glass with those of flint glass, these two materials having different indices of refraction and dispersion. But, aside from the mechanical difficulties which arise when the lens is of the minute dimensions required for use with the microscope, other perplexities are introduced by the fact that the use of a wide pencil of light is a desideratum, in order to gain sufficient illumination when large magnification is to be secured.

In the attempt to overcome those difficulties, the foremost physical philosophers of the time came to the aid of the best opticians. Very early in the century, Dr. (afterwards Sir David) Brewster, the renowned Scotch physicist, suggested that certain advantages might accrue from the use of such gems as have high refractive and low dispersive indices, in place of lenses made of glass. Accordingly lenses were made of diamond, of sapphire, and so on, and with some measure of success. But in 1812 a much more important innovation was introduced by Dr. William Hyde Wollaston, one of the greatest and most versatile, and, since the death of Cavendish, by far the most eccentric of English natural philosophers. This was the suggestion to use two plano-convex lenses, placed at a prescribed distance apart, in lieu of the single double-convex lens generally used. This combination largely overcame the spherical aberration, and it gained immediate fame as the "Wollaston doublet."

To obviate loss of light in such a doublet from increase of reflecting surfaces, Dr. Brewster suggested filling the interspace between the two lenses with a cement having the same index of refraction as the lenses themselves--an improvement of manifest advantage. An improvement yet

more important was made by Dr. Wollaston himself in the introduction of the diaphragm to limit the field of vision between the lenses, instead of in front of the anterior lens. A pair of lenses thus equipped Dr. Wollaston called the periscopic microscope. Dr. Brewster suggested that in such a lens the same object might be attained with greater ease by grinding an equatorial groove about a thick or globular lens and filling the groove with an opaque cement. This arrangement found much favor, and came subsequently to be known as a Coddington lens, though Mr. Coddington laid no claim to being its inventor.

Sir John Herschel, another of the very great physicists of the time, also gave attention to the problem of improving the microscope, and in 1821 he introduced what was called an aplanatic combination of lenses, in which, as the name implies, the spherical aberration was largely done away with. It was thought that the use of this Herschel aplanatic combination as an eyepiece, combined with the Wollaston doublet for the objective, came as near perfection as the compound microscope was likely soon to come. But in reality the instrument thus constructed, though doubtless superior to any predecessor, was so defective that for practical purposes the simple microscope, such as the doublet or the Coddington, was preferable to the more complicated one.

Many opticians, indeed, quite despaired of ever being able to make a satisfactory refracting compound microscope, and some of them had taken up anew Sir Isaac Newton's suggestion in reference to a reflecting microscope. In particular, Professor Giovanni Battista Amici, a very famous mathematician and practical optician of Modena, succeeded in constructing a reflecting microscope which was said to be superior to any compound microscope of the time, though the events of the ensuing years were destined to rob it of all but historical value. For there were others, fortunately, who did not despair of the possibilities of the refracting microscope, and their efforts were destined before long to be crowned with a degree of success not even dreamed of by any preceding generation.

The man to whom chief credit is due for directing those final steps that made the compound microscope a practical implement instead of a scientific toy was the English amateur optician Joseph Jackson Lister. Combining mathematical knowledge with mechanical ingenuity, and having the practical aid of the celebrated optician Tulley, he devised formulae for the combination of lenses of crown glass with others of flint glass, so adjusted that the refractive errors of one were corrected or compensated by the other, with the result of producing lenses of hitherto unequalled powers of definition; lenses capable of showing an image highly magnified, yet relatively free from those distortions and fringes of color that had heretofore been so disastrous to true interpretation of magnified structures.

Lister had begun his studies of the lens in 1824, but it was not until 1830 that he contributed to the Royal Society the famous paper detailing his theories and experiments. Soon after this various continental opticians who had long been working along similar lines took the matter up, and their expositions, in particular that of Amici, introduced the improved compound microscope to the attention of microscopists everywhere. And it required but the most casual trial to convince the experienced observers that a new implement of scientific research had been placed in their hands which carried them a long step nearer the observation of the intimate physical processes which lie at the foundation of vital phenomena. For the physiologist this perfection of the compound microscope had the same significance that the, discovery of America had for the fifteenth-century

geographers--it promised a veritable world of utterly novel revelations. Nor was the fulfilment of that promise long delayed.

Indeed, so numerous and so important were the discoveries now made in the realm of minute anatomy that the rise of histology to the rank of an independent science may be said to date from this period. Hitherto, ever since the discovery of magnifying-glasses, there had been here and there a man, such as Leuwenhoek or Malpighi, gifted with exceptional vision, and perhaps unusually happy in his conjectures, who made important contributions to the knowledge of the minute structure of organic tissues; but now of a sudden it became possible for the veriest tyro to confirm or refute the laborious observations of these pioneers, while the skilled observer could step easily beyond the barriers of vision that hitherto were quite impassable. And so, naturally enough, the physiologists of the fourth decade of the nineteenth century rushed as eagerly into the new realm of the microscope as, for example, their successors of to-day are exploring the realm of the X-ray.

Lister himself, who had become an eager interrogator of the instrument he had perfected, made many important discoveries, the most notable being his final settlement of the long-mooted question as to the true form of the red corpuscles of the human blood. In reality, as everybody knows nowadays, these are biconcave disks, but owing to their peculiar figure it is easily possible to misinterpret the appearances they present when seen through a poor lens, and though Dr. Thomas Young and various other observers had come very near the truth regarding them, unanimity of opinion was possible only after the verdict of the perfected microscope was given.

These blood corpuscles are so infinitesimal in size that something like five millions of them are found in each cubic millimetre of the blood, yet they are isolated particles, each having, so to speak, its own personality. This, of course, had been known to microscopists since the days of the earliest lenses. It had been noticed, too, by here and there an observer, that certain of the solid tissues seemed to present something of a granular texture, as if they, too, in their ultimate constitution, were made up of particles. And now, as better and better lenses were constructed, this idea gained ground constantly, though for a time no one saw its full significance. In the case of vegetable tissues, indeed, the fact that little particles encased a membranous covering, and called cells, are the ultimate visible units of structure had long been known. But it was supposed that animal tissues differed radically from this construction. The elementary particles of vegetables "were regarded to a certain extent as individuals which composed the entire plant, while, on the other hand, no such view was taken of the elementary parts of animals."

ROBERT BROWN AND THE CELL NUCLEUS

In the year 1833 a further insight into the nature of the ultimate particles of plants was gained through the observation of the English microscopist Robert Brown, who, in the course of his microscopic studies of the epidermis of orchids, discovered in the cells "an opaque spot," which he named the nucleus. Doubtless the same "spot" had been seen often enough before by other observers, but Brown was the first to recognize it as a component part of the vegetable cell and to give it a name.

"I shall conclude my observations on Orchideae," said Brown, "with a notice of some points of their general structure, which chiefly relate to the cellular tissue. In each cell of the epidermis of a great part of this family, especially of those with membranous leaves, a single circular areola, generally somewhat more opaque than, the membrane of the cell, is observable. This areola, which is more or less distinctly granular, is slightly convex, and although it seems to be on the surface is in reality covered by the outer lamina of the cell. There is no regularity as to its place in the cell; it is not unfrequently, however, central or nearly so.

"As only one areola belongs to each cell, and as in many cases where it exists in the common cells of the epidermis, it is also visible in the cutaneous glands or stomata, and in these is always double--one being on each side of the limb--it is highly probable that the cutaneous gland is in all cases composed of two cells of peculiar form, the line of union being the longitudinal axis of the disk or pore.

"This areola, or nucleus of the cell as perhaps it might be termed, is not confined to the epidermis, being also found, not only in the pubescence of the surface, particularly when jointed, as in cypripedium, but in many cases in the parenchyma or internal cells of the tissue, especially when these are free from the deposition of granular matter.

"In the compressed cells of the epidermis the nucleus is in a corresponding degree flattened; but in the internal tissue it is often nearly spherical, more or less firmly adhering to one of the walls, and projecting into the cavity of the cell. In this state it may not unfrequently be found. in the substance of the column and in that of the perianthium.

"The nucleus is manifest also in the tissue of the stigma, where in accordance with the compression of the utriculi, it has an intermediate form, being neither so much flattened as in the epidermis nor so convex as it is in the internal tissue of the column.

"I may here remark that I am acquainted with one case of apparent exception to the nucleus being solitary in each utriculus or cell--namely, in Bletia Tankervilliae. In the utriculi of the stigma of this plant, I have generally, though not always, found a second areola apparently on the surface, and composed of much larger granules than the ordinary nucleus, which is formed of very minute granular matter, and seems to be deep seated.

"Mr. Bauer has represented the tissue of the stigma, in the species of Bletia, both before and, as he believes, after impregnation; and in the latter state the utriculi are marked with from one to three areolae of similar appearance.

"The nucleus may even be supposed to exist in the pollen of this family. In the early stages of its formation, at least a minute areola is of ten visible in the simple grain, and in each of the constituent parts of cells of the compound grain. But these areolae may perhaps rather be considered as merely the points of production of the tubes.

"This nucleus of the cell is not confined to orchideae, but is equally manifest in many other monocotyledonous families; and I have even found it, hitherto however in very few cases, in the epidermis of dicotyledonous plants; though in this primary division it may perhaps be said to exist

in the early stages of development of the pollen. Among monocotyledons, the orders in which it is most remarkable are Liliaceae, Hemerocallideae, Asphodeleae, Irideae, and Commelineae.

"In some plants belonging to this last-mentioned family, especially in Tradascantia virginica, and several nearly related species, it is uncommonly distinct, not in the epidermis and in the jointed hairs of the filaments, but in the tissue of the stigma, in the cells of the ovulum even before impregnation, and in all the stages of formation of the grains of pollen, the evolution of which is so remarkable in tradascantia.

"The few indications of the presence of this nucleus, or areola, that I have hitherto met with in the publications of botanists are chiefly in some figures of epidermis, in the recent works of Meyen and Purkinje, and in one case, in M. Adolphe Broigniart's memoir on the structure of leaves. But so little importance seems to be attached to it that the appearance is not always referred to in the explanations of the figures in which it is represented. Mr. Bauer, however, who has also figured it in the utriculi of the stigma of Bletia Tankervilliae has more particularly noticed it, and seems to consider it as only visible after impregnation."[2]

SCHLEIDEN AND SCHWANN AND THE CELL THEORY

That this newly recognized structure must be important in the economy of the cell was recognized by Brown himself, and by the celebrated German Meyen, who dealt with it in his work on vegetable physiology, published not long afterwards; but it remained for another German, the professor of botany in the University of Jena, Dr. M. J. Schleiden, to bring the nucleus to popular attention, and to assert its all-importance in the economy of the cell.

Schleiden freely acknowledged his indebtedness to Brown for first knowledge of the nucleus, but he soon carried his studies of that structure far beyond those of its discoverer. He came to believe that the nucleus is really the most important portion of the cell, in that it is the original structure from which the remainder of the cell is developed. Hence he named it the cytoblast. He outlined his views in an epochal paper published in Muller's Archives in 1838, under title of "Beitrage zur Phytogenesis." This paper is in itself of value, yet the most important outgrowth of Schleiden's observations of the nucleus did not spring from his own labors, but from those of a friend to whom he mentioned his discoveries the year previous to their publication. This friend was Dr. Theodor Schwann, professor of physiology in the University of Louvain.

At the moment when these observations were communicated to him Schwann was puzzling over certain details of animal histology which he could not clearly explain. His great teacher, Johannes Muller, had called attention to the strange resemblance to vegetable cells shown by certain cells of the chorda dorsalis (the embryonic cord from which the spinal column is developed), and Schwann himself had discovered a corresponding similarity in the branchial cartilage of a tadpole. Then, too, the researches of Friedrich Henle had shown that the particles that make up the epidermis of animals are very cell-like in appearance. Indeed, the cell-like character of certain animal tissues had come to be matter of common note among students of minute anatomy. Schwann felt that this similarity could not be mere coincidence, but he had gained no clew to further insight until Schleiden called his attention to the nucleus. Then at once he reasoned that if there really is the

correspondence between vegetable and animal tissues that he suspected, and if the nucleus is so important in the vegetable cell as Schleiden believed, the nucleus should also be found in the ultimate particles of animal tissues.

Schwann's researches soon showed the entire correctness of this assumption. A closer study of animal tissues under the microscope showed, particularly in the case of embryonic tissues, that "opaque spots" such as Schleiden described are really to be found there in abundance--forming, indeed, a most characteristic phase of the structure. The location of these nuclei at comparatively regular intervals suggested that they are found in definite compartments of the tissue, as Schleiden had shown to be the case with vegetables; indeed, the walls that separated such cell-like compartments one from another were in some cases visible. Particularly was this found to be the case with embryonic tissues, and the study of these soon convinced Schwann that his original surmise had been correct, and that all animal tissues are in their incipiency composed of particles not unlike the ultimate particles of vegetables in short, of what the botanists termed cells. Adopting this name, Schwann propounded what soon became famous as his cell theory, under title of Mikroskopische Untersuchungen uber die Ubereinstimmung in der Structur und dent Wachsthum der Thiere und Pflanzen. So expeditious had been his work that this book was published early in 1839, only a few months after the appearance of Schleiden's paper.

As the title suggests, the main idea that actuated Schwann was to unify vegetable and animal tissues. Accepting cell-structure as the basis of all vegetable tissues, he sought to show that the same is true of animal tissues, all the seeming diversities of fibre being but the alteration and development of what were originally simple cells. And by cell Schwann meant, as did Schleiden also, what the word ordinarily implies--a cavity walled in on all sides. He conceived that the ultimate constituents of all tissues were really such minute cavities, the most important part of which was the cell wall, with its associated nucleus. He knew, indeed, that the cell might be filled with fluid contents, but he regarded these as relatively subordinate in importance to the wall itself. This, however, did not apply to the nucleus, which was supposed to lie against the cell wall and in the beginning to generate it. Subsequently the wall might grow so rapidly as to dissociate itself from its contents, thus becoming a hollow bubble or true cell; but the nucleus, as long as it lasted, was supposed to continue in contact with the cell wall. Schleiden had even supposed the nucleus to be a constituent part of the wall, sometimes lying enclosed between two layers of its substance, and Schwann quoted this view with seeming approval. Schwann believed, however, that in the mature cell the nucleus ceased to be functional and disappeared.

The main thesis as to the similarity of development of vegetable and animal tissues and the cellular nature of the ultimate constitution of both was supported by a mass of carefully gathered evidence which a multitude of microscopists at once confirmed, so Schwann's work became a classic almost from the moment of its publication. Of course various other workers at once disputed Schwann's claim to priority of discovery, in particular the English microscopist Valentin, who asserted, not without some show of justice, that he was working closely along the same lines. Put so, for that matter, were numerous others, as Henle, Turpin, Du-mortier, Purkinje, and Muller, all of whom Schwann himself had quoted. Moreover, there were various physiologists who earlier than any of these had foreshadowed the cell theory--notably Kaspar Friedrich Wolff, towards the close of the previous century, and Treviranus about 1807, But, as we have seen in so many other departments of science, it is one thing to foreshadow a discovery, it is quite another to give it full expression and

make it germinal of other discoveries. And when Schwann put forward the explicit claim that "there is one universal principle of development for the elementary parts, of organisms, however different, and this principle is the formation of cells," he enunciated a doctrine which was for all practical purposes absolutely new and opened up a novel field for the microscopist to enter. A most important era in physiology dates from the publication of his book in 1839.

THE CELL THEORY ELABORATED

That Schwann should have gone to embryonic tissues for the establishment of his ideas was no doubt due very largely to the influence of the great Russian Karl Ernst von Baer, who about ten years earlier had published the first part of his celebrated work on embryology, and whose ideas were rapidly gaining ground, thanks largely to the advocacy of a few men, notably Johannes Muller, in Germany, and William B. Carpenter, in England, and to the fact that the improved microscope had made minute anatomy popular. Schwann's researches made it plain that the best field for the study of the animal cell is here, and a host of explorers entered the field. The result of their observations was, in the main, to confirm the claims of Schwann as to the universal prevalence of the cell. The long-current idea that animal tissues grow only as a sort of deposit from the blood-vessels was now discarded, and the fact of so-called plantlike growth of animal cells, for which Schwann contended, was universally accepted. Yet the full measure of the affinity between the two classes of cells was not for some time generally apprehended.

Indeed, since the substance that composes the cell walls of plants is manifestly very different from the limiting membrane of the animal cell, it was natural, so long as the, wall was considered the most essential part of the structure, that the divergence between the two classes of cells should seem very pronounced. And for a time this was the conception of the matter that was uniformly accepted. But as time went on many observers had their attention called to the peculiar characteristics of the contents of the cell, and were led to ask themselves whether these might not be more important than had been supposed. In particular, Dr. Hugo von Mohl, professor of botany in the University of Tubingen, in the course of his exhaustive studies of the vegetable cell, was impressed with the peculiar and characteristic appearance of the cell contents. He observed universally within the cell "an opaque, viscid fluid, having granules intermingled in it," which made up the main substance of the cell, and which particularly impressed him because under certain conditions it could be seen to be actively in motion, its parts separated into filamentous streams.

Von Mohl called attention to the fact that this motion of the cell contents had been observed as long ago as 1774 by Bonaventura Corti, and rediscovered in 1807 by Treviranus, and that these observers had described the phenomenon under the "most unsuitable name of 'rotation of the cell sap.' Von Mohl recognized that the streaming substance was something quite different from sap. He asserted that the nucleus of the cell lies within this substance and not attached to the cell wall as Schleiden had contended. He saw, too, that the chlorophyl granules, and all other of the cell contents, are incorporated with the "opaque, viscid fluid," and in 1846 he had become so impressed with the importance of this universal cell substance that be gave it the name of protoplasm. Yet in so doing he had no intention of subordinating the cell wall. The fact that Payen, in 1844, had demonstrated that the cell walls of all vegetables, high or low, are composed largely of one

substance, cellulose, tended to strengthen the position of the cell wall as the really essential structure, of which the protoplasmic contents were only subsidiary products.

Meantime, however, the students of animal histology were more and more impressed with the seeming preponderance of cell contents over cell walls in the tissues they studied. They, too, found the cell to be filled with a viscid, slimy fluid capable of motion. To this Dujardin gave the name of sarcode. Presently it came to be known, through the labors of Kolliker, Nageli, Bischoff, and various others, that there are numerous lower forms of animal life which seem to be composed of this sarcode, without any cell wall whatever. The same thing seemed to be true of certain cells of higher organisms, as the blood corpuscles. Particularly in the case of cells that change their shape markedly, moving about in consequence of the streaming of their sarcode, did it seem certain that no cell wall is present, or that, if present, its role must be insignificant.

And so histologists came to question whether, after all, the cell contents rather than the enclosing wall must not be the really essential structure, and the weight of increasing observations finally left no escape from the conclusion that such is really the case. But attention being thus focalized on the cell contents, it was at once apparent that there is a far closer similarity between the ultimate particles of vegetables and those of animals than had been supposed. Cellulose and animal membrane being now regarded as more by-products, the way was clear for the recognition of the fact that vegetable protoplasm and animal sarcode are marvellously similar in appearance and general properties. The closer the observation the more striking seemed this similarity; and finally, about 1860, it was demonstrated by Heinrich de Bary and by Max Schultze that the two are to all intents and purposes identical. Even earlier Remak had reached a similar conclusion, and applied Von Mohl's word protoplasm to animal cell contents, and now this application soon became universal. Thenceforth this protoplasm was to assume the utmost importance in the physiological world, being recognized as the universal "physical basis of life," vegetable and animal alike. This amounted to the logical extension and culmination of Schwann's doctrine as to the similarity of development of the two animate kingdoms. Yet at the, same time it was in effect the banishment of the cell that Schwann had defined. The word cell was retained, it is true, but it no longer signified a minute cavity. It now implied, as Schultze defined it, "a small mass of protoplasm endowed with the attributes of life." This definition was destined presently to meet with yet another modification, as we shall see; but the conception of the protoplasmic mass as the essential ultimate structure, which might or might not surround itself with a protective covering, was a permanent addition to physiological knowledge. The earlier idea had, in effect, declared the shell the most important part of the egg; this developed view assigned to the yolk its true position.

In one other important regard the theory of Schleiden and Schwann now became modified. This referred to the origin of the cell. Schwann had regarded cell growth as a kind of crystallization, beginning with the deposit of a nucleus about a granule in the intercellular substance--the cytoblastema, as Schleiden called it. But Von Mohl, as early as 1835, had called attention to the formation of new vegetable cells through the division of a pre-existing cell. Ehrenberg, another high authority of the time, contended that no such division occurs, and the matter was still in dispute when Schleiden came forward with his discovery of so-called free cell-formation within the parent cell, and this for a long time diverted attention from the process of division which Von Mohl had described. All manner of schemes of cell-formation were put forward during the ensuing years by a multitude of observers, and gained currency notwithstanding Von Mohl's reiterated contention

that there are really but two ways in which the formation of new cells takes place--namely, "first, through division of older cells; secondly, through the formation of secondary cells lying free in the cavity of a cell."

But gradually the researches of such accurate observers as Unger, Nageli, Kolliker, Reichart, and Remak tended to confirm the opinion of Von Mohl that cells spring only from cells, and finally Rudolf Virchow brought the matter to demonstration about 1860. His Omnis cellula e cellula became from that time one of the accepted data of physiology. This was supplemented a little later by Fleming's Omnis nucleus e nucleo, when still more refined methods of observation had shown that the part of the cell which always first undergoes change preparatory to new cell-formation is the all-essential nucleus. Thus the nucleus was restored to the important position which Schwann and Schleiden had given it, but with greatly altered significance. Instead of being a structure generated de novo from non-cellular substance, and disappearing as soon as its function of cell-formation was accomplished, the nucleus was now known as the central and permanent feature of every cell, indestructible while the cell lives, itself the division-product of a pre-existing nucleus, and the parent, by division of its substance, of other generations of nuclei. The word cell received a final definition as "a small mass of protoplasm supplied with a nucleus."

In this widened and culminating general view of the cell theory it became clear that every animate organism, animal or vegetable, is but a cluster of nucleated cells, all of which, in each individual case, are the direct descendants of a single primordial cell of the ovum. In the developed individuals of higher organisms the successive generations of cells become marvellously diversified in form and in specific functions; there is a wonderful division of labor, special functions being chiefly relegated to definite groups of cells; but from first to last there is no function developed that is not present, in a primitive way, in every cell, however isolated; nor does the developed cell, however specialized, ever forget altogether any one of its primordial functions or capacities. All physiology, then, properly interpreted, becomes merely a study of cellular activities; and the development of the cell theory takes its place as the great central generalization in physiology of the nineteenth century. Something of the later developments of this theory we shall see in another connection.

ANIMAL CHEMISTRY

Just at the time when the microscope was opening up the paths that were to lead to the wonderful cell theory, another novel line of interrogation of the living organism was being put forward by a different set of observers. Two great schools of physiological chemistry had arisen--one under guidance of Liebig and Wohler, in Germany, the other dominated by the great French master Jean Baptiste Dumas. Liebig had at one time contemplated the study of medicine, and Dumas had achieved distinction in connection with Prevost, at Geneva, in the field of pure physiology before he turned his attention especially to chemistry. Both these masters, therefore, and Wohler as well, found absorbing interest in those phases of chemistry that have to do with the functions of living tissues; and it was largely through their efforts and the labors of their followers that the prevalent idea that vital processes are dominated by unique laws was discarded and physiology was brought within the recognized province of the chemist. So at about the time when the microscope had taught that the cell is the really essential structure of the living organism, the chemists had come to

understand that every function of the organism is really the expression of a chemical change--that each cell is, in short, a miniature chemical laboratory. And it was this combined point of view of anatomist and chemist, this union of hitherto dissociated forces, that made possible the inroads into the unexplored fields of physiology that were effected towards the middle of the nineteenth century.

One of the first subjects reinvestigated and brought to proximal solution was the long-mooted question of the digestion of foods. Spallanzani and Hunter had shown in the previous century that digestion is in some sort a solution of foods; but little advance was made upon their work until 1824, when Prout detected the presence of hydrochloric acid in the gastric juice. A decade later Sprott and Boyd detected the existence of peculiar glands in the gastric mucous membrane; and Cagniard la Tour and Schwann independently discovered that the really active principle of the gastric juice is a substance which was named pepsin, and which was shown by Schwann to be active in the presence of hydrochloric acid.

Almost coincidently, in 1836, it was discovered by Purkinje and Pappenheim that another organ than the stomach--namely, the pancreas--has a share in digestion, and in the course of the ensuing decade it came to be known, through the efforts of Eberle, Valentin, and Claude Bernard, that this organ is all-important in the digestion of starchy and fatty foods. It was found, too, that the liver and the intestinal glands have each an important share in the work of preparing foods for absorption, as also has the saliva--that, in short, a coalition of forces is necessary for the digestion of all ordinary foods taken into the stomach.

And the chemists soon discovered that in each one of the essential digestive juices there is at least one substance having certain resemblances to pepsin, though acting on different kinds of food. The point of resemblance between all these essential digestive agents is that each has the remarkable property of acting on relatively enormous quantities of the substance which it can digest without itself being destroyed or apparently even altered. In virtue of this strange property, pepsin and the allied substances were spoken of as ferments, but more recently it is customary to distinguish them from such organized ferments as yeast by designating them enzymes. The isolation of these enzymes, and an appreciation of their mode of action, mark a long step towards the solution of the riddle of digestion, but it must be added that we are still quite in the dark as to the real ultimate nature of their strange activity.

In a comprehensive view, the digestive organs, taken as a whole, are a gateway between the outside world and the more intimate cells of the organism. Another equally important gateway is furnished by the lungs, and here also there was much obscurity about the exact method of functioning at the time of the revival of physiological chemistry. That oxygen is consumed and carbonic acid given off during respiration the chemists of the age of Priestley and Lavoisier had indeed made clear, but the mistaken notion prevailed that it was in the lungs themselves that the important burning of fuel occurs, of which carbonic acid is a chief product. But now that attention had been called to the importance of the ultimate cell, this misconception could not long hold its ground, and as early as 1842 Liebig, in the course of his studies of animal heat, became convinced that it is not in the lungs, but in the ultimate tissues to which they are tributary, that the true consumption of fuel takes place. Reviving Lavoisier's idea, with modifications and additions, Liebig contended, and in the face of opposition finally demonstrated, that the source of animal heat is really the consumption of

the fuel taken in through the stomach and the lungs. He showed that all the activities of life are really the product of energy liberated solely through destructive processes, amounting, broadly speaking, to combustion occurring in the ultimate cells of the organism. Here is his argument:

LIEBIG ON ANIMAL HEAT

"The oxygen taken into the system is taken out again in the same forms, whether in summer or in winter; hence we expire more carbon in cold weather, and when the barometer is high, than we do in warm weather; and we must consume more or less carbon in our food in the same proportion; in Sweden more than in Sicily; and in our more temperate climate a full eighth more in winter than in summer.

"Even when we consume equal weights of food in cold and warm countries, infinite wisdom has so arranged that the articles of food in different climates are most unequal in the proportion of carbon they contain. The fruits on which the natives of the South prefer to feed do not in the fresh state contain more than twelve per cent. of carbon, while the blubber and train-oil used by the inhabitants of the arctic regions contain from sixty-six to eighty per cent. of carbon.

"It is no difficult matter, in warm climates, to study moderation in eating, and men can bear hunger for a long time under the equator; but cold and hunger united very soon exhaust the body.

"The mutual action between the elements of the food and the oxygen conveyed by the circulation of the blood to every part of the body is the source of animal heat.

"All living creatures whose existence depends on the absorption of oxygen possess within themselves a source of heat independent of surrounding objects.

"This truth applies to all animals, and extends besides to the germination of seeds, to the flowering of plants, and to the maturation of fruits. It is only in those parts of the body to which arterial blood, and with it the oxygen absorbed in respiration, is conveyed that heat is produced. Hair, wool, or feathers do not possess an elevated temperature. This high temperature of the animal body, or, as it may be called, disengagement of heat, is uniformly and under all circumstances the result of the combination of combustible substance with oxygen.

"In whatever way carbon may combine with oxygen, the act of combination cannot take place without the disengagement of heat. It is a matter of indifference whether the combination takes place rapidly or slowly, at a high or at a low temperature; the amount of heat liberated is a constant quantity. The carbon of the food, which is converted into carbonic acid within the body, must give out exactly as much heat as if it had been directly burned in the air or in oxygen gas; the only difference is that the amount of heat produced is diffused over unequal times. In oxygen the combustion is more rapid and the heat more intense; in air it is slower, the temperature is not so high, but it continues longer.

"It is obvious that the amount of heat liberated must increase or diminish with the amount of oxygen introduced in equal times by respiration. Those animals which respire frequently, and

consequently consume much oxygen, possess a higher temperature than others which, with a body of equal size to be heated, take into the system less oxygen. The temperature of a child (102 degrees) is higher than that of an adult (99.5 degrees). That of birds (104 to 105.4 degrees) is higher than that of quadrupeds (98.5 to 100.4 degrees), or than that of fishes or amphibia, whose proper temperature is from 3.7 to 2.6 degrees higher than that of the medium in which they live. All animals, strictly speaking, are warm-blooded; but in those only which possess lungs is the temperature of the body independent of the surrounding medium.

"The most trustworthy observations prove that in all climates, in the temperate zones as well as at the equator or the poles, the temperature of the body in man, and of what are commonly called warm-blooded animals, is invariably the same; yet how different are the circumstances in which they live.

"The animal body is a heated mass, which bears the same relation to surrounding objects as any other heated mass. It receives heat when the surrounding objects are hotter, it loses heat when they are colder than itself. We know that the rapidity of cooling increases with the difference between the heated body and that of the surrounding medium--that is, the colder the surrounding medium the shorter the time required for the cooling of the heated body. How unequal, then, must be the loss of heat of a man at Palermo, where the actual temperature is nearly equal to that of the body, and in the polar regions, where the external temperature is from 70 to 90 degrees lower.

"Yet notwithstanding this extremely unequal loss of heat, experience has shown that the blood of an inhabitant of the arctic circle has a temperature as high as that of the native of the South, who lives in so different a medium. This fact, when its true significance is perceived, proves that the heat given off to the surrounding medium is restored within the body with great rapidity. This compensation takes place more rapidly in winter than in summer, at the pole than at the equator.

"Now in different climates the quantity of oxygen introduced into the system of respiration, as has been already shown, varies according to the temperature of the external air; the quantity of inspired oxygen increases with the loss of heat by external cooling, and the quantity of carbon or hydrogen necessary to combine with this oxygen must be increased in like ratio. It is evident that the supply of heat lost by cooling is effected by the mutual action of the elements of the food and the inspired oxygen, which combine together. To make use of a familiar, but not on that account a less just illustration, the animal body acts, in this respect, as a furnace, which we supply with fuel. It signifies nothing what intermediate forms food may assume, what changes it may undergo in the body, the last change is uniformly the conversion of carbon into carbonic acid and of its hydrogen into water; the unassimilated nitrogen of the food, along with the unburned or unoxidized carbon, is expelled in the excretions. In order to keep up in a furnace a constant temperature, we must vary the supply of fuel according to the external temperature--that is, according to the supply of oxygen.

"In the animal body the food is the fuel; with a proper supply of oxygen we obtain the heat given out during its oxidation or combustion."[3]

BLOOD CORPUSCLES, MUSCLES, AND GLANDS

Further researches showed that the carriers of oxygen, from the time of its absorption in the lungs till its liberation in the ultimate tissues, are the red corpuscles, whose function had been supposed to be the mechanical one of mixing of the blood. It transpired that the red corpuscles are composed chiefly of a substance which Kuhne first isolated in crystalline form in 1865, and which was named haemoglobin--a substance which has a marvellous affinity for oxygen, seizing on it eagerly at the lungs vet giving it up with equal readiness when coursing among the remote cells of the body. When freighted with oxygen it becomes oxyhaemoglobin and is red in color; when freed from its oxygen it takes a purple hue; hence the widely different appearance of arterial and venous blood, which so puzzled the early physiologists.

This proof of the vitally important role played by the red-blood corpuscles led, naturally, to renewed studies of these infinitesimal bodies. It was found that they may vary greatly in number at different periods in the life of the same individual, proving that they may be both developed and destroyed in the adult organism. Indeed, extended observations left no reason to doubt that the process of corpuscle formation and destruction may be a perfectly normal one--that, in short, every red-blood corpuscle runs its course and dies like any more elaborate organism. They are formed constantly in the red marrow of bones, and are destroyed in the liver, where they contribute to the formation of the coloring matter of the bile. Whether there are other seats of such manufacture and destruction of the corpuscles is not yet fully determined. Nor are histologists agreed as to whether the red-blood corpuscles themselves are to be regarded as true cells, or merely as fragments of cells budded out from a true cell for a special purpose; but in either case there is not the slightest doubt that the chief function of the red corpuscle is to carry oxygen.

If the oxygen is taken to the ultimate cells before combining with the combustibles it is to consume, it goes without saying that these combustibles themselves must be carried there also. Nor could it be in doubt that the chiefest of these ultimate tissues, as regards, quantity of fuel required, are the muscles. A general and comprehensive view of the organism includes, then, digestive apparatus and lungs as the channels of fuel-supply; blood and lymph channels as the transportation system; and muscle cells, united into muscle fibres, as the consumption furnaces, where fuel is burned and energy transformed and rendered available for the purposes of the organism, supplemented by a set of excretory organs, through which the waste products--the ashes--are eliminated from the system.

But there remain, broadly speaking, two other sets of organs whose size demonstrates their importance in the economy of the organism, yet whose functions are not accounted for in this synopsis. These are those glandlike organs, such as the spleen, which have no ducts and produce no visible secretions, and the nervous mechanism, whose central organs are the brain and spinal cord. What offices do these sets of organs perform in the great labor-specializing aggregation of cells which we call a living organism?

As regards the ductless glands, the first clew to their function was given when the great Frenchman Claude Bernard (the man of whom his admirers loved to say, "He is not a physiologist merely; he is physiology itself") discovered what is spoken of as the glycogenic function of the liver. The liver itself, indeed, is not a ductless organ, but the quantity of its biliary output seems utterly disproportionate to its enormous size, particularly when it is considered that in the case of the human species the liver contains normally about one-fifth of all the blood in the entire body. Bernard discovered that the blood undergoes a change of composition in passing through the liver.

The liver cells (the peculiar forms of which had been described by Purkinje, Henle, and Dutrochet about 1838) have the power to convert certain of the substances that come to them into a starchlike compound called glycogen, and to store this substance away till it is needed by the organism. This capacity of the liver cells is quite independent of the bile-making power of the same cells; hence the discovery of this glycogenic function showed that an organ may have more than one pronounced and important specific function. But its chief importance was in giving a clew to those intermediate processes between digestion and final assimilation that are now known to be of such vital significance in the economy of the organism.

In the forty odd years that have elapsed since this pioneer observation of Bernard, numerous facts have come to light showing the extreme importance of such intermediate alterations of food-supplies in the blood as that performed by the liver. It has been shown that the pancreas, the spleen, the thyroid gland, the suprarenal capsules are absolutely essential, each in its own way, to the health of the organism, through metabolic changes which they alone seem capable of performing; and it is suspected that various other tissues, including even the muscles themselves, have somewhat similar metabolic capacities in addition to their recognized functions. But so extremely intricate is the chemistry of the substances involved that in no single case has the exact nature of the metabolisms wrought by these organs been fully made out. Each is in its way a chemical laboratory indispensable to the right conduct of the organism, but the precise nature of its operations remains inscrutable. The vast importance of the operations of these intermediate organs is unquestioned.

A consideration of the functions of that other set of organs known collectively as the nervous system is reserved for a later chapter.

VI. THEORIES OF ORGANIC EVOLUTION

GOETHE AND THE METAMORPHOSIS OF PARTS

When Coleridge said of Humphry Davy that he might have been the greatest poet of his time had he not chosen rather to be the greatest chemist, it is possible that the enthusiasm of the friend outweighed the caution of the critic. But however that may be, it is beyond dispute that the man who actually was the greatest poet of that time might easily have taken the very highest rank as a scientist had not the muse distracted his attention. Indeed, despite these distractions, Johann Wolfgang von Goethe achieved successes in the field of pure science that would insure permanent recognition for his name had he never written a stanza of poetry. Such is the versatility that marks the highest genius.

It was in 1790 that Goethe published the work that laid the foundations of his scientific reputation--the work on the Metamorphoses of Plants, in which he advanced the novel doctrine that all parts of the flower are modified or metamorphosed leaves.

"Every one who observes the growth of plants, even superficially," wrote Goethe, "will notice that certain external parts of them become transformed at times and go over into the forms of the contiguous parts, now completely, now to a greater or less degree. Thus, for example, the single flower is transformed into a double one when, instead of stamens, petals are developed, which are either exactly like the other petals of the corolla in form, and color or else still bear visible signs of their origin.

"When we observe that it is possible for a plant in this way to take a step backward, we shall give so much the more heed to the regular course of nature and learn the laws of transformation according to which she produces one part through another, and displays the most varying forms through the modification of one single organ.

"Let us first direct our attention to the plant at the moment when it develops out of the seed-kernel. The first organs of its upward growth are known by the name of cotyledons; they have also been called seed-leaves.

"They often appear shapeless, filled with new matter, and are just as thick as they are broad. Their vessels are unrecognizable and are hardly to be distinguished from the mass of the whole; they bear almost no resemblance to a leaf, and we could easily be misled into regarding them as special organs. Occasionally, however, they appear as real leaves, their vessels are capable of the most minute development, their similarity to the following leaves does not permit us to take them for special organs, but we recognize them instead to be the first leaves of the stalk.

"The cotyledons are mostly double, and there is an observation to be made here which will appear still more important as we proceed--that is, that the leaves of the first node are often paired, even when the following leaves of the stalk stand alternately upon it. Here we see an approximation and a joining of parts which nature afterwards separates and places at a distance from one another. It is still more remarkable when the cotyledons take the form of many little leaves gathered about an axis, and the stalk which grows gradually from their midst produces the following leaves arranged around it singly in a whorl. This may be observed very exactly in the growth of the pinus species. Here a corolla of needles forms at the same time a calyx, and we shall have occasion to remember the present case in connection with similar phenomena later.

"On the other hand, we observe that even the cotyledons which are most like a leaf when compared with the following leaves of the stalk are always more undeveloped or less developed. This is chiefly noticeable in their margin which is extremely simple and shows few traces of indentation.

"A few or many of the next following leaves are often already present in the seed, and lie enclosed between the cotyledons; in their folded state they are known by the name of plumules. Their form, as compared with the cotyledons and the following leaves, varies in different plants. Their chief point of variance, however, from the cotyledons is that they are flat, delicate, and formed like real leaves generally. They are wholly green, rest on a visible node, and can no longer deny their relationship to the following leaves of the stalk, to which, however, they are usually still inferior, in so far as that their margin is not completely developed.

"The further development, however, goes on ceaselessly in the leaf, from node to node; its midrib is elongated, and more or less additional ribs stretch out from this towards the sides. The leaves now appear notched, deeply indented, or composed of several small leaves, in which last case they seem to form complete little branches. The date-palm furnishes a striking example of such a successive transformation of the simplest leaf form. A midrib is elongated through a succession of several leaves, the single fan-shaped leaf becomes torn and diverted, and a very complicated leaf is developed, which rivals a branch in form.

"The transition to inflorescence takes place more or less rapidly. In the latter case we usually observe that the leaves of the stalk loose their different external divisions, and, on the other hand, spread out more or less in their lower parts where they are attached to the stalk. If the transition takes place rapidly, the stalk, suddenly become thinner and more elongated since the node of the last-developed leaf, shoots up and collects several leaves around an axis at its end.

"That the petals of the calyx are precisely the same organs which have hitherto appeared as leaves on the stalk, but now stand grouped about a common centre in an often very different form, can, as it seems to me, be most clearly demonstrated. Already in connection with the cotyledons above, we noticed a similar working of nature. The first species, while they are developing out of the seed-kernel, display a radiate crown of unmistakable needles; and in the first childhood of these plants we see already indicated that force of nature whereby when they are older their flowering and fruit-giving state will be produced.

"We see this force of nature, which collects several leaves around an axis, produce a still closer union and make these approximated, modified leaves still more unrecognizable by joining them together either wholly or partially. The bell-shaped or so-called one-petalled calices represent these cloudy connected leaves, which, being more or less indented from above, or divided, plainly show their origin.

"We can observe the transition from the calyx to the corolla in more than one instance, for, although the color of the calyx is still usually green, and like the color of the leaves of the stalk, it nevertheless often varies in one or another of its parts--at the tips, the margins, the back, or even, the inward side--while the outer still remains on green.

"The relationship of the corolla to the leaves of the stalk is shown in more than one way, since on the stalks of some plants appear leaves which are already more or less colored long before they approach inflorescence; others are fully colored when near inflorescence. Nature also goes over at once to the corolla, sometimes by skipping over the organs of the calyx, and in such a case we likewise have an opportunity to observe that leaves of the stalk become transformed into petals. Thus on the stalk of tulips, for instance, there sometimes appears an almost completely developed and colored petal. Even more remarkable is the case when such a leaf, half green and half of it belonging to the stalk, remains attached to the latter, while another colored part is raised with the corolla, and the leaf is thus torn in two.

"The relationship between the petals and stamens is very close. In some instances nature makes the transition regular--e.g., among the Canna and several plants of the same family. A true, little-modified petal is drawn together on its upper margin, and produces a pollen sac, while the rest of

the petal takes the place of the stamen. In double flowers we can observe this transition in all its stages. In several kinds of roses, within the fully developed and colored petals there appear other ones which are drawn together in the middle or on the side. This drawing together is produced by a small weal, which appears as a more or less complete pollen sac, and in the same proportion the leaf approaches the simple form of a stamen.

"The pistil in many cases looks almost like a stamen without anthers, and the relationship between the formation of the two is much closer than between the other parts. In retrograde fashion nature often produces cases where the style and stigma (Narben) become retransformed into petals--that is, the Ranunculus Asiaticus becomes double by transforming the stigma and style of the fruit-receptacle into real petals, while the stamens are often found unchanged immediately behind the corolla.

"In the seed receptacles, in spite of their formation, of their special object, and of their method of being joined together, we cannot fail to recognize the leaf form. Thus, for instance, the pod would be a simple leaf folded and grown together on its margin; the siliqua would consist of more leaves folded over another; the compound receptacles would be explained as being several leaves which, being united above one centre, keep their inward parts separate and are joined on their margins. We can convince ourselves of this by actual sight when such composite capsules fall apart after becoming ripe, because then every part displays an opened pod."[1]

The theory thus elaborated of the metamorphosis of parts was presently given greater generality through extension to the animal kingdom, in the doctrine which Goethe and Oken advanced independently, that the vertebrate skull is essentially a modified and developed vertebra. These were conceptions worthy of a poet--impossible, indeed, for any mind that had not the poetic faculty of correlation. But in this case the poet's vision was prophetic of a future view of the most prosaic science. The doctrine of metamorphosis of parts soon came to be regarded as of fundamental importance.

But the doctrine had implications that few of its early advocates realized. If all the parts of a flower--sepal, petal, stamen, pistil, with their countless deviations of contour and color--are but modifications of the leaf, such modification implies a marvellous differentiation and development. To assert that a stamen is a metamorphosed leaf means, if it means anything, that in the long sweep of time the leaf has by slow or sudden gradations changed its character through successive generations, until the offspring, so to speak, of a true leaf has become a stamen. But if such a metamorphosis as this is possible--if the seemingly wide gap between leaf and stamen may be spanned by the modification of a line of organisms--where does the possibility of modification of organic type find its bounds? Why may not the modification of parts go on along devious lines until the remote descendants of an organism are utterly unlike that organism? Why may we not thus account for the development of various species of beings all sprung from one parent stock? That, too, is a poet's dream; but is it only a dream? Goethe thought not. Out of his studies of metamorphosis of parts there grew in his mind the belief that the multitudinous species of plants and animals about us have been evolved from fewer and fewer earlier parent types, like twigs of a giant tree drawing their nurture from the same primal root. It was a bold and revolutionary thought, and the world regarded it as but the vagary of a poet.

ERASMUS DARWIN

Just at the time when this thought was taking form in Goethe's brain, the same idea was germinating in the mind of another philosopher, an Englishman of international fame, Dr. Erasmus Darwin, who, while he lived, enjoyed the widest popularity as a poet, the rhymed couplets of his Botanic Garden being quoted everywhere with admiration. And posterity repudiating the verse which makes the body of the book, yet grants permanent value to the book itself, because, forsooth, its copious explanatory foot-notes furnish an outline of the status of almost every department of science of the time.

But even though he lacked the highest art of the versifier, Darwin had, beyond peradventure, the imagination of a poet coupled with profound scientific knowledge; and it was his poetic insight, correlating organisms seemingly diverse in structure and imbuing the lowliest flower with a vital personality, which led him to suspect that there are no lines of demarcation in nature. "Can it be," he queries, "that one form of organism has developed from another; that different species are really but modified descendants of one parent stock?" The alluring thought nestled in his mind and was nurtured there, and grew in a fixed belief, which was given fuller expression in his Zoonomia and in the posthumous Temple of Nature.

Here is his rendering of the idea as versified in the Temple of Nature:

"Organic life beneath the shoreless waves Was born, and nursed in Ocean's pearly caves; First forms minute, unseen by spheric glass, Move on the mud, or pierce the watery mass; These, as successive generations bloom, New powers acquire and larger limbs assume; Whence countless groups of vegetation spring, And breathing realms of fin, and feet, and wing.

"Thus the tall Oak, the giant of the wood, Which bears Britannia's thunders on the flood; The Whale, unmeasured monster of the main; The lordly lion, monarch of the plain; The eagle, soaring in the realms of air, Whose eye, undazzled, drinks the solar glare; Imperious man, who rules the bestial crowd, Of language, reason, and reflection proud, With brow erect, who scorns this earthy sod, And styles himself the image of his God-- Arose from rudiments of form and sense, An embryon point or microscopic ens!"[2]

Here, clearly enough, is the idea of evolution. But in that day there was little proof forthcoming of its validity that could satisfy any one but a poet, and when Erasmus Darwin died, in 1802, the idea of transmutation of species was still but an unsubstantiated dream.

It was a dream, however, which was not confined to Goethe and Darwin. Even earlier the idea had come more or less vaguely to another great dreamer--and worker--of Germany, Immanuel Kant, and to several great Frenchmen, including De Maillet, Maupertuis, Robinet, and the famous naturalist Buffon--a man who had the imagination of a poet, though his message was couched in most artistic prose. Not long after the middle of the eighteenth century Buffon had put forward the idea of transmutation of species, and he reiterated it from time to time from then on till his death in 1788. But the time was not yet ripe for the idea of transmutation of species to burst its bonds.

And yet this idea, in a modified or undeveloped form, had taken strange hold upon the generation that was upon the scene at the close of the eighteenth century. Vast numbers of hitherto unknown species of animals had been recently discovered in previously unexplored regions of the globe, and the wise men were sorely puzzled to account for the disposal of all of these at the time of the deluge. It simplified matters greatly to suppose that many existing species had been developed since the episode of the ark by modification of the original pairs. The remoter bearings of such a theory were overlooked for the time, and the idea that American animals and birds, for example, were modified descendants of Old-World forms--the jaguar of the leopard, the puma of the lion, and so on--became a current belief with that class of humanity who accept almost any statement as true that harmonizes with their prejudices without realizing its implications.

Thus it is recorded with eclat that the discovery of the close proximity of America at the northwest with Asia removes all difficulties as to the origin of the Occidental faunas and floras, since Oriental species might easily have found their way to America on the ice, and have been modified as we find them by "the well-known influence of climate." And the persons who gave expression to this idea never dreamed of its real significance. In truth, here was the doctrine of evolution in a nutshell, and, because its ultimate bearings were not clear, it seemed the most natural of doctrines. But most of the persons who advanced it would have turned from it aghast could they have realized its import. As it was, however, only here and there a man like Buffon reasoned far enough to inquire what might be the limits of such assumed transmutation; and only here and there a Darwin or a Goethe reached the conviction that there are no limits.

LAMARCK VERSUS CUVIER

And even Goethe and Darwin had scarcely passed beyond that tentative stage of conviction in which they held the thought of transmutation of species as an ancillary belief not ready for full exposition. There was one of their contemporaries, however, who, holding the same conception, was moved to give it full explication. This was the friend and disciple of Buffon, Jean Baptiste de Lamarck. Possessed of the spirit of a poet and philosopher, this great Frenchman had also the widest range of technical knowledge, covering the entire field of animate nature. The first half of his long life was devoted chiefly to botany, in which he attained high distinction. Then, just at the beginning of the nineteenth century, he turned to zoology, in particular to the lower forms of animal life. Studying these lowly organisms, existing and fossil, he was more and more impressed with the gradations of form everywhere to be seen; the linking of diverse families through intermediate ones; and in particular with the predominance of low types of life in the earlier geological strata. Called upon constantly to classify the various forms of life in the course of his systematic writings, he found it more and more difficult to draw sharp lines of demarcation, and at last the suspicion long harbored grew into a settled conviction that there is really no such thing as a species of organism in nature; that "species" is a figment of the human imagination, whereas in nature there are only individuals.

That certain sets of individuals are more like one another than like other sets is of course patent, but this only means, said Lamarck, that these similar groups have had comparatively recent common ancestors, while dissimilar sets of beings are more remotely related in consanguinity. But trace back the lines of descent far enough, and all will culminate in one original stock. All forms of life

whatsoever are modified descendants of an original organism. From lowest to highest, then, there is but one race, one species, just as all the multitudinous branches and twigs from one root are but one tree. For purposes of convenience of description, we may divide organisms into orders, families, genera, species, just as we divide a tree into root, trunk, branches, twigs, leaves; but in the one case, as in the other, the division is arbitrary and artificial.

In Philosophie Zoologique (1809), Lamarck first explicitly formulated his ideas as to the transmutation of species, though he had outlined them as early as 1801. In this memorable publication not only did he state his belief more explicitly and in fuller detail than the idea had been expressed by any predecessor, but he took another long forward step, carrying him far beyond all his forerunners except Darwin, in that he made an attempt to explain the way in which the transmutation of species had been brought about. The changes have been wrought, he said, through the unceasing efforts of each organism to meet the needs imposed upon it by its environment. Constant striving means the constant use of certain organs. Thus a bird running by the seashore is constantly tempted to wade deeper and deeper in pursuit of food; its incessant efforts tend to develop its legs, in accordance with the observed principle that the use of any organ tends to strengthen and develop it. But such slightly increased development of the legs is transmitted to the off spring of the bird, which in turn develops its already improved legs by its individual efforts, and transmits the improved tendency. Generation after generation this is repeated, until the sum of the infinitesimal variations, all in the same direction, results in the production of the long-legged wading-bird. In a similar way, through individual effort and transmitted tendency, all the diversified organs of all creatures have been developed--the fin of the fish, the wing of the bird, the hand of man; nay, more, the fish itself, the bird, the man, even. Collectively the organs make up the entire organism; and what is true of the individual organs must be true also of their ensemble, the living being.

Whatever might be thought of Lamarck's explanation of the cause of transmutation--which really was that already suggested by Erasmus Darwin--the idea of the evolution for which he contended was but the logical extension of the conception that American animals are the modified and degenerated descendants of European animals. But people as a rule are little prone to follow ideas to their logical conclusions, and in this case the conclusions were so utterly opposed to the proximal bearings of the idea that the whole thinking world repudiated them with acclaim. The very persons who had most eagerly accepted the idea of transmutation of European species into American species, and similar limited variations through changed environment, because of the relief thus given the otherwise overcrowded ark, were now foremost in denouncing such an extension of the doctrine of transmutation as Lamarck proposed.

And, for that matter, the leaders of the scientific world were equally antagonistic to the Lamarckian hypothesis. Cuvier in particular, once the pupil of Lamarck, but now his colleague, and in authority more than his peer, stood out against the transmutation doctrine with all his force. He argued for the absolute fixity of species, bringing to bear the resources of a mind which, as a mere repository of facts, perhaps never was excelled. As a final and tangible proof of his position, he brought forward the bodies of ibises that had been embalmed by the ancient Egyptians, and showed by comparison that these do not differ in the slightest particular from the ibises that visit the Nile to-day.

Cuvier's reasoning has such great historical interest--being the argument of the greatest opponent of evolution of that day--that we quote it at some length.

"The following objections," he says, "have already been started against my conclusions. Why may not the presently existing races of mammiferous land quadrupeds be mere modifications or varieties of those ancient races which we now find in the fossil state, which modifications may have been produced by change of climate and other local circumstances, and since raised to the present excessive difference by the operations of similar causes during a long period of ages?

"This objection may appear strong to those who believe in the indefinite possibility of change of form in organized bodies, and think that, during a succession of ages and by alterations of habitudes, all the species may change into one another, or one of them give birth to all the rest. Yet to these persons the following answer may be given from their own system: If the species have changed by degrees, as they assume, we ought to find traces of this gradual modification. Thus, between the palaeotherium and the species of our own day, we should be able to discover some intermediate forms; and yet no such discovery has ever been made. Since the bowels of the earth have not preserved monuments of this strange genealogy, we have no right to conclude that the ancient and now extinct species were as permanent in their forms and characters as those which exist at present; or, at least, that the catastrophe which destroyed them did not leave sufficient time for the productions of the changes that are alleged to have taken place.

"In order to reply to those naturalists who acknowledge that the varieties of animals are restrained by nature within certain limits, it would be necessary to examine how far these limits extend. This is a very curious inquiry, and in itself exceedingly interesting under a variety of relations, but has been hitherto very little attended to. . . . .

Wild animals which subsist upon herbage feel the influence of climate a little more extensively, because there is added to it the influence of food, both in regard to its abundance and its quality. Thus the elephants of one forest are larger than those of another; their tusks also grow somewhat longer in places where their food may happen to be more favorable for the production of the substance of ivory. The same may take place in regard to the horns of stags and reindeer. But let us examine two elephants, the most dissimilar that can be conceived, we shall not discover the smallest difference in the number and articulations of the bones, the structure of the teeth, etc. . . . .

"Nature appears also to have guarded against the alterations of species which might proceed from mixture of breeds by influencing the various species of animals with mutual aversion from one another. Hence all the cunning and all the force that man is able to exert is necessary to accomplish such unions, even between species that have the nearest resemblances. And when the mule breeds that are thus produced by these forced conjunctions happen to be fruitful, which is seldom the case, this fecundity never continues beyond a few generations, and would not probably proceed so far without a continuance of the same cares which excited it at first. Thus we never see in a wild state intermediate productions between the hare and the rabbit, between the stag and the doe, or between the marten and the weasel. But the power of man changes this established order, and continues to produce all these intermixtures of which the various species are susceptible, but which they would never produce if left to themselves.

"The degrees of these variations are proportional to the intensity of the causes that produced them--namely, the slavery or subjection under which those animals are to man. They do not proceed far in half-domesticated species. In the cat, for example, a softer or harsher fur, more brilliant or more varied colors, greater or less size--these form the whole extent of variety in the species; the skeleton of the cat of Angora differs in no regular and constant circumstances from the wild-cat of Europe. . . . .

The most remarkable effects of the influence of man are produced upon that animal which he has reduced most completely under subjection. Dogs have been transported by mankind into every part of the world and have submitted their action to his entire direction. Regulated in their unions by the pleasure or caprice of their masters, the almost endless varieties of dogs differ from one another in color, in length, and abundance of hair, which is sometimes entirely wanting; in their natural instincts; in size, which varies in measure as one to five, mounting in some instances to more than a hundredfold in bulk; in the form of their ears, noses, and tails; in the relative length of their legs; in the progressive development of the brain, in several of the domesticated varieties occasioning alterations even in the form of the head, some of them having long, slender muzzles with a flat forehead, others having short muzzles with a forehead convex, etc., insomuch that the apparent difference between a mastiff and a water-spaniel and between a greyhound and a pugdog are even more striking than between almost any of the wild species of a genus. . . . .

It follows from these observations that animals have certain fixed and natural characters which resist the effects of every kind of influence, whether proceeding from natural causes or human interference; and we have not the smallest reason to suspect that time has any more effect on them than climate.

"I am aware that some naturalists lay prodigious stress upon the thousands which they can call into action by a dash of their pens. In such matters, however, our only way of judging as to the effects which may be produced by a long period of time is by multiplying, as it were, such as are produced by a shorter time. With this view I have endeavored to collect all the ancient documents respecting the forms of animals; and there are none equal to those furnished by the Egyptians, both in regard to their antiquity and abundance. They have not only left us representatives of animals, but even their identical bodies embalmed and preserved in the catacombs.

"I have examined, with the greatest attention, the engraved figures of quadrupeds and birds brought from Egypt to ancient Rome, and all these figures, one with another, have a perfect resemblance to their intended objects, such as they still are to-day.

"From all these established facts, there does not seem to be the smallest foundation for supposing that the new genera which I have discovered or established among extraneous fossils, such as the paleoetherium, anoplotherium, megalonyx, mastodon, pterodactylis, etc., have ever been the sources of any of our present animals, which only differ so far as they are influenced by time or climate. Even if it should prove true, which I am far from believing to be the case, that the fossil elephants, rhinoceroses, elks, and bears do not differ further from the existing species of the same genera than the present races of dogs differ among themselves, this would by no means be a sufficient reason to conclude that they were of the same species; since the races or varieties of dogs

have been influenced by the trammels of domesticity, which those other animals never did, and indeed never could, experience."[3]

To Cuvier's argument from the fixity of Egyptian mummified birds and animals, as above stated, Lamarck replied that this proved nothing except that the ibis had become perfectly adapted to its Egyptian surroundings in an early day, historically speaking, and that the climatic and other conditions of the Nile Valley had not since then changed. His theory, he alleged, provided for the stability of species under fixed conditions quite as well as for transmutation under varying conditions.

But, needless to say, the popular verdict lay with Cuvier; talent won for the time against genius, and Lamarck was looked upon as an impious visionary. His faith never wavered, however. He believed that he had gained a true insight into the processes of animate nature, and he reiterated his hypotheses over and over, particularly in the introduction to his Histoire Naturelle des Animaux sans Vertebres, in 1815, and in his Systeme des Connaissances Positives de l'Homme, in 1820. He lived on till 1829, respected as a naturalist, but almost unrecognized as a prophet.

TENTATIVE ADVANCES

While the names of Darwin and Goethe, and in particular that of Lamarck, must always stand out in high relief in this generation as the exponents of the idea of transmutation of species, there are a few others which must not be altogether overlooked in this connection. Of these the most conspicuous is that of Gottfried Reinhold Treviranus, a German naturalist physician, professor of mathematics in the lyceum at Bremen.

It was an interesting coincidence that Treviranus should have published the first volume of his Biologie, oder Philosophie der lebenden Natur, in which his views on the transmutation of species were expounded, in 1802, the same twelvemonth in which Lamarck's first exposition of the same doctrine appeared in his Recherches sur l'Organisation des Corps Vivants. It is singular, too, that Lamarck, in his Hydrogelogie of the same date, should independently have suggested "biology" as an appropriate word to express the general science of living things. It is significant of the tendency of thought of the time that the need of such a unifying word should have presented itself simultaneously to independent thinkers in different countries.

That same memorable year, Lorenz Oken, another philosophical naturalist, professor in the University of Zurich, published the preliminary outlines of his Philosophie der Natur, which, as developed through later publications, outlined a theory of spontaneous generation and of evolution of species. Thus it appears that this idea was germinating in the minds of several of the ablest men of the time during the first decade of our century. But the singular result of their various explications was to give sudden check to that undercurrent of thought which for some time had been setting towards this conception. As soon as it was made clear whither the concession that animals may be changed by their environment must logically trend, the recoil from the idea was instantaneous and fervid. Then for a generation Cuvier was almost absolutely dominant, and his verdict was generally considered final.

There was, indeed, one naturalist of authority in France who had the hardihood to stand out against Cuvier and his school, and who was in a position to gain a hearing, though by no means to divide the following. This was Etienne Geoffroy Saint-Hilaire, the famous author of the Philosophie Anatomique, and for many years the colleague of Lamarck at the Jardin des Plantes. Like Goethe, Geoffroy was pre-eminently an anatomist, and, like the great German, he had early been impressed with the resemblances between the analogous organs of different classes of beings. He conceived the idea that an absolute unity of type prevails throughout organic nature as regards each set of organs. Out of this idea grew his gradually formed belief that similarity of structure might imply identity of origin--that, in short, one species of animal might have developed from another.

Geoffroy's grasp of this idea of transmutation was by no means so complete as that of Lamarck, and he seems never to have fully determined in his own mind just what might be the limits of such development of species. Certainly he nowhere includes all organic creatures in one line of descent, as Lamarck had done; nevertheless, he held tenaciously to the truth as he saw it, in open opposition to Cuvier, with whom he held a memorable debate at the Academy of Sciences in 1830--the debate which so aroused the interest and enthusiasm of Goethe, but which, in the opinion of nearly every one else, resulted in crushing defeat for Geoffrey, and brilliant, seemingly final, victory for the advocate of special creation and the fixity of species.

With that all ardent controversy over the subject seemed to end, and for just a quarter of a century to come there was published but a single argument for transmutation of species which attracted any general attention whatever. This oasis in a desert generation was a little book called Vestiges of the Natural History of Creation, which appeared anonymously in England in 1844, and which passed through numerous editions, and was the subject of no end of abusive and derisive comment. This book, the authorship of which remained for forty years a secret, is now conceded to have been the work of Robert Chambers, the well-known English author and publisher. The book itself is remarkable as being an avowed and unequivocal exposition of a general doctrine of evolution, its view being as radical and comprehensive as that of Lamarck himself. But it was a resume of earlier efforts rather than a new departure, to say nothing of its technical shortcomings, which may best be illustrated by a quotation.

"The whole question," says Chambers, "stands thus: For the theory of universal order--that is, order as presiding in both the origin and administration of the world--we have the testimony of a vast number of facts in nature, and this one in addition--that whatever is left from the domain of ignorance, and made undoubted matter of science, forms a new support to the same doctrine. The opposite view, once predominant, has been shrinking for ages into lesser space, and now maintains a footing only in a few departments of nature which happen to be less liable than others to a clear investigation. The chief of these, if not almost the only one, is the origin of the organic kingdoms. So long as this remains obscure, the supernatural will have a certain hold upon enlightened persons. Should it ever be cleared up in a way that leaves no doubt of a natural origin of plants and animals, there must be a complete revolution in the view which is generally taken of the relation of the Father of our being.

"This prepares the way for a few remarks on the present state of opinion with regard to the origin of organic nature. The great difficulty here is the apparent determinateness of species. These forms of life being apparently unchangeable, or at least always showing a tendency to return to the character

from which they have diverged, the idea arises that there can have been no progression from one to another; each must have taken its special form, independently of other forms, directly from the appointment of the Creator. The Edinburgh Review writer says, 'they were created by the hand of God and adapted to the conditions of the period.' Now it is, in the first place, not certain that species constantly maintain a fixed character, for we have seen that what were long considered as determinate species have been transmuted into others. Passing, however, from this fact, as it is not generally received among men of science, there remain some great difficulties in connection with the idea of special creation. First we should have to suppose, as pointed out in my former volume, a most startling diversity of plan in the divine workings, a great general plan or system of law in the leading events of world-making, and a plan of minute, nice operation, and special attention in some of the mere details of the process. The discrepancy between the two conceptions is surely overpowering, when we allow ourselves to see the whole matter in a steady and rational light. There is, also, the striking fact of an ascertained historical progress of plants and animals in the order of their organization; marine and cellular plants and invertebrated animals first, afterwards higher examples of both. In an arbitrary system we had surely no reason to expect mammals after reptiles; yet in this order they came. The writer in the Edinburgh Review speaks of animals as coming in adaptation to conditions, but this is only true in a limited sense. The groves which formed the coal-beds might have been a fitting habitation for reptiles, birds, and mammals, as such groves are at the present day; yet we see none of the last of these classes and hardly any traces of the two first at that period of the earth. Where the iguanodon lived the elephant might have lived, but there was no elephant at that time. The sea of the Lower Silurian era was capable of supporting fish, but no fish existed. It hence forcibly appears that theatres of life must have remained unserviceable, or in the possession of a tenantry inferior to what might have enjoyed them, for many ages: there surely would have been no such waste allowed in a system where Omnipotence was working upon the plan of minute attention to specialities. The fact seems to denote that the actual procedure of the peopling of the earth was one of a natural kind, requiring a long space of time for its evolution. In this supposition the long existence of land without land animals, and more particularly without the noblest classes and orders, is only analogous to the fact, not nearly enough present to the minds of a civilized people, that to this day the bulk of the earth is a waste as far as man is concerned.

"Another startling objection is in the infinite local variation of organic forms. Did the vegetable and animal kingdoms consist of a definite number of species adapted to peculiarities of soil and climate, and universally distributed, the fact would be in harmony with the idea of special exertion. But the truth is that various regions exhibit variations altogether without apparent end or purpose. Professor Henslow enumerates forty-five distinct flowers or sets of plants upon the surface of the earth, notwithstanding that many of these would be equally suitable elsewhere. The animals of different continents are equally various, few species being the same in any two, though the general character may conform. The inference at present drawn from this fact is that there must have been, to use the language of the Rev. Dr. Pye Smith, 'separate and original creations, perhaps at different and respectively distinct epochs.' It seems hardly conceivable that rational men should give an adherence to such a doctrine when we think of what it involves. In the single fact that it necessitates a special fiat of the inconceivable Author of this sand-cloud of worlds to produce the flora of St. Helena, we read its more than sufficient condemnation. It surely harmonizes far better with our general ideas of nature to suppose that, just as all else in this far-spread science was formed on the laws impressed upon it at first by its Author, so also was this. An exception presented to us in such

a light appears admissible only when we succeed in forbidding our minds to follow out those reasoning processes to which, by another law of the Almighty, they tend, and for which they are adapted."[4]

Such reasoning as this naturally aroused bitter animadversions, and cannot have been without effect in creating an undercurrent of thought in opposition to the main trend of opinion of the time. But the book can hardly be said to have done more than that. Indeed, some critics have denied it even this merit. After its publication, as before, the conception of transmutation of species remained in the popular estimation, both lay and scientific, an almost forgotten "heresy."

It is true that here and there a scientist of greater or less repute--as Von Buch, Meckel, and Von Baer in Germany, Bory Saint-Vincent in France, Wells, Grant, and Matthew in England, and Leidy in America--had expressed more or less tentative dissent from the doctrine of special creation and immutability of species, but their unaggressive suggestions, usually put forward in obscure publications, and incidentally, were utterly overlooked and ignored. And so, despite the scientific advances along many lines at the middle of the century, the idea of the transmutability of organic races had no such prominence, either in scientific or unscientific circles, as it had acquired fifty years before. Special creation held the day, seemingly unopposed.

DARWIN AND THE ORIGIN OF SPECIES

But even at this time the fancied security of the special-creation hypothesis was by no means real. Though it seemed so invincible, its real position was that of an apparently impregnable fortress beneath which, all unbeknown to the garrison, a powder-mine has been dug and lies ready for explosion. For already there existed in the secluded work-room of an English naturalist, a manuscript volume and a portfolio of notes which might have sufficed, if given publicity, to shatter the entire structure of the special-creation hypothesis. The naturalist who, by dint of long and patient effort, had constructed this powder-mine of facts was Charles Robert Darwin, grandson of the author of Zoonomia.

As long ago as July 1, 1837, young Darwin, then twenty-eight years of age, had opened a private journal, in which he purposed to record all facts that came to him which seemed to have any bearing on the moot point of the doctrine of transmutation of species. Four or five years earlier, during the course of that famous trip around the world with Admiral Fitzroy, as naturalist to the Beagle, Darwin had made the personal observations which first tended to shake his belief of the fixity of species. In South America, in the Pampean formation, he had discovered "great fossil animals covered with armor like that on the existing armadillos," and had been struck with this similarity of type between ancient and existing faunas of the same region. He was also greatly impressed by the manner in which closely related species of animals were observed to replace one another as he proceeded southward over the continent; and "by the South-American character of most of the productions of the Galapagos Archipelago, and more especially by the manner in which they differ slightly on each island of the group, none of the islands appearing to be very ancient in a geological sense."

At first the full force of these observations did not strike him; for, under sway of Lyell's geological conceptions, he tentatively explained the relative absence of life on one of the Galapagos Islands by suggesting that perhaps no species had been created since that island arose. But gradually it dawned upon him that such facts as he had observed "could only be explained on the supposition that species gradually become modified." From then on, as he afterwards asserted, the subject haunted him; hence the journal of 1837.

It will thus be seen that the idea of the variability of species came to Charles Darwin as an inference from personal observations in the field, not as a thought borrowed from books. He had, of course, read the works of his grandfather much earlier in life, but the arguments of Zoonomia and The Temple of Nature had not served in the least to weaken his acceptance of the current belief in fixity of species. Nor had he been more impressed with the doctrine of Lamarck, so closely similar to that of his grandfather. Indeed, even after his South-American experience had aroused him to a new point of view he was still unable to see anything of value in these earlier attempts at an explanation of the variation of species. In opening his journal, therefore, he had no preconceived notion of upholding the views of these or any other makers of hypotheses, nor at the time had he formulated any hypothesis of his own. His mind was open and receptive; he was eager only for facts which might lead him to an understanding of a problem which seemed utterly obscure. It was something to feel sure that species have varied; but how have such variations been brought about?

It was not long before Darwin found a clew which he thought might lead to the answer he sought. In casting about for facts he had soon discovered that the most available field for observation lay among domesticated animals, whose numerous variations within specific lines are familiar to every one. Thus under domestication creatures so tangibly different as a mastiff and a terrier have sprung from a common stock. So have the Shetland pony, the thoroughbred, and the draught-horse. In short, there is no domesticated animal that has not developed varieties deviating more or less widely from the parent stock. Now, how has this been accomplished? Why, clearly, by the preservation, through selective breeding, of seemingly accidental variations. Thus one horseman, by constantly selecting animals that "chance" to have the right build and stamina, finally develops a race of running-horses; while another horseman, by selecting a different series of progenitors, has developed a race of slow, heavy draught animals.

So far, so good; the preservation of "accidental" variations through selective breeding is plainly a means by which races may be developed that are very different from their original parent form. But this is under man's supervision and direction. By what process could such selection be brought about among creatures in a state of nature? Here surely was a puzzle, and one that must be solved before another step could be taken in this direction.

The key to the solution of this puzzle came into Darwin's mind through a chance reading of the famous essay on "Population" which Thomas Robert Malthus had published almost half a century before. This essay, expositing ideas by no means exclusively original with Malthus, emphasizes the fact that organisms tend to increase at a geometrical ratio through successive generations, and hence would overpopulate the earth if not somehow kept in check. Cogitating this thought, Darwin gained a new insight into the processes of nature. He saw that in virtue of this tendency of each race of beings to overpopulate the earth, the entire organic world, animal and vegetable, must be in

a state of perpetual carnage and strife, individual against individual, fighting for sustenance and life.

That idea fully imagined, it becomes plain that a selective influence is all the time at work in nature, since only a few individuals, relatively, of each generation can come to maturity, and these few must, naturally, be those best fitted to battle with the particular circumstances in the midst of which they are placed. In other words, the individuals best adapted to their surroundings will, on the average, be those that grow to maturity and produce offspring. To these offspring will be transmitted the favorable peculiarities. Thus these peculiarities will become permanent, and nature will have accomplished precisely what the human breeder is seen to accomplish. Grant that organisms in a state of nature vary, however slightly, one from another (which is indubitable), and that such variations will be transmitted by a parent to its offspring (which no one then doubted); grant, further, that there is incessant strife among the various organisms, so that only a small proportion can come to maturity--grant these things, said Darwin, and we have an explanation of the preservation of variations which leads on to the transmutation of species themselves.

This wonderful coign of vantage Darwin had reached by 1839. Here was the full outline of his theory; here were the ideas which afterwards came to be embalmed in familiar speech in the phrases "spontaneous variation," and the "survival of the fittest," through "natural selection." After such a discovery any ordinary man would at once have run through the streets of science, so to speak, screaming "Eureka!" Not so Darwin. He placed the manuscript outline of his theory in his portfolio, and went on gathering facts bearing on his discovery. In 1844 he made an abstract in a manuscript book of the mass of facts by that time accumulated. He showed it to his friend Hooker, made careful provision for its publication in the event of his sudden death, then stored it away in his desk and went ahead with the gathering of more data. This was the unexploded powder-mine to which I have just referred.

Twelve years more elapsed--years during which the silent worker gathered a prodigious mass of facts, answered a multitude of objections that arose in his own mind, vastly fortified his theory. All this time the toiler was an invalid, never knowing a day free from illness and discomfort, obliged to husband his strength, never able to work more than an hour and a half at a stretch; yet he accomplished what would have been vast achievements for half a dozen men of robust health. Two friends among the eminent scientists of the day knew of his labors--Sir Joseph Hooker, the botanist, and Sir Charles Lyell, the geologist. Gradually Hooker had come to be more than half a convert to Darwin's views. Lyell was still sceptical, yet he urged Darwin to publish his theory without further delay lest he be forestalled. At last the patient worker decided to comply with this advice, and in 1856 he set to work to make another and fuller abstract of the mass of data he had gathered.

And then a strange thing happened. After Darwin had been at work on his "abstract" about two years, but before he had published a line of it, there came to him one day a paper in manuscript, sent for his approval by a naturalist friend named Alfred Russel Wallace, who had been for some time at work in the East India Archipelago. He read the paper, and, to his amazement, found that it contained an outline of the same theory of "natural selection" which he himself had originated and for twenty years had worked upon. Working independently, on opposite sides of the globe, Darwin and Wallace had hit upon the same explanation of the cause of transmutation of species. "Were

Wallace's paper an abstract of my unpublished manuscript of 1844," said Darwin, "it could not better express my ideas."

Here was a dilemma. To publish this paper with no word from Darwin would give Wallace priority, and wrest from Darwin the credit of a discovery which he had made years before his codiscoverer entered the field. Yet, on the other hand, could Darwin honorably do otherwise than publish his friend's paper and himself remain silent? It was a complication well calculated to try a man's soul. Darwin's was equal to the test. Keenly alive to the delicacy of the position, he placed the whole matter before his friends Hooker and Lyell, and left the decision as to a course of action absolutely to them. Needless to say, these great men did the one thing which insured full justice to all concerned. They counselled a joint publication, to include on the one hand Wallace's paper, and on the other an abstract of Darwin's ideas, in the exact form in which it had been outlined by the author in a letter to Asa Gray in the previous year--an abstract which was in Gray's hands before Wallace's paper was in existence. This joint production, together with a full statement of the facts of the case, was presented to the Linnaean Society of London by Hooker and Lyell on the evening of July 1, 1858, this being, by an odd coincidence, the twenty-first anniversary of the day on which Darwin had opened his journal to collect facts bearing on the "species question." Not often before in the history of science has it happened that a great theory has been nurtured in its author's brain through infancy and adolescence to its full legal majority before being sent out into the world.

Thus the fuse that led to the great powder-mine had been lighted. The explosion itself came more than a year later, in November, 1859, when Darwin, after thirteen months of further effort, completed the outline of his theory, which was at first begun as an abstract for the Linnaean Society, but which grew to the size of an independent volume despite his efforts at condensation, and which was given that ever-to-be-famous title, The Origin of Species by Means of Natural Selection, or the Preservation of Favored Races in the Struggle for Life. And what an explosion it was! The joint paper of 1858 had made a momentary flare, causing the hearers, as Hooker said, to "speak of it with bated breath," but beyond that it made no sensation. What the result was when the Origin itself appeared no one of our generation need be told. The rumble and roar that it made in the intellectual world have not yet altogether ceased to echo after more than forty years of reverberation.

NEW CHAMPIONS

To the Origin of Species, then, and to its author, Charles Darwin, must always be ascribed chief credit for that vast revolution in the fundamental beliefs of our race which has come about since 1859, and which made the second half of the century memorable. But it must not be overlooked that no such sudden metamorphosis could have been effected had it not been for the aid of a few notable lieutenants, who rallied to the standards of the leader immediately after the publication of the Origin. Darwin had all along felt the utmost confidence in the ultimate triumph of his ideas. "Our posterity," he declared, in a letter to Hooker, "will marvel as much about the current belief [in special creation] as we do about fossil shells having been thought to be created as we now see them." But he fully realized that for the present success of his theory of transmutation the championship of a few leaders of science was all-essential. He felt that if he could make converts of Hooker and Lyell and of Thomas Henry Huxley at once, all would be well.

His success in this regard, as in others, exceeded his expectations. Hooker was an ardent disciple from reading the proof-sheets before the book was published; Lyell renounced his former beliefs and fell into line a few months later; while Huxley, so soon as he had mastered the central idea of natural selection, marvelled that so simple yet all-potent a thought had escaped him so long, and then rushed eagerly into the fray, wielding the keenest dialectic blade that was drawn during the entire controversy. Then, too, unexpected recruits were found in Sir John Lubbock and John Tyndall, who carried the war eagerly into their respective territories; while Herbert Spencer, who had advocated a doctrine of transmutation on philosophic grounds some years before Darwin published the key to the mystery--and who himself had barely escaped independent discovery of that key--lent his masterful influence to the cause. In America the famous botanist Asa Gray, who had long been a correspondent of Darwin's but whose advocacy of the new theory had not been anticipated, became an ardent propagandist; while in Germany Ernst Heinrich Haeckel, the youthful but already noted zoologist, took up the fight with equal enthusiasm.

Against these few doughty champions--with here and there another of less general renown--was arrayed, at the outset, practically all Christendom. The interest of the question came home to every person of intelligence, whatever his calling, and the more deeply as it became more and more clear how far-reaching are the real bearings of the doctrine of natural selection. Soon it was seen that should the doctrine of the survival of the favored races through the struggle for existence win, there must come with it as radical a change in man's estimate of his own position as had come in the day when, through the efforts of Copernicus and Galileo, the world was dethroned from its supposed central position in the universe. The whole conservative majority of mankind recoiled from this necessity with horror. And this conservative majority included not laymen merely, but a vast preponderance of the leaders of science also.

With the open-minded minority, on the other hand, the theory of natural selection made its way by leaps and bounds. Its delightful simplicity--which at first sight made it seem neither new nor important--coupled with the marvellous comprehensiveness of its implications, gave it a hold on the imagination, and secured it a hearing where other theories of transmutation of species had been utterly scorned. Men who had found Lamarck's conception of change through voluntary effort ridiculous, and the vaporings of the Vestiges altogether despicable, men whose scientific cautions held them back from Spencer's deductive argument, took eager hold of that tangible, ever-present principle of natural selection, and were led on and on to its goal. Hour by hour the attitude of the thinking world towards this new principle changed; never before was so great a revolution wrought so suddenly.

Nor was this merely because "the times were ripe" or "men's minds prepared for evolution." Darwin himself bears witness that this was not altogether so. All through the years in which he brooded this theory he sounded his scientific friends, and could find among them not one who acknowledged a doctrine of transmutation. The reaction from the stand-point of Lamarck and Erasmus Darwin and Goethe had been complete, and when Charles Darwin avowed his own conviction he expected always to have it met with ridicule or contempt. In 1857 there was but one man speaking with any large degree of authority in the world who openly avowed a belief in transmutation of species--that man being Herbert Spencer. But the Origin of Species came, as Huxley has said, like a flash in the darkness, enabling the benighted voyager to see the way. The

score of years during which its author had waited and worked had been years well spent. Darwin had become, as he himself says, a veritable Croesus, "overwhelmed with his riches in facts"--facts of zoology, of selective artificial breeding, of geographical distribution of animals, of embryology, of paleontology. He had massed his facts about his theory, condensed them and recondensed, until his volume of five hundred pages was an encyclopaedia in scope. During those long years of musing he had thought out almost every conceivable objection to his theory, and in his book every such objection was stated with fullest force and candor, together with such reply as the facts at command might dictate. It was the force of those twenty years of effort of a master-mind that made the sudden breach in the breaswtork{sic} of current thought.

Once this breach was effected the work of conquest went rapidly on. Day by day squads of the enemy capitulated and struck their arms. By the time another score of years had passed the doctrine of evolution had become the working hypothesis of the scientific world. The revolution had been effected.

And from amid the wreckage of opinion and belief stands forth the figure of Charles Darwin, calm, imperturbable, serene; scatheless to ridicule, contumely, abuse; unspoiled by ultimate success; unsullied alike by the strife and the victory--take him for all in all, for character, for intellect, for what he was and what he did, perhaps the most Socratic figure of the century. When, in 1882, he died, friend and foe alike conceded that one of the greatest sons of men had rested from his labors, and all the world felt it fitting that the remains of Charles Darwin should be entombed in Westminster Abbey close beside the honored grave of Isaac Newton. Nor were there many who would dispute the justice of Huxley's estimate of his accomplishment: "He found a great truth trodden under foot. Reviled by bigots, and ridiculed by all the world, he lived long enough to see it, chiefly by his own efforts, irrefragably established in science, inseparably incorporated with the common thoughts of men, and only hated and feared by those who would revile but dare not."

THE ORIGIN OF THE FITTEST

Wide as are the implications of the great truth which Darwin and his co-workers established, however, it leaves quite untouched the problem of the origin of those "favored variations" upon which it operates. That such variations are due to fixed and determinate causes no one understood better than Darwin; but in his original exposition of his doctrine he made no assumption as to what these causes are. He accepted the observed fact of variation--as constantly witnessed, for example, in the differences between parents and offspring--and went ahead from this assumption.

But as soon as the validity of the principle of natural selection came to be acknowledged speculators began to search for the explanation of those variations which, for purposes of argument, had been provisionally called "spontaneous." Herbert Spencer had all along dwelt on this phase of the subject, expounding the Lamarckian conceptions of the direct influence of the environment (an idea which had especially appealed to Buffon and to Geoffroy Saint-Hilaire), and of effort in response to environment and stimulus as modifying the individual organism, and thus supplying the basis for the operation of natural selection. Haeckel also became an advocate of this idea, and presently there arose a so-called school of neo-Lamarckians, which developed particular strength and prominence in America under the leadership of Professors A. Hyatt and E. D. Cope.

But just as the tide of opinion was turning strongly in this direction, an utterly unexpected obstacle appeared in the form of the theory of Professor August Weismann, put forward in 1883, which antagonized the Lamarckian conception (though not touching the Darwinian, of which Weismann is a firm upholder) by denying that individual variations, however acquired by the mature organism, are transmissible. The flurry which this denial created has not yet altogether subsided, but subsequent observations seem to show that it was quite disproportionate to the real merits of the case. Notwithstanding Professor Weismann's objections, the balance of evidence appears to favor the view that the Lamarckian factor of acquired variations stands as the complement of the Darwinian factor of natural selection in effecting the transmutation of species.

Even though this partial explanation of what Professor Cope calls the "origin of the fittest" be accepted, there still remains one great life problem which the doctrine of evolution does not touch. The origin of species, genera, orders, and classes of beings through endless transmutations is in a sense explained; but what of the first term of this long series? Whence came that primordial organism whose transmuted descendants make up the existing faunas and floras of the globe?

There was a time, soon after the doctrine of evolution gained a hearing, when the answer to that question seemed to some scientists of authority to have been given by experiment. Recurring to a former belief, and repeating some earlier experiments, the director of the Museum of Natural History at Rouen, M. F. A. Pouchet, reached the conclusion that organic beings are spontaneously generated about us constantly, in the familiar processes of putrefaction, which were known to be due to the agency of microscopic bacteria. But in 1862 Louis Pasteur proved that this seeming spontaneous generation is in reality due to the existence of germs in the air. Notwithstanding the conclusiveness of these experiments, the claims of Pouchet were revived in England ten years later by Professor Bastian; but then the experiments of John Tyndall, fully corroborating the results of Pasteur, gave a final quietus to the claim of "spontaneous generation" as hitherto formulated.

There for the moment the matter rests. But the end is not yet. Fauna and flora are here, and, thanks to Lamarck and Wallace and Darwin, their development, through the operation of those "secondary causes" which we call laws of nature, has been proximally explained. The lowest forms of life have been linked with the highest in unbroken chains of descent. Meantime, through the efforts of chemists and biologists, the gap between the inorganic and the organic worlds, which once seemed almost infinite, has been constantly narrowed. Already philosophy can throw a bridge across that gap. But inductive science, which builds its own bridges, has not yet spanned the chasm, small though it appear. Until it shall have done so, the bridge of organic evolution is not quite complete; yet even as it stands to-day it is perhaps the most stupendous scientific structure of the nineteenth century.

VII. EIGHTEENTH-CENTURY MEDICINE

THE SYSTEM OF BOERHAAVE

At least two pupils of William Harvey distinguished themselves in medicine, Giorgio Baglivi (1669-1707), who has been called the "Italian Sydenham," and Hermann Boerhaave (1668-1738). The work of Baglivi was hardly begun before his early death removed one of the most promising of the early eighteenth-century physicians. Like Boerhaave, he represents a type of skilled, practical clinitian rather than the abstract scientist. One of his contributions to medical literature is the first accurate description of typhoid, or, as he calls it, mesenteric fever.

If for nothing else, Boerhaave must always be remembered as the teacher of Von Haller, but in his own day he was the widest known and the most popular teacher in the medical world. He was the idol of his pupils at Leyden, who flocked to his lectures in such numbers that it became necessary to "tear down the walls of Leyden to accommodate them." His fame extended not only all over Europe but to Asia, North America, and even into South America. A letter sent him from China was addressed to "Boerhaave in Europe." His teachings represent the best medical knowledge of his day, a high standard of morality, and a keen appreciation of the value of observation; and it was through such teachings imparted to his pupils and advanced by them, rather than to any new discoveries, that his name is important in medical history. His arrangement and classification of the different branches of medicine are interesting as representing the attitude of the medical profession towards these various branches at that time.

"In the first place we consider Life; then Health, afterwards Diseases; and lastly their several Remedies.

"Health the first general branch of Physic in our Institutions is termed Physiology, or the Animal Oeconomy; demonstrating the several Parts of the human Body, with their Mechanism and Actions.

"The second branch of Physic is called Pathology, treating of Diseases, their Differences, Causes and Effects, or Symptoms; by which the human Body is known to vary from its healthy state.

"The third part of Physic is termed Semiotica, which shows the Signs distinguishing between sickness and Health, Diseases and their Causes in the human Body; it also imports the State and Degrees of Health and Diseases, and presages their future Events.

"The fourth general branch of Physic is termed Hygiene, or Prophylaxis.

"The fifth and last part of Physic is called Therapeutica; which instructs us in the Nature, Preparation and uses of the Materia Medica; and the methods of applying the same, in order to cure Diseases and restore lost Health."[1]

From this we may gather that his general view of medicine was not unlike that taken at the present time.

Boerhaave's doctrines were arranged into a "system" by Friedrich Hoffmann, of Halle (1660-1742), this system having the merit of being simple and more easily comprehended than many others. In this system forces were considered inherent in matter, being expressed as mechanical movements,

and determined by mass, number, and weight. Similarly, forces express themselves in the body by movement, contraction, and relaxation, etc., and life itself is movement, "particularly movement of the heart." Life and death are, therefore, mechanical phenomena, health is determined by regularly recurring movements, and disease by irregularity of them. The body is simply a large hydraulic machine, controlled by "the aether" or "sensitive soul," and the chief centre of this soul lies in the medulla.

In the practical application of medicines to diseases Hoffman used simple remedies, frequently with happy results, for whatever the medical man's theory may be he seldom has the temerity to follow it out logically, and use the remedies indicated by his theory to the exclusion of long-established, although perhaps purely empirical, remedies. Consequently, many vague theorists have been excellent practitioners, and Hoffman was one of these. Some of the remedies he introduced are still in use, notably the spirits of ether, or "Hoffman's anodyne."

ANIMISTS, VITALISTS, AND ORGANICISTS

Besides Hoffman's system of medicine, there were numerous others during the eighteenth century, most of which are of no importance whatever; but three, at least, that came into existence and disappeared during the century are worthy of fuller notice. One of these, the Animists, had for its chief exponent Georg Ernst Stahl of "phlogiston" fame; another, the Vitalists, was championed by Paul Joseph Barthez (1734-1806); and the third was the Organicists. This last, while agreeing with the other two that vital activity cannot be explained by the laws of physics and chemistry, differed in not believing that life "was due to some spiritual entity," but rather to the structure of the body itself.

The Animists taught that the soul performed functions of ordinary life in man, while the life of lower animals was controlled by ordinary mechanical principles. Stahl supported this theory ardently, sometimes violently, at times declaring that there were "no longer any doctors, only mechanics and chemists." He denied that chemistry had anything to do with medicine, and, in the main, discarded anatomy as useless to the medical man. The soul, he thought, was the source of all vital movement; and the immediate cause of death was not disease but the direct action of the soul. When through some lesion, or because the machinery of the body has become unworkable, as in old age, the soul leaves the body and death is produced. The soul ordinarily selects the channels of the circulation, and the contractile parts, as the route for influencing the body. Hence in fever the pulse is quickened, due to the increased activity of the soul, and convulsions and spasmodic movements in disease are due, to the, same cause. Stagnation of the, blood was supposed to be a fertile cause of diseases, and such diseases were supposed to arise mostly from "plethora"--an all-important element in Stahl's therapeutics. By many this theory is regarded as an attempt on the part of the pious Stahl to reconcile medicine and theology in a way satisfactory to both physicians and theologians, but, like many conciliatory attempts, it was violently opposed by both doctors and ministers.

A belief in such a theory would lead naturally to simplicity in therapeutics, and in this respect at least Stahl was consistent. Since the soul knew more about the body than any physician could know, Stahl conceived that it would be a hinderance rather than a help for the physician to interfere

with complicated doses of medicine. As he advanced in age this view of the administration of drugs grew upon him, until after rejecting quinine, and finally opium, he at last used only salt and water in treating his patients. From this last we may judge that his "system," if not doing much good, was at least doing little harm.

The theory of the Vitalists was closely allied to that of the Animists, and its most important representative, Paul Joseph Barthez, was a cultured and eager scientist. After an eventful and varied career as physician, soldier, editor, lawyer, and philosopher in turn, he finally returned to the field of medicine, was made consulting physician by Napoleon in 1802, and died in Paris four years later.

The theory that he championed was based on the assumption that there was a "vital principle," the nature of which was unknown, but which differed from the thinking mind, and was the cause of the phenomena of life. This "vital principle" differed from the soul, and was not exhibited in human beings alone, but even in animals and plants. This force, or whatever it might be called, was supposed to be present everywhere in the body, and all diseases were the results of it.

The theory of the Organicists, like that of the Animists and Vitalists, agreed with the other two that vital activity could not be explained by the laws of physics and chemistry, but, unlike them, it held that it was a part of the structure of the body itself. Naturally the practical physicians were more attracted by this tangible doctrine than by vague theories "which converted diseases into unknown derangements of some equally unknown 'principle.' "

It is perhaps straining a point to include this brief description of these three schools of medicine in the history of the progress of the science. But, on the whole, they were negatively at least prominent factors in directing true progress along its proper channel, showing what courses were not to be pursued. Some one has said that science usually stumbles into the right course only after stumbling into all the wrong ones; and if this be only partially true, the wrong ones still play a prominent if not a very creditable part. Thus the medical systems of William Cullen (1710-1790), and John Brown (1735-1788), while doing little towards the actual advancement of scientific medicine, played so conspicuous a part in so wide a field that the "Brunonian system" at least must be given some little attention.

According to Brown's theory, life, diseases, and methods of cure are explained by the property of "excitability." All exciting powers were supposed to be stimulating, the apparent debilitating effects of some being due to a deficiency in the amount of stimulus. Thus "the whole phenomena of life, health, as well as disease, were supposed to consist of stimulus and nothing else." This theory created a great stir in the medical world, and partisans and opponents sprang up everywhere. In Italy it was enthusiastically supported; in England it was strongly opposed; while in Scotland riots took place between the opposing factions. Just why this system should have created any stir, either for or against it, is not now apparent.

Like so many of the other "theorists" of his century, Brown's practical conclusions deduced from his theory (or perhaps in spite of it) were generally beneficial to medicine, and some of them extremely valuable in the treatment of diseases. He first advocated the modern stimulant, or

"feeding treatment" of fevers, and first recognized the usefulness of animal soups and beef-tea in certain diseases.

THE SYSTEM OF HAHNEMANN

Just at the close of the century there came into prominence the school of homoeopathy, which was destined to influence the practice of medicine very materially and to outlive all the other eighteenth-century schools. It was founded by Christian Samuel Friedrich Hahnemann (1755-1843), a most remarkable man, who, after propounding a theory in his younger days which was at least as reasonable as most of the existing theories, had the misfortune to outlive his usefulness and lay his doctrine open to ridicule by the unreasonable teachings of his dotage,

Hahnemann rejected all the teachings of morbid anatomy and pathology as useless in practice, and propounded his famous "similia similibus curantur"--that all diseases were to be cured by medicine which in health produced symptoms dynamically similar to the disease under treatment. If a certain medicine produced a headache when given to a healthy person, then this medicine was indicated in case of headaches, etc. At the present time such a theory seems crude enough, but in the latter part of the eighteenth century almost any theory was as good as the ones propounded by Animists, Vitalists, and other such schools. It certainly had the very commendable feature of introducing simplicity in the use of drugs in place of the complicated prescriptions then in vogue. Had Hahnemann stopped at this point he could not have been held up to the indefensible ridicule that was brought upon him, with considerable justice, by his later theories. But he lived onto propound his extraordinary theory of "potentiality"--that medicines gained strength by being diluted--and his even more extraordinary theory that all chronic diseases are caused either by the itch, syphilis, or fig-wart disease, or are brought on by medicines.

At the time that his theory of potentialities was promulgated, the medical world had gone mad in its administration of huge doses of compound mixtures of drugs, and any reaction against this was surely an improvement. In short, no medicine at all was much better than the heaping doses used in common practice; and hence one advantage, at least, of Hahnemann's methods. Stated briefly, his theory was that if a tincture be reduced to one-fiftieth in strength, and this again reduced to one-fiftieth, and this process repeated up to thirty such dilutions, the potency of such a medicine will be increased by each dilution, Hahnemann himself preferring the weakest, or, as he would call it, the strongest dilution. The absurdity of such a theory is apparent when it is understood that long before any drug has been raised to its thirtieth dilution it has been so reduced in quantity that it cannot be weighed, measured, or recognized as being present in the solution at all by any means known to chemists. It is but just to modern followers of homoeopathy to say that while most of them advocate small dosage, they do not necessarily follow the teachings of Hahnemann in this respect, believing that the theory of the dose "has nothing more to do with the original law of cure than the psora (itch) theory has; and that it was one of the later creations of Hahnemann's mind."

Hahnemann's theory that all chronic diseases are derived from either itch, syphilis, or fig-wart disease is no longer advocated by his followers, because it is so easily disproved, particularly in the case of itch. Hahnemann taught that fully three-quarters of all diseases were caused by "itch struck

in," and yet it had been demonstrated long before his day, and can be demonstrated any time, that itch is simply a local skin disease caused by a small parasite.

JENNER AND VACCINATION

All advances in science have a bearing, near or remote, on the welfare of our race; but it remains to credit to the closing decade of the eighteenth century a discovery which, in its power of direct and immediate benefit to humanity, surpasses any other discovery of this or any previous epoch. Needless to say, I refer to Jenner's discovery of the method of preventing smallpox by inoculation with the virus of cow-pox. It detracts nothing from the merit of this discovery to say that the preventive power of accidental inoculation had long been rumored among the peasantry of England. Such vague, unavailing half-knowledge is often the forerunner of fruitful discovery.

To all intents and purposes Jenner's discovery was original and unique. Nor, considered as a perfect method, was it in any sense an accident. It was a triumph of experimental science. The discoverer was no novice in scientific investigation, but a trained observer, who had served a long apprenticeship in scientific observation under no less a scientist than the celebrated John Hunter. At the age of twenty-one Jenner had gone to London to pursue his medical studies, and soon after he proved himself so worthy a pupil that for two years he remained a member of Hunter's household as his favorite pupil. His taste for science and natural history soon attracted the attention of Sir Joseph Banks, who intrusted him with the preparation of the zoological specimens brought back by Captain Cook's expedition in 1771. He performed this task so well that he was offered the position of naturalist to the second expedition, but declined it, preferring to take up the practice of his profession in his native town of Berkeley.

His many accomplishments and genial personality soon made him a favorite both as a physician and in society. He was a good singer, a fair violinist and flute-player, and a very successful writer of prose and verse. But with all his professional and social duties he still kept up his scientific investigations, among other things making some careful observations on the hibernation of hedgehogs at the instigation of Hunter, the results of which were laid before the Royal Society. He also made quite extensive investigations as to the geological formations and fossils found in his neighborhood.

Even during his student days with Hunter he had been much interested in the belief, current in the rural districts of Gloucestershire, of the antagonism between cow-pox and small-pox, a person having suffered from cow-pox being immuned to small-pox. At various times Jenner had mentioned the subject to Hunter, and he was constantly making inquiries of his fellow-practitioners as to their observations and opinions on the subject. Hunter was too fully engrossed in other pursuits to give the matter much serious attention, however, and Jenner's brothers of the profession gave scant credence to the rumors, although such rumors were common enough.

At this time the practice of inoculation for preventing small-pox, or rather averting the severer forms of the disease, was widely practised. It was customary, when there was a mild case of the disease, to take some of the virus from the patient and inoculate persons who had never had the disease, producing a similar attack in them. Unfortunately there were many objections to this

practice. The inoculated patient frequently developed a virulent form of the disease and died; or if he recovered, even after a mild attack, he was likely to be "pitted" and disfigured. But, perhaps worst of all, a patient so inoculated became the source of infection to others, and it sometimes happened that disastrous epidemics were thus brought about. The case was a most perplexing one, for the awful scourge of small-pox hung perpetually over the head of every person who had not already suffered and recovered from it. The practice of inoculation was introduced into England by Lady Mary Wortley Montague (1690-1762), who had seen it practised in the East, and who announced her intention of "introducing it into England in spite of the doctors."

From the fact that certain persons, usually milkmaids, who had suffered from cow-pox seemed to be immuned to small-pox, it would seem a very simple process of deduction to discover that cow-pox inoculation was the solution of the problem of preventing the disease. But there was another form of disease which, while closely resembling cow-pox and quite generally confounded with it, did not produce immunity. The confusion of these two forms of the disease had constantly misled investigations as to the possibility of either of them immunizing against smallpox, and the confusion of these two diseases for a time led Jenner to question the possibility of doing so. After careful investigations, however, he reached the conclusion that there was a difference in the effects of the two diseases, only one of which produced immunity from small-pox.

"There is a disease to which the horse, from his state of domestication, is frequently subject," wrote Jenner, in his famous paper on vaccination. "The farriers call it the grease. It is an inflammation and swelling in the heel, accompanied at its commencement with small cracks or fissures, from which issues a limpid fluid possessing properties of a very peculiar kind. This fluid seems capable of generating a disease in the human body (after it has undergone the modification I shall presently speak of) which bears so strong a resemblance to small-pox that I think it highly probable it may be the source of that disease.

"In this dairy country a great number of cows are kept, and the office of milking is performed indiscriminately by men and maid servants. One of the former having been appointed to apply dressings to the heels of a horse affected with the malady I have mentioned, and not paying due attention to cleanliness, incautiously bears his part in milking the cows with some particles of the infectious matter adhering to his fingers. When this is the case it frequently happens that a disease is communicated to the cows, and from the cows to the dairy-maids, which spreads through the farm until most of the cattle and domestics feel its unpleasant consequences. This disease has obtained the name of Cow-Pox. It appears on the nipples of the cows in the form of irregular pustules. At their first appearance they are commonly of a palish blue, or rather of a color somewhat approaching to livid, and are surrounded by an inflammation. These pustules, unless a timely remedy be applied, frequently degenerate into phagedenic ulcers, which prove extremely troublesome. The animals become indisposed, and the secretion of milk is much lessened. Inflamed spots now begin to appear on different parts of the hands of the domestics employed in milking, and sometimes on the wrists, which run on to suppuration, first assuming the appearance of the small vesications produced by a burn. Most commonly they appear about the joints of the fingers and at their extremities; but whatever parts are affected, if the situation will admit the superficial suppurations put on a circular form with their edges more elevated than their centre and of a color distinctly approaching to blue. Absorption takes place, and tumors appear in each axilla. The system becomes affected, the pulse is quickened; shiverings, succeeded by heat, general

lassitude, and pains about the loins and limbs, with vomiting, come on. The head is painful, and the patient is now and then even affected with delirium. These symptoms, varying in their degrees of violence, generally continue from one day to three or four, leaving ulcerated sores about the hands which, from the sensibility of the parts, are very troublesome and commonly heal slowly, frequently becoming phagedenic, like those from which they sprang. During the progress of the disease the lips, nostrils, eyelids, and other parts of the body are sometimes affected with sores; but these evidently arise from their being heedlessly rubbed or scratched by the patient's infected fingers. No eruptions on the skin have followed the decline of the feverish symptoms in any instance that has come under my inspection, one only excepted, and in this case a very few appeared on the arms: they were very minute, of a vivid red color, and soon died away without advancing to maturation, so that I cannot determine whether they had any connection with the preceding symptoms.

"Thus the disease makes its progress from the horse (as I conceive) to the nipple of the cow, and from the cow to the human subject.

"Morbid matter of various kinds, when absorbed into the system, may produce effects in some degree similar; but what renders the cow-pox virus so extremely singular is that the person that has been thus affected is forever after secure from the infection of small-pox, neither exposure to the variolous effluvia nor the insertion of the matter into the skin producing this distemper."[2]

In 1796 Jenner made his first inoculation with cowpox matter, and two months later the same subject was inoculated with small-pox matter. But, as Jenner had predicted, no attack of small-pox followed. Although fully convinced by this experiment that the case was conclusively proven, he continued his investigations, waiting two years before publishing his discovery. Then, fortified by indisputable proofs, he gave it to the world. The immediate effects of his announcement have probably never been equalled in the history of scientific discovery, unless, perhaps, in the single instance of the discovery of anaesthesia. In Geneva and Holland clergymen advocated the practice of vaccination from their pulpits; in some of the Latin countries religious processions were formed for receiving vaccination; Jenner's birthday was celebrated as a feast in Germany; and the first child vaccinated in Russia was named "Vaccinov" and educated at public expense. In six years the discovery had penetrated to the most remote corners of civilization; it had even reached some savage nations. And in a few years small-pox had fallen from the position of the most dreaded of all diseases to that of being practically the only disease for which a sure and easy preventive was known.

Honors were showered upon Jenner from the Old and the New World, and even Napoleon, the bitter hater of the English, was among the others who honored his name. On one occasion Jenner applied to the Emperor for the release of certain Englishmen detained in France. The petition was about to be rejected when the name of the petitioner was mentioned. "Ah," said Napoleon, "we can refuse nothing to that name!"

It is difficult for us of to-day clearly to conceive the greatness of Jenner's triumph, for we can only vaguely realize what a ruthless and ever-present scourge smallpox had been to all previous generations of men since history began. Despite all efforts to check it by medication and by direct inoculation, it swept now and then over the earth as an all-devastating pestilence, and year by year

it claimed one-tenth of all the beings in Christendom by death as its average quota of victims. "From small-pox and love but few remain free," ran the old saw. A pitted face was almost as much a matter of course a hundred years ago as a smooth one is to-day.

Little wonder, then, that the world gave eager acceptance to Jenner's discovery. No urging was needed to induce the majority to give it trial; passengers on a burning ship do not hold aloof from the life-boats. Rich and poor, high and low, sought succor in vaccination and blessed the name of their deliverer. Of all the great names that were before the world in the closing days of the century, there was perhaps no other one at once so widely known and so uniformly reverenced as that of the great English physician Edward Jenner. Surely there was no other one that should be recalled with greater gratitude by posterity.

VIII. NINETEENTH-CENTURY MEDICINE

PHYSICAL DIAGNOSIS

Although Napoleon Bonaparte, First Consul, was not lacking in self-appreciation, he probably did not realize that in selecting a physician for his own needs he was markedly influencing the progress of medical science as a whole. Yet so strangely are cause and effect adjusted in human affairs that this simple act of the First Consul had that very unexpected effect. For the man chosen was the envoy of a new method in medical practice, and the fame which came to him through being physician to the First Consul, and subsequently to the Emperor, enabled him to promulgate the method in a way otherwise impracticable. Hence the indirect but telling value to medical science of Napoleon's selection.

The physician in question was Jean Nicolas de Corvisart. His novel method was nothing more startling than the now-familiar procedure of tapping the chest of a patient to elicit sounds indicative of diseased tissues within. Every one has seen this done commonly enough in our day, but at the beginning of the century Corvisart, and perhaps some of his pupils, were probably the only physicians in the world who resorted to this simple and useful procedure. Hence Napoleon's surprise when, on calling in Corvisart, after becoming somewhat dissatisfied with his other physicians Pinel and Portal, his physical condition was interrogated in this strange manner. With characteristic shrewdness Bonaparte saw the utility of the method, and the physician who thus attempted to substitute scientific method for guess-work in the diagnosis of disease at once found favor in his eyes and was installed as his regular medical adviser.

For fifteen years before this Corvisart had practised percussion, as the chest-tapping method is called, without succeeding in convincing the profession of its value. The method itself, it should be added, had not originated with Corvisart, nor did the French physician for a moment claim it as his own. The true originator of the practice was the German physician Avenbrugger, who published a book about it as early as 1761. This book had even been translated into French, then the language of international communication everywhere, by Roziere de la Chassagne, of Montpellier, in 1770;

but no one other than Corvisart appears to have paid any attention to either original or translation. It was far otherwise, however, when Corvisart translated Avenbrugger's work anew, with important additions of his own, in 1808.

"I know very well how little reputation is allotted to translator and commentators," writes Corvisart, "and I might easily have elevated myself to the rank of an author if I had elaborated anew the doctrine of Avenbrugger and published an independent work on percussion. In this way, however, I should have sacrificed the name of Avenbrugger to my own vanity, a thing which I am unwilling to do. It is he, and the beautiful invention which of right belongs to him, that I desire to recall to life."[1]

By this time a reaction had set in against the metaphysical methods in medicine that had previously been so alluring; the scientific spirit of the time was making itself felt in medical practice; and this, combined with Corvisart's fame, brought the method of percussion into immediate and well-deserved popularity. Thus was laid the foundation for the method of so-called physical diagnosis, which is one of the corner-stones of modern medicine.

The method of physical diagnosis as practised in our day was by no means completed, however, with the work of Corvisart. Percussion alone tells much less than half the story that may be elicited from the organs of the chest by proper interrogation. The remainder of the story can only be learned by applying the ear itself to the chest, directly or indirectly. Simple as this seems, no one thought of practising it for some years after Corvisart had shown the value of percussion.

Then, in 1815, another Paris physician, Rene Theophile Hyacinthe Laennec, discovered, almost by accident, that the sound of the heart-beat could be heard surprisingly through a cylinder of paper held to the ear and against the patient's chest. Acting on the hint thus received, Laennec substituted a hollow cylinder of wood for the paper, and found himself provided with an instrument through which not merely heart sounds but murmurs of the lungs in respiration could be heard with almost startling distinctness.

The possibility of associating the varying chest sounds with diseased conditions of the organs within appealed to the fertile mind of Laennec as opening new vistas in therapeutics, which he determined to enter to the fullest extent practicable. His connection with the hospitals of Paris gave him full opportunity in this direction, and his labors of the next few years served not merely to establish the value of the new method as an aid to diagnosis, but laid the foundation also for the science of morbid anatomy. In 1819 Laennec published the results of his labors in a work called Traite d'Auscultation Mediate,[2] a work which forms one of the landmarks of scientific medicine. By mediate auscultation is meant, of course, the interrogation of the chest with the aid of the little instrument already referred to, an instrument which its originator thought hardly worth naming until various barbarous appellations were applied to it by others, after which Laennec decided to call it the stethoscope, a name which it has ever since retained.

In subsequent years the form of the stethoscope, as usually employed, was modified and its value augmented by a binauricular attachment, and in very recent years a further improvement has been made through application of the principle of the telephone; but the essentials of auscultation with the stethoscope were established in much detail by Laennec, and the honor must always be his of

thus taking one of the longest single steps by which practical medicine has in our century acquired the right to be considered a rational science. Laennec's efforts cost him his life, for he died in 1826 of a lung disease acquired in the course of his hospital practice; but even before this his fame was universal, and the value of his method had been recognized all over the world. Not long after, in 1828, yet another French physician, Piorry, perfected the method of percussion by introducing the custom of tapping, not the chest directly, but the finger or a small metal or hard-rubber plate held against the chest-mediate percussion, in short. This perfected the methods of physical diagnosis of diseases of the chest in all essentials; and from that day till this percussion and auscultation have held an unquestioned place in the regular armamentarium of the physician.

Coupled with the new method of physical diagnosis in the effort to substitute knowledge for guess-work came the studies of the experimental physiologists--in particular, Marshall Hall in England and Francois Magendie in France; and the joint efforts of these various workers led presently to the abandonment of those severe and often irrational depletive methods--blood-letting and the like--that had previously dominated medical practice. To this end also the "statistical method," introduced by Louis and his followers, largely contributed; and by the close of the first third of our century the idea was gaining ground that the province of therapeutics is to aid nature in combating disease, and that this may often be accomplished better by simple means than by the heroic measures hitherto thought necessary. In a word, scientific empiricism was beginning to gain a hearing in medicine as against the metaphysical preconceptions of the earlier generations.

PARASITIC DISEASES

I have just adverted to the fact that Napoleon Bonaparte, as First Consul and as Emperor, was the victim of a malady which caused him to seek the advice of the most distinguished physicians of Paris. It is a little shocking to modern sensibilities to read that these physicians, except Corvisart, diagnosed the distinguished patient's malady as "gale repercutee"--that is to say, in idiomatic English, the itch "struck in." It is hardly necessary to say that no physician of today would make so inconsiderate a diagnosis in the case of a royal patient. If by any chance a distinguished patient were afflicted with the itch, the sagacious physician would carefully hide the fact behind circumlocutions and proceed to eradicate the disease with all despatch. That the physicians of Napoleon did otherwise is evidence that at the beginning of the century the disease in question enjoyed a very different status. At that time itch, instead of being a most plebeian malady, was, so to say, a court disease. It enjoyed a circulation, in high circles and in low, that modern therapeutics has quite denied it; and the physicians of the time gave it a fictitious added importance by ascribing to its influence the existence of almost any obscure malady that came under their observation. Long after Napoleon's time gale continued to hold this proud distinction. For example, the imaginative Dr. Hahnemann did not hesitate to affirm, as a positive maxim, that three-fourths of all the ills that flesh is heir to were in reality nothing but various forms of "gale repercutee."

All of which goes to show how easy it may be for a masked pretender to impose on credulous humanity, for nothing is more clearly established in modern knowledge than the fact that "gale repercutee" was simply a name to hide a profound ignorance; no such disease exists or ever did exist. Gale itself is a sufficiently tangible reality, to be sure, but it is a purely local disease of the skin, due to a perfectly definite cause, and the dire internal conditions formerly ascribed to it have

really no causal connection with it whatever. This definite cause, as every one nowadays knows, is nothing more or less than a microscopic insect which has found lodgment on the skin, and has burrowed and made itself at home there. Kill that insect and the disease is no more; hence it has come to be an axiom with the modern physician that the itch is one of the three or four diseases that he positively is able to cure, and that very speedily. But it was far otherwise with the physicians of the first third of our century, because to them the cause of the disease was an absolute mystery.

It is true that here and there a physician had claimed to find an insect lodged in the skin of a sufferer from itch, and two or three times the claim had been made that this was the cause of the malady, but such views were quite ignored by the general profession, and in 1833 it was stated in an authoritative medical treatise that the "cause of gale is absolutely unknown." But even at this time, as it curiously happened, there were certain ignorant laymen who had attained to a bit of medical knowledge that was withheld from the inner circles of the profession. As the peasantry of England before Jenner had known of the curative value of cow-pox over small-pox, so the peasant women of Poland had learned that the annoying skin disease from which they suffered was caused by an almost invisible insect, and, furthermore, had acquired the trick of dislodging the pestiferous little creature with the point of a needle. From them a youth of the country, F. Renucci by name, learned the open secret. He conveyed it to Paris when he went there to study medicine, and in 1834 demonstrated it to his master Alibert. This physician, at first sceptical, soon was convinced, and gave out the discovery to the medical world with an authority that led to early acceptance.

Now the importance of all this, in the present connection, is not at all that it gave the clew to the method of cure of a single disease. What makes the discovery epochal is the fact that it dropped a brand-new idea into the medical ranks--an idea destined, in the long-run, to prove itself a veritable bomb--the idea, namely, that a minute and quite unsuspected animal parasite may be the cause of a well-known, widely prevalent, and important human disease. Of course the full force of this idea could only be appreciated in the light of later knowledge; but even at the time of its coming it sufficed to give a great impetus to that new medical knowledge, based on microscopical studies, which had but recently been made accessible by the inventions of the lens-makers. The new knowledge clarified one very turbid medical pool and pointed the way to the clarification of many others.

Almost at the same time that the Polish medical student was demonstrating the itch mite in Paris, it chanced, curiously enough, that another medical student, this time an Englishman, made an analogous discovery of perhaps even greater importance. Indeed, this English discovery in its initial stages slightly antedated the other, for it was in 1833 that the student in question, James Paget, interne in St. Bartholomew's Hospital, London, while dissecting the muscular tissues of a human subject, found little specks of extraneous matter, which, when taken to the professor of comparative anatomy, Richard Owen, were ascertained, with the aid of the microscope, to be the cocoon of a minute and hitherto unknown insect. Owen named the insect Trichina spiralis. After the discovery was published it transpired that similar specks had been observed by several earlier investigators, but no one had previously suspected or, at any rate, demonstrated their nature. Nor was the full story of the trichina made out for a long time after Owen's discovery. It was not till 1847 that the American anatomist Dr. Joseph Leidy found the cysts of trichina in the tissues of pork; and another decade or so elapsed after that before German workers, chief among whom were Leuckart, Virchow, and Zenker, proved that the parasite gets into the human system through

ingestion of infected pork, and that it causes a definite set of symptoms of disease which hitherto had been mistaken for rheumatism, typhoid fever, and other maladies. Then the medical world was agog for a time over the subject of trichinosis; government inspection of pork was established in some parts of Germany; American pork was excluded altogether from France; and the whole subject thus came prominently to public attention. But important as the trichina parasite proved on its own account in the end, its greatest importance, after all, was in the share it played in directing attention at the time of its discovery in 1833 to the subject of microscopic parasites in general.

The decade that followed that discovery was a time of great activity in the study of microscopic organisms and microscopic tissues, and such men as Ehrenberg and Henle and Bory Saint-Vincent and Kolliker and Rokitansky and Remak and Dujardin were widening the bounds of knowledge of this new subject with details that cannot be more than referred to here. But the crowning achievement of the period in this direction was the discovery made by the German, J. L. Schoenlein, in 1839, that a very common and most distressing disease of the scalp, known as favus, is really due to the presence and growth on the scalp of a vegetable organism of microscopic size. Thus it was made clear that not merely animal but also vegetable organisms of obscure, microscopic species have causal relations to the diseases with which mankind is afflicted. This knowledge of the parasites was another long step in the direction of scientific medical knowledge; but the heights to which this knowledge led were not to be scaled, or even recognized, until another generation of workers had entered the field.

PAINLESS SURGERY

Meantime, in quite another field of medicine, events were developing which led presently to a revelation of greater immediate importance to humanity than any other discovery that had come in the century, perhaps in any field of science whatever. This was the discovery of the pain-dispelling power of the vapor of sulphuric ether inhaled by a patient undergoing a surgical operation. This discovery came solely out of America, and it stands curiously isolated, since apparently no minds in any other country were trending towards it even vaguely. Davy, in England, had indeed originated the method of medication by inhalation, and earned out some most interesting experiments fifty years earlier, and it was doubtless his experiments with nitrous oxide gas that gave the clew to one of the American investigators; but this was the sole contribution of preceding generations to the subject, and since the beginning of the century, when Davy turned his attention to other matters, no one had made the slightest advance along the same line until an American dentist renewed the investigation.

In view of the sequel, Davy's experiments merit full attention. Here is his own account of them, as written in 1799:

"Immediately after a journey of one hundred and twenty-six miles, in which I had no sleep the preceding night, being much exhausted, I respired seven quarts of nitrous oxide gas for near three minutes. It produced the usual pleasurable effects and slight muscular motion. I continued exhilarated for some minutes afterwards, but in half an hour found myself neither more nor less exhausted than before the experiment. I had a great propensity to sleep.

"To ascertain with certainty whether the more extensive action of nitrous oxide compatible with life was capable of producing debility, I resolved to breathe the gas for such a time, and in such quantities, as to produce excitement equal in duration and superior in intensity to that occasioned by high intoxication from opium or alcohol.

"To habituate myself to the excitement, and to carry it on gradually, on December 26th I was enclosed in an air-tight breathing-box, of the capacity of about nine and one-half cubic feet, in the presence of Dr. Kinglake. After I had taken a situation in which I could by means of a curved thermometer inserted under the arm, and a stop-watch, ascertain the alterations in my pulse and animal heat, twenty quarts of nitrous oxide were thrown into the box.

"For three minutes I experienced no alteration in my sensations, though immediately after the introduction of the nitrous oxide the smell and taste of it were very evident. In four minutes I began to feel a slight glow in the cheeks and a generally diffused warmth over the chest, though the temperature of the box was not quite 50 degrees. . . . In twenty-five minutes the animal heat was 100 degrees, pulse 124. In thirty minutes twenty quarts more of gas were introduced.

"My sensations were now pleasant; I had a generally diffused warmth without the slightest moisture of the skin, a sense of exhilaration similar to that produced by a small dose of wine, and a disposition to muscular motion and to merriment.

"In three-quarters of an hour the pulse was 104 and the animal heat not 99.5 degrees, the temperature of the chamber 64 degrees. The pleasurable feelings continued to increase, the pulse became fuller and slower, till in about an hour it was 88, when the animal heat was 99 degrees. Twenty quarts more of air were admitted. I had now a great disposition to laugh, luminous points seemed frequently to pass before my eyes, my hearing was certainly more acute, and I felt a pleasant lightness and power of exertion in my muscles. In a short time the symptoms became stationary; breathing was rather oppressed, and on account of the great desire for action rest was painful.

"I now came out of the box, having been in precisely an hour and a quarter. The moment after I began to respire twenty quarts of unmingled nitrous oxide. A thrilling extending from the chest to the extremities was almost immediately produced. I felt a sense of tangible extension highly pleasurable in every limb; my visible impressions were dazzling and apparently magnified, I heard distinctly every sound in the room, and was perfectly aware of my situation. By degrees, as the pleasurable sensations increased, I lost all connection with external things; trains of vivid visible images rapidly passed through my mind and were connected with words in such a manner as to produce perceptions perfectly novel.

"I existed in a world of newly connected and newly modified ideas. I theorized; I imagined that I made discoveries. When I was awakened from this semi-delirious trance by Dr. Kinglake, who took the bag from my mouth, indignation and pride were the first feelings produced by the sight of persons about me. My emotions were enthusiastic and sublime; and for a minute I walked about the room perfectly regardless of what was said to me. As I recovered my former state of mind, I felt an inclination to communicate the discoveries I had made during the experiment. I endeavored to recall the ideas--they were feeble and indistinct; one collection of terms, however, presented itself,

and, with most intense belief and prophetic manner, I exclaimed to Dr. Kinglake, 'Nothing exists but thoughts!--the universe is composed of impressions, ideas, pleasures, and pains.' "[3]

From this account we see that Davy has anaesthetized himself to a point where consciousness of surroundings was lost, but not past the stage of exhilaration. Had Dr. Kinglake allowed the inhaling-bag to remain in Davy's mouth for a few moments longer complete insensibility would have followed. As it was, Davy appears to have realized that sensibility was dulled, for he adds this illuminative suggestion: "As nitrous oxide in its extensive operation appears capable of destroying physical pain, it may probably be used with advantage during surgical operations in which no great effusion of blood takes place."[4]

Unfortunately no one took advantage of this suggestion at the time, and Davy himself became interested in other fields of science and never returned to his physiological studies, thus barely missing one of the greatest discoveries in the entire field of science. In the generation that followed no one seems to have thought of putting Davy's suggestion to the test, and the surgeons of Europe had acknowledged with one accord that all hope of finding a means to render operations painless must be utterly abandoned--that the surgeon's knife must ever remain a synonym for slow and indescribable torture. By an odd coincidence it chanced that Sir Benjamin Brodie, the acknowledged leader of English surgeons, had publicly expressed this as his deliberate though regretted opinion at a time when the quest which he considered futile had already led to the most brilliant success in America, and while the announcement of the discovery, which then had no transatlantic cable to convey it, was actually on its way to the Old World.

The American dentist just referred to, who was, with one exception to be noted presently, the first man in the world to conceive that the administration of a definite drug might render a surgical operation painless and to give the belief application was Dr. Horace Wells, of Hartford, Connecticut. The drug with which he experimented was nitrous oxide--the same that Davy had used; the operation that he rendered painless was no more important than the extraction of a tooth--yet it sufficed to mark a principle; the year of the experiment was 1844.

The experiments of Dr. Wells, however, though important, were not sufficiently demonstrative to bring the matter prominently to the attention of the medical world. The drug with which he experimented proved not always reliable, and he himself seems ultimately to have given the matter up, or at least to have relaxed his efforts. But meantime a friend, to whom he had communicated his belief and expectations, took the matter up, and with unremitting zeal carried forward experiments that were destined to lead to more tangible results. This friend was another dentist, Dr. W. T. G. Morton, of Boston, then a young man full of youthful energy and enthusiasm. He seems to have felt that the drug with which Wells had experimented was not the most practicable one for the purpose, and so for several months he experimented with other allied drugs, until finally he hit upon sulphuric ether, and with this was able to make experiments upon animals, and then upon patients in the dental chair, that seemed to him absolutely demonstrative.

Full of eager enthusiasm, and absolutely confident of his results, he at once went to Dr. J. C. Warren, one of the foremost surgeons of Boston, and asked permission to test his discovery decisively on one of the patients at the Boston Hospital during a severe operation. The request was granted; the test was made on October 16, 1846, in the presence of several of the foremost surgeons

of the city and of a body of medical students. The patient slept quietly while the surgeon's knife was plied, and awoke to astonished comprehension that the ordeal was over. The impossible, the miraculous, had been accomplished.[5]

Swiftly as steam could carry it--slowly enough we should think it to-day--the news was heralded to all the world. It was received in Europe with incredulity, which vanished before repeated experiments. Surgeons were loath to believe that ether, a drug that had long held a place in the subordinate armamentarium of the physician, could accomplish such a miracle. But scepticism vanished before the tests which any surgeon might make, and which surgeons all over the world did make within the next few weeks. Then there came a lingering outcry from a few surgeons, notably some of the Parisians, that the shock of pain was beneficial to the patient, hence that anaesthesia--as Dr. Oliver Wendell Holmes had christened the new method--was a procedure not to be advised. Then, too, there came a hue-and-cry from many a pulpit that pain was God-given, and hence, on moral grounds, to be clung to rather than renounced. But the outcry of the antediluvians of both hospital and pulpit quickly received its quietus; for soon it was clear that the patient who did not suffer the shock of pain during an operation rallied better than the one who did so suffer, while all humanity outside the pulpit cried shame to the spirit that would doom mankind to suffer needless agony. And so within a few months after that initial operation at the Boston Hospital in 1846, ether had made good its conquest of pain throughout the civilized world. Only by the most active use of the imagination can we of this present day realize the full meaning of that victory.

It remains to be added that in the subsequent bickerings over the discovery--such bickerings as follow every great advance--two other names came into prominent notice as sharers in the glory of the new method. Both these were Americans--the one, Dr. Charles T. Jackson, of Boston; the other, Dr. Crawford W. Long, of Alabama. As to Dr. Jackson, it is sufficient to say that he seems to have had some vague inkling of the peculiar properties of ether before Morton's discovery. He even suggested the use of this drug to Morton, not knowing that Morton had already tried it; but this is the full measure of his association with the discovery. Hence it is clear that Jackson's claim to equal share with Morton in the discovery was unwarranted, not to say absurd.

Dr. Long's association with the matter was far different and altogether honorable. By one of those coincidences so common in the history of discovery, he was experimenting with ether as a pain-destroyer simultaneously with Morton, though neither so much as knew of the existence of the other. While a medical student he had once inhaled ether for the intoxicant effects, as other medical students were wont to do, and when partially under influence of the drug he had noticed that a chance blow to his shins was painless. This gave him the idea that ether might be used in surgical operations; and in subsequent years, in the course of his practice in a small Georgia town, he put the idea into successful execution. There appears to be no doubt whatever that he performed successful minor operations under ether some two or three years before Morton's final demonstration; hence that the merit of first using the drug, or indeed any drug, in this way belongs to him. But, unfortunately, Dr. Long did not quite trust the evidence of his own experiments. Just at that time the medical journals were full of accounts of experiments in which painless operations were said to be performed through practice of hypnotism, and Dr. Long feared that his own success might be due to an incidental hypnotic influence rather than to the drug. Hence he delayed announcing his apparent discovery until he should have opportunity for further tests--and opportunities did not come every day to the country practitioner. And while he waited, Morton

anticipated him, and the discovery was made known to the world without his aid. It was a true scientific caution that actuated Dr. Long to this delay, but the caution cost him the credit, which might otherwise have been his, of giving to the world one of the greatest blessings--dare we not, perhaps, say the very greatest?--that science has ever conferred upon humanity.

A few months after the use of ether became general, the Scotch surgeon Sir J. Y. Simpson[6] discovered that another drug, chloroform, could be administered with similar effects; that it would, indeed, in many cases produce anaesthesia more advantageously even than ether. From that day till this surgeons have been more or less divided in opinion as to the relative merits of the two drugs; but this fact, of course, has no bearing whatever upon the merit of the first discovery of the method of anaesthesia. Even had some other drug subsequently quite banished ether, the honor of the discovery of the beneficent method of anaesthesia would have been in no wise invalidated. And despite all cavillings, it is unequivocally established that the man who gave that method to the world was William T. G. Morton.

PASTEUR AND THE GERM THEORY OF DISEASE

The discovery of the anaesthetic power of drugs was destined presently, in addition to its direct beneficences, to aid greatly in the progress of scientific medicine, by facilitating those experimental studies of animals from which, before the day of anaesthesia, many humane physicians were withheld, and which in recent years have led to discoveries of such inestimable value to humanity. But for the moment this possibility was quite overshadowed by the direct benefits of anaesthesia, and the long strides that were taken in scientific medicine during the first fifteen years after Morton's discovery were mainly independent of such aid. These steps were taken, indeed, in a field that at first glance might seem to have a very slight connection with medicine. Moreover, the chief worker in the field was not himself a physician. He was a chemist, and the work in which he was now engaged was the study of alcoholic fermentation in vinous liquors. Yet these studies paved the way for the most important advances that medicine has made in any century towards the plane of true science; and to this man more than to any other single individual--it might almost be said more than to all other individuals--was due this wonderful advance. It is almost superfluous to add that the name of this marvellous chemist was Louis Pasteur.

The studies of fermentation which Pasteur entered upon in 1854 were aimed at the solution of a controversy that had been waging in the scientific world with varying degrees of activity for a quarter of a century. Back in the thirties, in the day of the early enthusiasm over the perfected microscope, there had arisen a new interest in the minute forms of life which Leeuwenhoek and some of the other early workers with the lens had first described, and which now were shown to be of almost universal prevalence. These minute organisms had been studied more or less by a host of observers, but in particular by the Frenchman Cagniard Latour and the German of cell-theory fame, Theodor Schwann. These men, working independently, had reached the conclusion, about 1837, that the micro-organisms play a vastly more important role in the economy of nature than any one previously had supposed. They held, for example, that the minute specks which largely make up the substance of yeast are living vegetable organisms, and that the growth of these organisms is the cause of the important and familiar process of fermentation. They even came to hold, at least

tentatively, the opinion that the somewhat similar micro-organisms to be found in all putrefying matter, animal or vegetable, had a causal relation to the process of putrefaction.

This view, particularly as to the nature of putrefaction, was expressed even more outspokenly a little later by the French botanist Turpin. Views so supported naturally gained a following; it was equally natural that so radical an innovation should be antagonized. In this case it chanced that one of the most dominating scientific minds of the time, that of Liebig, took a firm and aggressive stand against the new doctrine. In 1839 he promulgated his famous doctrine of fermentation, in which he stood out firmly against any "vitalistic" explanation of the phenomena, alleging that the presence of micro-organisms in fermenting and putrefying substances was merely incidental, and in no sense causal. This opinion of the great German chemist was in a measure substantiated by experiments of his compatriot Helmholtz, whose earlier experiments confirmed, but later ones contradicted, the observations of Schwann, and this combined authority gave the vitalistic conception a blow from which it had not rallied at the time when Pasteur entered the field. Indeed, it was currently regarded as settled that the early students of the subject had vastly over-estimated the importance of micro-organisms.

And so it came as a new revelation to the generality of scientists of the time, when, in 1857 and the succeeding half-decade, Pasteur published the results of his researches, in which the question had been put to a series of altogether new tests, and brought to unequivocal demonstration.

He proved that the micro-organisms do all that his most imaginative predecessors had suspected, and more. Without them, he proved, there would be no fermentation, no putrefaction--no decay of any tissues, except by the slow process of oxidation. It is the microscopic yeast-plant which, by seizing on certain atoms of the molecule, liberates the remaining atoms in the form of carbonic-acid and alcohol, thus effecting fermentation; it is another microscopic plant--a bacterium, as Devaine had christened it--which in a similar way effects the destruction of organic molecules, producing the condition which we call putrefaction. Pasteur showed, to the amazement of biologists, that there are certain forms of these bacteria which secure the oxygen which all organic life requires, not from the air, but by breaking up unstable molecules in which oxygen is combined; that putrefaction, in short, has its foundation in the activities of these so-called anaerobic bacteria.

In a word, Pasteur showed that all the many familiar processes of the decay of organic tissues are, in effect, forms of fermentation, and would not take place at all except for the presence of the living micro-organisms. A piece of meat, for example, suspended in an atmosphere free from germs, will dry up gradually, without the slightest sign of putrefaction, regardless of the temperature or other conditions to which it may have been subjected. Let us witness one or two series of these experiments as presented by Pasteur himself in one of his numerous papers before the Academy of Sciences.

EXPERIMENTS WITH GRAPE SUGAR

"In the course of the discussion which took place before the Academy upon the subject of the generation of ferments properly so-called, there was a good deal said about that of wine, the oldest

fermentation known. On this account I decided to disprove the theory of M. Fremy by a decisive experiment bearing solely upon the juice of grapes.

"I prepared forty flasks of a capacity of from two hundred and fifty to three hundred cubic centimetres and filled them half full with filtered grape-must, perfectly clear, and which, as is the case of all acidulated liquids that have been boiled for a few seconds, remains uncontaminated although the curved neck of the flask containing them remain constantly open during several months or years.

"In a small quantity of water I washed a part of a bunch of grapes, the grapes and the stalks together, and the stalks separately. This washing was easily done by means of a small badger's-hair brush. The washing-water collected the dust upon the surface of the grapes and the stalks, and it was easily shown under the microscope that this water held in suspension a multitude of minute organisms closely resembling either fungoid spores, or those of alcoholic Yeast, or those of Mycoderma vini, etc. This being done, ten of the forty flasks were preserved for reference; in ten of the remainder, through the straight tube attached to each, some drops of the washing-water were introduced; in a third series of ten flasks a few drops of the same liquid were placed after it had been boiled; and, finally, in the ten remaining flasks were placed some drops of grape-juice taken from the inside of a perfect fruit. In order to carry out this experiment, the straight tube of each flask was drawn out into a fine and firm point in the lamp, and then curved. This fine and closed point was filed round near the end and inserted into the grape while resting upon some hard substance. When the point was felt to touch the support of the grape it was by a slight pressure broken off at the point file mark. Then, if care had been taken to create a slight vacuum in the flask, a drop of the juice of the grape got into it, the filed point was withdrawn, and the aperture immediately closed in the alcohol lamp. This decreased pressure of the atmosphere in the flask was obtained by the following means: After warming the sides of the flask either in the hands or in the lamp-flame, thus causing a small quantity of air to be driven out of the end of the curved neck, this end was closed in the lamp. After the flask was cooled, there was a tendency to suck in the drop of grape-juice in the manner just described.

"The drop of grape-juice which enters into the flask by this suction ordinarily remains in the curved part of the tube, so that to mix it with the must it was necessary to incline the flask so as to bring the must into contact with the juice and then replace the flask in its normal position. The four series of comparative experiments produced the following results:

"The first ten flasks containing the grape-must boiled in pure air did not show the production of any organism. The grape-must could possibly remain in them for an indefinite number of years. Those in the second series, containing the water in which the grapes had been washed separately and together, showed without exception an alcoholic fermentation which in several cases began to appear at the end of forty-eight hours when the experiment took place at ordinary summer temperature. At the same time that the yeast appeared, in the form of white traces, which little by little united themselves in the form of a deposit on the sides of all the flasks, there were seen to form little flakes of Mycellium, often as a single fungoid growth or in combination, these fungoid growths being quite independent of the must or of any alcoholic yeast. Often, also, the Mycoderma vini appeared after some days upon the surface of the liquid. The Vibria and the lactic ferments properly so called did not appear on account of the nature of the liquid.

"The third series of flasks, the washing-water in which had been previously boiled, remained unchanged, as in the first series. Those of the fourth series, in which was the juice of the interior of the grapes, remained equally free from change, although I was not always able, on account of the delicacy of the experiment, to eliminate every chance of error. These experiments cannot leave the least doubt in the mind as to the following facts:

Grape-must, after heating, never ferments on contact with the air, when the air has been deprived of the germs which it ordinarily holds in a state of suspension.

"The boiled grape-must ferments when there is introduced into it a very small quantity of water in which the surface of the grapes or their stalks have been washed.

"The grape-must does not ferment when this washing-water has been boiled and afterwards cooled.

"The grape-must does not ferment when there is added to it a small quantity of the juice of the inside of the grape.

"The yeast, therefore, which causes the fermentation of the grapes in the vintage-tub comes from the outside and not from the inside of the grapes. Thus is destroyed the hypothesis of MM. Trecol and Fremy, who surmised that the albuminous matter transformed itself into yeast on account of the vital germs which were natural to it. With greater reason, therefore, there is no longer any question of the theory of Liebig of the transformation of albuminoid matter into ferments on account of the oxidation."

FOREIGN ORGANISMS AND THE WORT OF BEER

"The method which I have just followed," Pasteur continues, "in order to show that there exists a correlation between the diseases of beer and certain microscopic organisms leaves no room for doubt, it seems to me, in regard to the principles I am expounding.

"Every time that the microscope reveals in the leaven, and especially in the active yeast, the production of organisms foreign to the alcoholic yeast properly so called, the flavor of the beer leaves something to be desired, much or little, according to the abundance and the character of these little germs. Moreover, when a finished beer of good quality loses after a time its agreeable flavor and becomes sour, it can be easily shown that the alcoholic yeast deposited in the bottles or the casks, although originally pure, at least in appearance, is found to be contaminated gradually with these filiform or other ferments. All this can be deduced from the facts already given, but some critics may perhaps declare that these foreign ferments are the consequences of the diseased condition, itself produced by unknown causes.

"Although this gratuitous hypothesis may be difficult to uphold, I will endeavor to corroborate the preceding observations by a clearer method of investigation. This consists in showing that the beer never has any unpleasant taste in all cases when the alcoholic ferment properly so called is not mixed with foreign ferments; that it is the same in the case of wort, and that wort, liable to changes

as it is, can be preserved unaltered if it is kept from those microscopic parasites which find in it a suitable nourishment and a field for growth.

"The employment of this second method has, moreover, the advantage of proving with certainty the proposition that I advanced at first--namely, that the germs of these organisms are derived from the dust of the atmosphere, carried about and deposited upon all objects, or scattered over the utensils and the materials used in a brewery-materials naturally charged with microscopic germs, and which the various operations in the store-rooms and the malt-house may multiply indefinitely.

"Let us take a glass flask with a long neck of from two hundred and fifty to three hundred cubic centimetres capacity, and place in it some wort, with or without hops, and then in the flame of a lamp draw out the neck of the flask to a fine point, afterwards heating the liquid until the steam comes out of the end of the neck. It can then be allowed to cool without any other precautions; but for additional safety there can be introduced into the little point a small wad of asbestos at the moment that the flame is withdrawn from beneath the flask. Before thus placing the asbestos it also can be passed through the flame, as well as after it has been put into the end of the tube. The air which then first re-enters the flask will thus come into contact with the heated glass and the heated liquid, so as to destroy the vitality of any dust germs that may exist in the air. The air itself will re-enter very gradually, and slowly enough to enable any dust to be taken up by the drop of water which the air forces up the curvature of the tube. Ultimately the tube will be dry, but the re-entering of the air will be so slow that the particles of dust will fall upon the sides of the tube. The experiments show that with this kind of vessel, allowing free communication with the air, and the dust not being allowed to enter, the dust will not enter at all events for a period of ten or twelve years, which has been the longest period devoted to these trials; and the liquid, if it were naturally limpid, will not be in the least polluted neither on its surface nor in its mass, although the outside of the flask may become thickly coated with dust. This is a most irrefutable proof of the impossibility of dust getting inside the flask.

"The wort thus prepared remains uncontaminated indefinitely, in spite of its susceptibility to change when exposed to the air under conditions which allow it to gather the dusty particles which float in the atmosphere. It is the same in the case of urine, beef-tea, and grape-must, and generally with all those putrefactable and fermentable liquids which have the property when heated to boiling-point of destroying the vitality of dust germs."[7]

There was nothing in these studies bearing directly upon the question of animal diseases, yet before they were finished they had stimulated progress in more than one field of pathology. At the very outset they sufficed to start afresh the inquiry as to the role played by micro-organisms in disease. In particular they led the French physician Devaine to return to some interrupted studies which he had made ten years before in reference to the animal disease called anthrax, or splenic fever, a disease that cost the farmers of Europe millions of francs annually through loss of sheep and cattle. In 1850 Devaine had seen multitudes of bacteria in the blood of animals who had died of anthrax, but he did not at that time think of them as having a causal relation to the disease. Now, however, in 1863, stimulated by Pasteur's new revelations regarding the power of bacteria, he returned to the subject, and soon became convinced, through experiments by means of inoculation, that the microscopic organisms he had discovered were the veritable and the sole cause of the infectious disease anthrax.

The publication of this belief in 1863 aroused a furor of controversy. That a microscopic vegetable could cause a virulent systemic disease was an idea altogether too startling to be accepted in a day, and the generality of biologists and physicians demanded more convincing proofs than Devaine as yet was able to offer.

Naturally a host of other investigators all over the world entered the field. Foremost among these was the German Dr. Robert Koch, who soon corroborated all that Devaine had observed, and carried the experiments further in the direction of the cultivation of successive generations of the bacteria in artificial media, inoculations being made from such pure cultures of the eighth generation, with the astonishing result that animals thus inoculated succumbed to the disease.

Such experiments seem demonstrative, yet the world was unconvinced, and in 1876, while the controversy was still at its height, Pasteur was prevailed upon to take the matter in hand. The great chemist was becoming more and more exclusively a biologist as the years passed, and in recent years his famous studies of the silk-worm diseases, which he proved due to bacterial infection, and of the question of spontaneous generation, had given him unequalled resources in microscopical technique. And so when, with the aid of his laboratory associates Duclaux and Chamberland and Roux, he took up the mooted anthrax question the scientific world awaited the issue with bated breath. And when, in 1877, Pasteur was ready to report on his studies of anthrax, he came forward with such a wealth of demonstrative experiments--experiments the rigid accuracy of which no one would for a moment think of questioning--going to prove the bacterial origin of anthrax, that scepticism was at last quieted for all time to come.

Henceforth no one could doubt that the contagious disease anthrax is due exclusively to the introduction into an animal's system of a specific germ--a microscopic plant--which develops there. And no logical mind could have a reasonable doubt that what is proved true of one infectious disease would some day be proved true also of other, perhaps of all, forms of infectious maladies.

Hitherto the cause of contagion, by which certain maladies spread from individual to individual, had been a total mystery, quite unillumined by the vague terms "miasm," "humor," "virus," and the like cloaks of ignorance. Here and there a prophet of science, as Schwann and Henle, had guessed the secret; but guessing, in science, is far enough from knowing. Now, for the first time, the world KNEW, and medicine had taken another gigantic stride towards the heights of exact science.

LISTER AND ANTISEPTIC SURGERY

Meantime, in a different though allied field of medicine there had been a complementary growth that led to immediate results of even more practical importance. I mean the theory and practice of antisepsis in surgery. This advance, like the other, came as a direct outgrowth of Pasteur's fermentation studies of alcoholic beverages, though not at the hands of Pasteur himself. Struck by the boundless implications of Pasteur's revelations regarding the bacteria, Dr. Joseph Lister (the present Lord Lister), then of Glasgow, set about as early as 1860 to make a wonderful application of these ideas. If putrefaction is always due to bacterial development, he argued, this must apply as well to living as to dead tissues; hence the putrefactive changes which occur in wounds and after

operations on the human subject, from which blood-poisoning so often follows, might be absolutely prevented if the injured surfaces could be kept free from access of the germs of decay.

In the hope of accomplishing this result, Lister began experimenting with drugs that might kill the bacteria without injury to the patient, and with means to prevent further access of germs once a wound was freed from them. How well he succeeded all the world knows; how bitterly he was antagonized for about a score of years, most of the world has already forgotten. As early as 1867 Lister was able to publish results pointing towards success in his great project; yet so incredulous were surgeons in general that even some years later the leading surgeons on the Continent had not so much as heard of his efforts. In 1870 the soldiers of Paris died, as of old, of hospital gangrene; and when, in 1871, the French surgeon Alphonse Guerin, stimulated by Pasteur's studies, conceived the idea of dressing wounds with cotton in the hope of keeping germs from entering them, he was quite unaware that a British contemporary had preceded him by a full decade in this effort at prevention and had made long strides towards complete success. Lister's priority, however, and the superiority of his method, were freely admitted by the French Academy of Sciences, which in 1881 officially crowned his achievement, as the Royal Society of London had done the year before.

By this time, to be sure, as everybody knows, Lister's new methods had made their way everywhere, revolutionizing the practice of surgery and practically banishing from the earth maladies that hitherto had been the terror of the surgeon and the opprobrium of his art. And these bedside studies, conducted in the end by thousands of men who had no knowledge of microscopy, had a large share in establishing the general belief in the causal relation that micro-organisms bear to disease, which by about the year 1880 had taken possession of the medical world. But they did more; they brought into equal prominence the idea that, the cause of a diseased condition being known, it maybe possible as never before to grapple with and eradicate that condition.

PREVENTIVE INOCULATION

The controversy over spontaneous generation, which, thanks to Pasteur and Tyndall, had just been brought to a termination, made it clear that no bacterium need be feared where an antecedent bacterium had not found lodgment; Listerism in surgery had now shown how much might be accomplished towards preventing the access of germs to abraded surfaces of the body and destroying those that already had found lodgment there. As yet, however, there was no inkling of a way in which a corresponding onslaught might be made upon those other germs which find their way into the animal organism by way of the mouth and the nostrils, and which, as was now clear, are the cause of those contagious diseases which, first and last, claim so large a proportion of mankind for their victims. How such means might be found now became the anxious thought of every imaginative physician, of every working microbiologist.

As it happened, the world was not kept long in suspense. Almost before the proposition had taken shape in the minds of the other leaders, Pasteur had found a solution. Guided by the empirical success of Jenner, he, like many others, had long practised inoculation experiments, and on February 9, 1880, he announced to the French Academy of Sciences that he had found a method of so reducing the virulence of a disease germ that when introduced into the system of a susceptible animal it produced only a mild form of the disease, which, however, sufficed to protect against the

usual virulent form exactly as vaccinia protects against small-pox. The particular disease experimented with was that infectious malady of poultry known familiarly as "chicken cholera." In October of the same year Pasteur announced the method by which this "attenuation of the virus," as he termed it, had been brought about--by cultivation of the disease germs in artificial media, exposed to the air, and he did not hesitate to assert his belief that the method would prove "susceptible of generalization"--that is to say, of application to other diseases than the particular one in question.

Within a few months he made good this prophecy, for in February, 1881, he announced to the Academy that with the aid, as before, of his associates MM. Chamberland and Roux, he had produced an attenuated virus of the anthrax microbe by the use of which, as he affirmed with great confidence, he could protect sheep, and presumably cattle, against that fatal malady. "In some recent publications," said Pasteur, "I announced the first case of the attenuation of a virus by experimental methods only. Formed of a special microbe of an extreme minuteness, this virus may be multiplied by artificial culture outside the animal body. These cultures, left alone without any possible external contamination, undergo, in the course of time, modifications of their virulency to a greater or less extent. The oxygen of the atmosphere is said to be the chief cause of these attenuations--that is, this lessening of the facilities of multiplication of the microbe; for it is evident that the difference of virulence is in some way associated with differences of development in the parasitic economy.

"There is no need to insist upon the interesting character of these results and the deductions to be made therefrom. To seek to lessen the virulence by rational means would be to establish, upon an experimental basis, the hope of preparing from an active virus, easily cultivated either in the human or animal body, a vaccine-virus of restrained development capable of preventing the fatal effects of the former. Therefore, we have applied all our energies to investigate the possible generalizing action of atmospheric oxygen in the attenuation of virus.

"The anthrax virus, being one that has been most carefully studied, seemed to be the first that should attract our attention. Every time, however, we encountered a difficulty. Between the microbe of chicken cholera and the microbe of anthrax there exists an essential difference which does not allow the new experiment to be verified by the old. The microbes of chicken cholera do not, in effect, seem to resolve themselves, in their culture, into veritable germs. The latter are merely cells, or articulations always ready to multiply by division, except when the particular conditions in which they become true germs are known.

"The yeast of beer is a striking example of these cellular productions, being able to multiply themselves indefinitely without the apparition of their original spores. There exist many mucedines (Mucedinae?) of tubular mushrooms, which in certain conditions of culture produce a chain of more or less spherical cells called Conidae. The latter, detached from their branches, are able to reproduce themselves in the form of cells, without the appearance, at least with a change in the conditions of culture, of the spores of their respective mucedines. These vegetable organisms can be compared to plants which are cultivated by slipping, and to produce which it is not necessary to have the fruits or the seeds of the mother plant.

The anthrax bacterium, in its artificial cultivation, behaves very differently. Its mycelian filaments, if one may so describe them, have been produced scarcely for twenty-four or forty-eight hours when they are seen to transform themselves, those especially which are in free contact with the air, into very refringent corpuscles, capable of gradually isolating themselves into true germs of slight organization. Moreover, observation shows that these germs, formed so quickly in the culture, do not undergo, after exposure for a time to atmospheric air, any change either in their vitality or their virulence. I was able to present to the Academy a tube containing some spores of anthrax bacteria produced four years ago, on March 21, 1887. Each year the germination of these little corpuscles has been tried, and each year the germination has been accomplished with the same facility and the same rapidity as at first. Each year also the virulence of the new cultures has been tested, and they have not shown any visible falling off. Therefore, how can we experiment with the action of the air upon the anthrax virus with any expectation of making it less virulent?

"The crucial difficulty lies perhaps entirely in this rapid reproduction of the bacteria germs which we have just related. In its form of a filament, and in its multiplication by division, is not this organism at all points comparable with the microbe of the chicken cholera?

"That a germ, properly so called, that a seed, does not suffer any modification on account of the air is easily conceived; but it is conceivable not less easily that if there should be any change it would occur by preference in the case of a mycelian fragment. It is thus that a slip which may have been abandoned in the soil in contact with the air does not take long to lose all vitality, while under similar conditions a seed is preserved in readiness to reproduce the plant. If these views have any foundation, we are led to think that in order to prove the action of the air upon the anthrax bacteria it will be indispensable to submit to this action the mycelian development of the minute organism under conditions where there cannot be the least admixture of corpuscular germs. Hence the problem of submitting the bacteria to the action of oxygen comes back to the question of presenting entirely the formation of spores. The question being put in this way, we are beginning to recognize that it is capable of being solved.

"We can, in fact, prevent the appearance of spores in the artificial cultures of the anthrax parasite by various artifices. At the lowest temperature at which this parasite can be cultivated--that is to say, about +16 degrees Centigrade--the bacterium does not produce germs--at any rate, for a very long time. The shapes of the minute microbe at this lowest limit of its development are irregular, in the form of balls and pears--in a word, they are monstrosities--but they are without spores. In the last regard also it is the same at the highest temperatures at which the parasite can be cultivated, temperatures which vary slightly according to the means employed. In neutral chicken bouillon the bacteria cannot be cultivated above 45 degrees. Culture, however, is easy and abundant at 42 to 43 degrees, but equally without any formation of spores. Consequently a culture of mycelian bacteria can be kept entirely free from germs while in contact with the open air at a temperature of from 42 to 43 degrees Centigrade. Now appear the three remarkable results. After about one month of waiting the culture dies--that is to say, if put into a fresh bouillon it becomes absolutely sterile.

"So much for the life and nutrition of this organism. In respect to its virulence, it is an extraordinary fact that it disappears entirely after eight days' culture at 42 to 43 degrees Centigrade, or, at any rate, the cultures are innocuous for the guinea-pig, the rabbit, and the sheep, the three kinds of animals most apt to contract anthrax. We are thus able to obtain, not only the attenuation of the

virulence, but also its complete suppression by a simple method of cultivation. Moreover, we see also the possibility of preserving and cultivating the terrible microbe in an inoffensive state. What is it that happens in these eight days at 43 degrees that suffices to take away the virulence of the bacteria? Let us remember that the microbe of chicken cholera dies in contact with the air, in a period somewhat protracted, it is true, but after successive attenuations. Are we justified in thinking that it ought to be the same in regard to the microbe of anthrax? This hypothesis is confirmed by experiment. Before the disappearance of its virulence the anthrax microbe passes through various degrees of attenuation, and, moreover, as is also the case with the microbe of chicken cholera, each of these attenuated states of virulence can be obtained by cultivation. Moreover, since, according to one of our recent Communications, anthrax is not recurrent, each of our attenuated anthrax microbes is, for the better-developed microbe, a vaccine--that is to say, a virus producing a less-malignant malady. What, therefore, is easier than to find in these a virus that will infect with anthrax sheep, cows, and horses, without killing them, and ultimately capable of warding off the mortal malady? We have practised this experiment with great success upon sheep, and when the season comes for the assembling of the flocks at Beauce we shall try the experiment on a larger scale.

"Already M. Toussaint has announced that sheep can be saved by preventive inoculations; but when this able observer shall have published his results; on the subject of which we have made such exhaustive studies, as yet unpublished, we shall be able to see the whole difference which exists between the two methods--the uncertainty of the one and the certainty of the other. That which we announce has, moreover, the very great advantage of resting upon the existence of a poison vaccine cultivable at will, and which can be increased indefinitely in the space of a few hours without having recourse to infected blood."[8]

This announcement was immediately challenged in a way that brought it to the attention of the entire world. The president of an agricultural society, realizing the enormous importance of the subject, proposed to Pasteur that his alleged discovery should be submitted to a decisive public test. He proposed to furnish a drove of fifty sheep half of which were to be inoculated with the attenuated virus of Pasteur. Subsequently all the sheep were to be inoculated with virulent virus, all being kept together in one pen under precisely the same conditions. The "protected" sheep were to remain healthy; the unprotected ones to die of anthrax; so read the terms of the proposition. Pasteur accepted the challenge; he even permitted a change in the programme by which two goats were substituted for two of the sheep, and ten cattle added, stipulating, however, that since his experiments had not yet been extended to cattle these should not be regarded as falling rigidly within the terms of the test.

It was a test to try the soul of any man, for all the world looked on askance, prepared to deride the maker of so preposterous a claim as soon as his claim should be proved baseless. Not even the fame of Pasteur could make the public at large, lay or scientific, believe in the possibility of what he proposed to accomplish. There was time for all the world to be informed of the procedure, for the first "preventive" inoculation--or vaccination, as Pasteur termed it--was made on May 5th, the second on May 17th, and another interval of two weeks must elapse before the final inoculations with the unattenuated virus. Twenty-four sheep, one goat, and five cattle were submitted to the preliminary vaccinations. Then, on May 31 st, all sixty of the animals were inoculated, a protected and unprotected one alternately, with an extremely virulent culture of anthrax microbes that had

been in Pasteur's laboratory since 1877. This accomplished, the animals were left together in one enclosure to await the issue.

Two days later, June 2d, at the appointed hour of rendezvous, a vast crowd, composed of veterinary surgeons, newspaper correspondents, and farmers from far and near, gathered to witness the closing scenes of this scientific tourney. What they saw was one of the most dramatic scenes in the history of peaceful science--a scene which, as Pasteur declared afterwards, "amazed the assembly." Scattered about the enclosure, dead, dying, or manifestly sick unto death, lay the unprotected animals, one and all, while each and every "protected" animal stalked unconcernedly about with every appearance of perfect health. Twenty of the sheep and the one goat were already dead; two other sheep expired under the eyes of the spectators; the remaining victims lingered but a few hours longer. Thus in a manner theatrical enough, not to say tragic, was proclaimed the unequivocal victory of science. Naturally enough, the unbelievers struck their colors and surrendered without terms; the principle of protective vaccination, with a virus experimentally prepared in the laboratory, was established beyond the reach of controversy.

That memorable scientific battle marked the beginning of a new era in medicine. It was a foregone conclusion that the principle thus established would be still further generalized; that it would be applied to human maladies; that in all probability it would grapple successfully, sooner or later, with many infectious diseases. That expectation has advanced rapidly towards realization. Pasteur himself made the application to the human subject in the disease hydrophobia in 1885, since which time that hitherto most fatal of maladies has largely lost its terrors. Thousands of persons bitten by mad dogs have been snatched from the fatal consequences of that mishap by this method at the Pasteur Institute in Paris, and at the similar institutes, built on the model of this parent one, that have been established all over the world in regions as widely separated as New York and Nha-Trang.

SERUM-THERAPY

In the production of the rabies vaccine Pasteur and his associates developed a method of attenuation of a virus quite different from that which had been employed in the case of the vaccines of chicken cholera and of anthrax. The rabies virus was inoculated into the system of guinea-pigs or rabbits and, in effect, cultivated in the systems of these animals. The spinal cord of these infected animals was found to be rich in the virus, which rapidly became attenuated when the cord was dried in the air. The preventive virus, of varying strengths, was made by maceration of these cords at varying stages of desiccation. This cultivation of a virus within the animal organism suggested, no doubt, by the familiar Jennerian method of securing small-pox vaccine, was at the same time a step in the direction of a new therapeutic procedure which was destined presently to become of all-absorbing importance--the method, namely, of so-called serum-therapy, or the treatment of a disease with the blood serum of an animal that has been subjected to protective inoculation against that disease.

The possibility of such a method was suggested by the familiar observation, made by Pasteur and numerous other workers, that animals of different species differ widely in their susceptibility to various maladies, and that the virus of a given disease may become more and more virulent when

passed through the systems of successive individuals of one species, and, contrariwise, less and less virulent when passed through the systems of successive individuals of another species. These facts suggested the theory that the blood of resistant animals might contain something directly antagonistic to the virus, and the hope that this something might be transferred with curative effect to the blood of an infected susceptible animal. Numerous experimenters all over the world made investigations along the line of this alluring possibility, the leaders perhaps being Drs. Behring and Kitasato, closely followed by Dr. Roux and his associates of the Pasteur Institute of Paris. Definite results were announced by Behring in 1892 regarding two important diseases--tetanus and diphtheria--but the method did not come into general notice until 1894, when Dr. Roux read an epoch-making paper on the subject at the Congress of Hygiene at Buda-Pesth.

In this paper Dr. Roux, after adverting to the labors of Behring, Ehrlich, Boer, Kossel, and Wasserman, described in detail the methods that had been developed at the Pasteur Institute for the development of the curative serum, to which Behring had given the since-familiar name antitoxine. The method consists, first, of the cultivation, for some months, of the diphtheria bacillus (called the Klebs-Loeffler bacillus, in honor of its discoverers) in an artificial bouillon, for the development of a powerful toxine capable of giving the disease in a virulent form.

This toxine, after certain details of mechanical treatment, is injected in small but increasing doses into the system of an animal, care being taken to graduate the amount so that the animal does not succumb to the disease. After a certain course of this treatment it is found that a portion of blood serum of the animal so treated will act in a curative way if injected into the blood of another animal, or a human patient, suffering with diphtheria. In other words, according to theory, an antitoxine has been developed in the system of the animal subjected to the progressive inoculations of the diphtheria toxine. In Dr. Roux's experience the animal best suited for the purpose is the horse, though almost any of the domesticated animals will serve the purpose.

But Dr. Roux's paper did not stop with the description of laboratory methods. It told also of the practical application of the serum to the treatment of numerous cases of diphtheria in the hospitals of Paris--applications that had met with a gratifying measure of success. He made it clear that a means had been found of coping successfully with what had been one of the most virulent and intractable of the diseases of childhood. Hence it was not strange that his paper made a sensation in all circles, medical and lay alike.

Physicians from all over the world flocked to Paris to learn the details of the open secret, and within a few months the new serum-therapy had an acknowledged standing with the medical profession everywhere. What it had accomplished was regarded as but an earnest of what the new method might accomplish presently when applied to the other infectious diseases.

Efforts at such applications were immediately begun in numberless directions--had, indeed, been under way in many a laboratory for some years before. It is too early yet to speak of the results in detail. But enough has been done to show that this method also is susceptible of the widest generalization. It is not easy at the present stage to sift that which is tentative from that which will be permanent; but so great an authority as Behring does not hesitate to affirm that today we possess, in addition to the diphtheria antitoxine, equally specific antitoxines of tetanus, cholera, typhus fever, pneumonia, and tuberculosis--a set of diseases which in the aggregate account for a

startling proportion of the general death-rate. Then it is known that Dr. Yersin, with the collaboration of his former colleagues of the Pasteur Institute, has developed, and has used with success, an antitoxine from the microbe of the plague which recently ravaged China.

Dr. Calmette, another graduate of the Pasteur Institute, has extended the range of the serum-therapy to include the prevention and treatment of poisoning by venoms, and has developed an antitoxine that has already given immunity from the lethal effects of snake bites to thousands of persons in India and Australia.

Just how much of present promise is tentative, just what are the limits of the methods--these are questions for the future to decide. But, in any event, there seems little question that the serum treatment will stand as the culminating achievement in therapeutics of our century. It is the logical outgrowth of those experimental studies with the microscope begun by our predecessors of the thirties, and it represents the present culmination of the rigidly experimental method which has brought medicine from a level of fanciful empiricism to the plane of a rational experimental science.

IX. THE NEW SCIENCE OF EXPERIMENTAL PSYCHOLOGY

BRAIN AND MIND

A little over a hundred years ago a reform movement was afoot in the world in the interests of the insane. As was fitting, the movement showed itself first in America, where these unfortunates were humanely cared for at a time when their treatment elsewhere was worse than brutal; but England and France quickly fell into line. The leader on this side of the water was the famous Philadelphian, Dr. Benjamin Rush, "the Sydenham of America"; in England, Dr. William Tuke inaugurated the movement; and in France, Dr. Philippe Pinel, single-handed, led the way. Moved by a common spirit, though acting quite independently, these men raised a revolt against the traditional custom which, spurning the insane as demon-haunted outcasts, had condemned these unfortunates to dungeons, chains, and the lash. Hitherto few people had thought it other than the natural course of events that the "maniac" should be thrust into a dungeon, and perhaps chained to the wall with the aid of an iron band riveted permanently about his neck or waist. Many an unfortunate, thus manacled, was held to the narrow limits of his chain for years together in a cell to which full daylight never penetrated; sometimes--iron being expensive--the chain was so short that the wretched victim could not rise to the upright posture or even shift his position upon his squalid pallet of straw.

In America, indeed, there being no Middle Age precedents to crystallize into established customs, the treatment accorded the insane had seldom or never sunk to this level. Partly for this reason, perhaps, the work of Dr. Rush at the Philadelphia Hospital, in 1784, by means of which the insane came to be humanely treated, even to the extent of banishing the lash, has been but little noted, while the work of the European leaders, though belonging to later decades, has been made famous.

And perhaps this is not as unjust as it seems, for the step which Rush took, from relatively bad to good, was a far easier one to take than the leap from atrocities to good treatment which the European reformers were obliged to compass. In Paris, for example, Pinel was obliged to ask permission of the authorities even to make the attempt at liberating the insane from their chains, and, notwithstanding his recognized position as a leader of science, he gained but grudging assent, and was regarded as being himself little better than a lunatic for making so manifestly unwise and hopeless an attempt. Once the attempt had been made, however, and carried to a successful issue, the amelioration wrought in the condition of the insane was so patent that the fame of Pinel's work at the Bicetre and the Salpetriere went abroad apace. It required, indeed, many years to complete it in Paris, and a lifetime of effort on the part of Pinel's pupil Esquirol and others to extend the reform to the provinces; but the epochal turning-point had been reached with Pinel's labors of the closing years of the eighteenth century.

The significance of this wise and humane reform, in the present connection, is the fact that these studies of the insane gave emphasis to the novel idea, which by-and-by became accepted as beyond question, that "demoniacal possession" is in reality no more than the outward expression of a diseased condition of the brain. This realization made it clear, as never before, how intimately the mind and the body are linked one to the other. And so it chanced that, in striking the shackles from the insane, Pinel and his confreres struck a blow also, unwittingly, at time-honored philosophical traditions. The liberation of the insane from their dungeons was an augury of the liberation of psychology from the musty recesses of metaphysics. Hitherto psychology, in so far as it existed at all, was but the subjective study of individual minds; in future it must become objective as well, taking into account also the relations which the mind bears to the body, and in particular to the brain and nervous system.

The necessity for this collocation was advocated quite as earnestly, and even more directly, by another worker of this period, whose studies were allied to those of alienists, and who, even more actively than they, focalized his attention upon the brain and its functions. This earliest of specialists in brain studies was a German by birth but Parisian by adoption, Dr. Franz Joseph Gall, originator of the since-notorious system of phrenology. The merited disrepute into which this system has fallen through the exposition of peripatetic charlatans should not make us forget that Dr. Gall himself was apparently a highly educated physician, a careful student of the brain and mind according to the best light of his time, and, withal, an earnest and honest believer in the validity of the system he had originated. The system itself, taken as a whole, was hopelessly faulty, yet it was not without its latent germ of truth, as later studies were to show. How firmly its author himself believed in it is evidenced by the paper which he contributed to the French Academy of Sciences in 1808. The paper itself was referred to a committee of which Pinel and Cuvier were members. The verdict of this committee was adverse, and justly so; yet the system condemned had at least one merit which its detractors failed to realize. It popularized the conception that the brain is the organ of mind. Moreover, by its insistence it rallied about it a band of scientific supporters, chief of whom was Dr. Kaspar Spurzlieim, a man of no mean abilities, who became the propagandist of phrenology in England and in America. Of course such advocacy and popularity stimulated opposition as well, and out of the disputations thus arising there grew presently a general interest in the brain as the organ of mind, quite aside from any preconceptions whatever as to the doctrines of Gall and Spurzheim.

Prominent among the unprejudiced class of workers who now appeared was the brilliant young Frenchman Louis Antoine Desmoulins, who studied first under the tutorage of the famous Magendie, and published jointly with him a classical work on the nervous system of vertebrates in 1825. Desmoulins made at least one discovery of epochal importance. He observed that the brains of persons dying in old age were lighter than the average and gave visible evidence of atrophy, and he reasoned that such decay is a normal accompaniment of senility. No one nowadays would question the accuracy of this observation, but the scientific world was not quite ready for it in 1825; for when Desmoulins announced his discovery to the French Academy, that august and somewhat patriarchal body was moved to quite unscientific wrath, and forbade the young iconoclast the privilege of further hearings. From which it is evident that the partially liberated spirit of the new psychology had by no means freed itself altogether, at the close of the first quarter of the nineteenth century, from the metaphysical cobwebs of its long incarceration.

FUNCTIONS OF THE NERVES

While studies of the brain were thus being inaugurated, the nervous system, which is the channel of communication between the brain and the outside world, was being interrogated with even more tangible results. The inaugural discovery was made in 1811 by Dr. (afterwards Sir Charles) Bell,[1] the famous English surgeon and experimental physiologist. It consisted of the observation that the anterior roots of the spinal nerves are given over to the function of conveying motor impulses from the brain outward, whereas the posterior roots convey solely sensory impulses to the brain from without. Hitherto it had been supposed that all nerves have a similar function, and the peculiar distribution of the spinal nerves had been an unsolved puzzle.

Bell's discovery was epochal; but its full significance was not appreciated for a decade, nor, indeed, was its validity at first admitted. In Paris, in particular, then the court of final appeal in all matters scientific, the alleged discovery was looked at askance, or quite ignored. But in 1823 the subject was taken up by the recognized leader of French physiology--Francois Magendie--in the course of his comprehensive experimental studies of the nervous system, and Bell's conclusions were subjected to the most rigid experimental tests and found altogether valid. Bell himself, meanwhile, had turned his attention to the cranial nerves, and had proved that these also are divisible into two sets--sensory and motor. Sometimes, indeed, the two sets of filaments are combined into one nerve cord, but if traced to their origin these are found to arise from different brain centres. Thus it was clear that a hitherto unrecognized duality of function pertains to the entire extra-cranial nervous system. Any impulse sent from the periphery to the brain must be conveyed along a perfectly definite channel; the response from the brain, sent out to the peripheral muscles, must traverse an equally definite and altogether different course. If either channel is interrupted--as by the section of its particular nerve tract--the corresponding message is denied transmission as effectually as an electric current is stopped by the section of the transmitting wire.

Experimenters everywhere soon confirmed the observations of Bell and Magendie, and, as always happens after a great discovery, a fresh impulse was given to investigations in allied fields. Nevertheless, a full decade elapsed before another discovery of comparable importance was made. Then Marshall Hall, the most famous of English physicians of his day, made his classical observations on the phenomena that henceforth were to be known as reflex action. In 1832, while

experimenting one day with a decapitated newt, he observed that the headless creature's limbs would contract in direct response to certain stimuli. Such a response could no longer be secured if the spinal nerves supplying a part were severed. Hence it was clear that responsive centres exist in the spinal cord capable of receiving a sensory message and of transmitting a motor impulse in reply--a function hitherto supposed to be reserved for the brain. Further studies went to show that such phenomena of reflex action on the part of centres lying outside the range of consciousness, both in the spinal cord and in the brain itself, are extremely common; that, in short, they enter constantly into the activities of every living organism and have a most important share in the sum total of vital movements. Hence, Hall's discovery must always stand as one of the great mile-stones of the advance of neurological science.

Hall gave an admirably clear and interesting account of his experiments and conclusions in a paper before the Royal Society, "On the Reflex Functions of the Medulla Oblongata and the Medulla Spinalis," from which, as published in the Transactions of the society for 1833, we may quote at some length:

"In the entire animal, sensation and voluntary motion, functions of the cerebrum, combine with the functions of the medulla oblongata and medulla spinalis, and may therefore render it difficult or impossible to determine those which are peculiar to each; if, in an animal deprived of the brain, the spinal marrow or the nerves supplying the muscles be stimulated, those muscles, whether voluntary or respiratory, are equally thrown into contraction, and, it may be added, equally in the complete and in the mutilated animal; and, in the case of the nerves, equally in limbs connected with and detached from the spinal marrow.

"The operation of all these various causes may be designated centric, as taking place AT, or at least in a direction FROM, central parts of the nervous system. But there is another function the phenomena of which are of a totally different order and obey totally different laws, being excited by causes in a situation which is EXCENTRIC in the nervous system--that is, distant from the nervous centres. This mode of action has not, I think, been hitherto distinctly understood by physiologists.

"Many of the phenomena of this principle of action, as they occur in the limbs, have certainly been observed. But, in the first place, this function is by no means confined to the limbs; for, while it imparts to each muscle its appropriate tone, and to each system of muscles its appropriate equilibrium or balance, it performs the still more important office of presiding over the orifices and terminations of each of the internal canals in the animal economy, giving them their due form and action; and, in the second place, in the instances in which the phenomena of this function have been noticed, they have been confounded, as I have stated, with those of sensation and volition; or, if they have been distinguished from these, they have been too indefinitely denominated instinctive, or automatic. I have been compelled, therefore, to adopt some new designation for them, and I shall now give the reasons for my choice of that which is given in the title of this paper--'Reflex Functions.'

"This property is characterized by being EXCITED in its action and REFLEX in its course: in every instance in which it is exerted an impression made upon the extremities of certain nerves is

conveyed to the medulla oblongata or the medulla spinalis, and is reflected along the nerves to parts adjacent to, or remote from, that which has received the impression.

"It is by this reflex character that the function to which I have alluded is to be distinguished from every other. There are, in the animal economy, four modes of muscular action, of muscular contraction. The first is that designated VOLUNTARY: volition, originated in the cerebrum and spontaneous in its acts, extends its influence along the spinal marrow and the motor nerves in a DIRECT LINE to the voluntary muscles. The SECOND is that of RESPIRATION: like volition, the motive influence in respiration passes in a DIRECT LINE from one point of the nervous system to certain muscles; but as voluntary motion seems to originate in the cerebrum, so the respiratory motions originate in the medulla oblongata: like the voluntary motions, the motions of respirations are spontaneous; they continue, at least, after the eighth pair of nerves have been divided. The THIRD kind of muscular action in the animal economy is that termed involuntary: it depends upon the principle of irritability and requires the IMMEDIATE application of a stimulus to the nervo-muscular fibre itself. These three kinds of muscular motion are well known to physiologists; and I believe they are all which have been hitherto pointed out. There is, however, a FOURTH, which subsists, in part, after the voluntary and respiratory motions have ceased, by the removal of the cerebrum and medulla oblongata, and which is attached to the medulla spinalis, ceasing itself when this is removed, and leaving the irritability undiminished. In this kind of muscular motion the motive influence does not originate in any central part of the nervous system, but from a distance from that centre; it is neither spontaneous in its action nor direct in its course; it is, on the contrary, EXCITED by the application of appropriate stimuli, which are not, however, applied immediately to the muscular or nervo-muscular fibre, but to certain membraneous parts, whence the impression is carried through the medulla, REFLECTED and reconducted to the part impressed, or conducted to a part remote from it in which muscular contraction is effected.

"The first three modes of muscular action are known only by actual movements of muscular contractions. But the reflex function exists as a continuous muscular action, as a power presiding over organs not actually in a state of motion, preserving in some, as the glottis, an open, in others, as the sphincters, a closed form, and in the limbs a due degree of equilibrium or balanced muscular action--a function not, I think, hitherto recognized by physiologists.

The three kinds of muscular motion hitherto known may be distinguished in another way. The muscles of voluntary motion and of respiration may be excited by stimulating the nerves which supply them, in any part of their course, whether at their source as a part of the medulla oblongata or the medulla spinalis or exterior to the spinal canal: the muscles of involuntary motion are chiefly excited by the actual contact of stimuli. In the case of the reflex function alone the muscles are excited by a stimulus acting mediately and indirectly in a curved and reflex course, along superficial subcutaneous or submucous nerves proceeding from the medulla. The first three of these causes of muscular motion may act on detached limbs or muscles. The last requires the connection with the medulla to be preserved entire.

"All the kinds of muscular motion may be unduly excited, but the reflex function is peculiar in being excitable in two modes of action, not previously subsisting in the animal economy, as in the case of sneezing, coughing, vomiting, etc. The reflex function also admits of being permanently

diminished or augmented and of taking on some other morbid forms, of which I shall treat hereafter.

"Before I proceed to the details of the experiments upon which this disposition rests, it may be well to point out several instances in illustration of the various sources of and the modes of muscular action which have been enumerated. None can be more familiar than the act of swallowing. Yet how complicated is the act! The apprehension of the food by the teeth and tongue, etc., is voluntary, and cannot, therefore, take place in an animal from which the cerebrum is removed. The transition of food over the glottis and along the middle and lower part of the pharynx depends upon the reflex action: it can take place in animals from which the cerebrum has been removed or the ninth pair of nerves divided; but it requires the connection with the medulla oblongata to be preserved entirely; and the actual contact of some substance which may act as a stimulus: it is attended by the accurate closure of the glottis and by the contraction of the pharynx. The completion of the act of deglutition is dependent upon the stimulus immediately impressed upon the muscular fibre of the oesophagus, and is the result of excited irritability.

"However plain these observations may have made the fact that there is a function of the nervous muscular system distinct from sensation, from the voluntary and respiratory motions, and from irritability, it is right, in every such inquiry as the present, that the statements and reasonings should be made with the experiment, as it were, actually before us. It has already been remarked that the voluntary and respiratory motions are spontaneous, not necessarily requiring the agency of a stimulus. If, then, an animal can be placed in such circumstances that such motions will certainly not take place, the power of moving remaining, it may be concluded that volition and the motive influence of respiration are annihilated. Now this is effected by removing the cerebrum and the medulla oblongata. These facts are fully proved by the experiments of Legallois and M. Flourens, and by several which I proceed to detail, for the sake of the opportunity afforded by doing so of stating the arguments most clearly.

"I divided the spinal marrow of a very lively snake between the second and third vertebrae. The movements of the animal were immediately before extremely vigorous and unintermitted. From the moment of the division of the spinal marrow it lay perfectly tranquil and motionless, with the exception of occasional gaspings and slight movements of the head. It became quite evident that this state of quiescence would continue indefinitely were the animal secured from all external impressions.

"Being now stimulated, the body began to move with great activity, and continued to do so for a considerable time, each change of position or situation bringing some fresh part of the surface of the animal into contact with the table or other objects and renewing the application of stimulants.

"At length the animal became again quiescent; and being carefully protected from all external impressions it moved no more, but died in the precise position and form which it had last assumed.

"It requires a little manoeuvre to perform this experiment successfully: the motions of the animal must be watched and slowly and cautiously arrested by opposing some soft substance, as a glove or cotton wool; they are by this means gradually lulled into quiescence. The slightest touch with a hard substance, the slightest stimulus, will, on the other hand, renew the movements on the animal

in an active form. But that this phenomenon does not depend upon sensation is further fully proved by the facts that the position last assumed, and the stimuli, may be such as would be attended by extreme or continued pain, if the sensibility were undestroyed: in one case the animal remained partially suspended over the acute edge of the table; in others the infliction of punctures and the application of a lighted taper did not prevent the animal, still possessed of active powers of motion, from passing into a state of complete and permanent quiescence."

In summing up this long paper Hall concludes with this sentence: "The reflex function appears in a word to be the COMPLEMENT of the functions of the nervous system hitherto known."[2]

All these considerations as to nerve currents and nerve tracts becoming stock knowledge of science, it was natural that interest should become stimulated as to the exact character of these nerve tracts in themselves, and all the more natural in that the perfected microscope was just now claiming all fields for its own. A troop of observers soon entered upon the study of the nerves, and the leader here, as in so many other lines of microscopical research, was no other than Theodor Schwann. Through his efforts, and with the invaluable aid of such other workers as Remak, Purkinje, Henle, Muller, and the rest, all the mystery as to the general characteristics of nerve tracts was cleared away. It came to be known that in its essentials a nerve tract is a tenuous fibre or thread of protoplasm stretching between two terminal points in the organism, one of such termini being usually a cell of the brain or spinal cord, the other a distribution-point at or near the periphery--for example, in a muscle or in the skin. Such a fibril may have about it a protective covering, which is known as the sheath of Schwann; but the fibril itself is the essential nerve tract; and in many cases, as Remak presently discovered, the sheath is dispensed with, particularly in case of the nerves of the so-called sympathetic system.

This sympathetic system of ganglia and nerves, by-the-bye, had long been a puzzle to the physiologists. Its ganglia, the seeming centre of the system, usually minute in size and never very large, are found everywhere through the organism, but in particular are gathered into a long double chain which lies within the body cavity, outside the spinal column, and represents the sole nervous system of the non-vertebrated organisms. Fibrils from these ganglia were seen to join the cranial and spinal nerve fibrils and to accompany them everywhere, but what special function they subserved was long a mere matter of conjecture and led to many absurd speculations. Fact was not substituted for conjecture until about the year 1851, when the great Frenchman Claude Bernard conclusively proved that at least one chief function of the sympathetic fibrils is to cause contraction of the walls of the arterioles of the system, thus regulating the blood-supply of any given part. Ten years earlier Henle had demonstrated the existence of annular bands of muscle fibres in the arterioles, hitherto a much-mooted question, and several tentative explanations of the action of these fibres had been made, particularly by the brothers Weber, by Stilling, who, as early as 1840, had ventured to speak of "vaso-motor" nerves, and by Schiff, who was hard upon the same track at the time of Bernard's discovery. But a clear light was not thrown on the subject until Bernard's experiments were made in 1851. The experiments were soon after confirmed and extended by Brown-Sequard, Waller, Budge, and numerous others, and henceforth physiologists felt that they understood how the blood-supply of any given part is regulated by the nervous system.

In reality, however, they had learned only half the story, as Bernard himself proved only a few years later by opening up a new and quite unsuspected chapter. While experimenting in 1858 he

discovered that there are certain nerves supplying the heart which, if stimulated, cause that organ to relax and cease beating. As the heart is essentially nothing more than an aggregation of muscles, this phenomenon was utterly puzzling and without precedent in the experience of physiologists. An impulse travelling along a motor nerve had been supposed to be able to cause a muscular contraction and to do nothing else; yet here such an impulse had exactly the opposite effect. The only tenable explanation seemed to be that this particular impulse must arrest or inhibit the action of the impulses that ordinarily cause the heart muscles to contract. But the idea of such inhibition of one impulse by another was utterly novel and at first difficult to comprehend. Gradually, however, the idea took its place in the current knowledge of nerve physiology, and in time it came to be understood that what happens in the case of the heart nerve-supply is only a particular case under a very general, indeed universal, form of nervous action. Growing out of Bernard's initial discovery came the final understanding that the entire nervous system is a mechanism of centres subordinate and centres superior, the action of the one of which may be counteracted and annulled in effect by the action of the other. This applies not merely to such physical processes as heart-beats and arterial contraction and relaxing, but to the most intricate functionings which have their counterpart in psychical processes as well. Thus the observation of the inhibition of the heart's action by a nervous impulse furnished the point of departure for studies that led to a better understanding of the modus operandi of the mind's activities than had ever previously been attained by the most subtle of psychologists.

PSYCHO-PHYSICS

The work of the nerve physiologists had thus an important bearing on questions of the mind. But there was another company of workers of this period who made an even more direct assault upon the "citadel of thought." A remarkable school of workers had been developed in Germany, the leaders being men who, having more or less of innate metaphysical bias as a national birthright, had also the instincts of the empirical scientist, and whose educational equipment included a profound knowledge not alone of physiology and psychology, but of physics and mathematics as well. These men undertook the novel task of interrogating the relations of body and mind from the standpoint of physics. They sought to apply the vernier and the balance, as far as might be, to the intangible processes of mind.

The movement had its precursory stages in the early part of the century, notably in the mathematical psychology of Herbart, but its first definite output to attract general attention came from the master-hand of Hermann Helmholtz in 1851. It consisted of the accurate measurement of the speed of transit of a nervous impulse along a nerve tract. To make such measurement had been regarded as impossible, it being supposed that the flight of the nervous impulse was practically instantaneous. But Helmholtz readily demonstrated the contrary, showing that the nerve cord is a relatively sluggish message-bearer. According to his experiments, first performed upon the frog, the nervous "current" travels less than one hundred feet per second. Other experiments performed soon afterwards by Helmholtz himself, and by various followers, chief among whom was Du Bois-Reymond, modified somewhat the exact figures at first obtained, but did not change the general bearings of the early results. Thus the nervous impulse was shown to be something far different, as regards speed of transit, at any rate, from the electric current to which it had been so often likened.

An electric current would flash halfway round the globe while a nervous impulse could travel the length of the human body--from a man's foot to his brain.

The tendency to bridge the gulf that hitherto had separated the physical from the psychical world was further evidenced in the following decade by Helmholtz's remarkable but highly technical study of the sensations of sound and of color in connection with their physical causes, in the course of which he revived the doctrine of color vision which that other great physiologist and physicist, Thomas Young, had advanced half a century before. The same tendency was further evidenced by the appearance, in 1852, of Dr. Hermann Lotze's famous Medizinische Psychologie, oder Physiologie der Seele, with its challenge of the old myth of a "vital force." But the most definite expression of the new movement was signalized in 1860, when Gustav Fechner published his classical work called Psychophysik. That title introduced a new word into the vocabulary of science. Fechner explained it by saying, "I mean by psychophysics an exact theory of the relation between spirit and body, and, in a general way, between the physical and the psychic worlds." The title became famous and the brunt of many a controversy. So also did another phrase which Fechner introduced in the course of his book--the phrase "physiological psychology." In making that happy collocation of words Fechner virtually christened a new science.

FECHNER EXPOUNDS WEBER'S LAW

The chief purport of this classical book of the German psycho-physiologist was the elaboration and explication of experiments based on a method introduced more than twenty years earlier by his countryman E. H. Weber, but which hitherto had failed to attract the attention it deserved. The method consisted of the measurement and analysis of the definite relation existing between external stimuli of varying degrees of intensity (various sounds, for example) and the mental states they induce. Weber's experiments grew out of the familiar observation that the nicety of our discriminations of various sounds, weights, or visual images depends upon the magnitude of each particular cause of a sensation in its relation with other similar causes. Thus, for example, we cannot see the stars in the daytime, though they shine as brightly then as at night. Again, we seldom notice the ticking of a clock in the daytime, though it may become almost painfully audible in the silence of the night. Yet again, the difference between an ounce weight and a two-ounce weight is clearly enough appreciable when we lift the two, but one cannot discriminate in the same way between a five-pound weight and a weight of one ounce over five pounds.

This last example, and similar ones for the other senses, gave Weber the clew to his novel experiments. Reflection upon every-day experiences made it clear to him that whenever we consider two visual sensations, or two auditory sensations, or two sensations of weight, in comparison one with another, there is always a limit to the keenness of our discrimination, and that this degree of keenness varies, as in the case of the weights just cited, with the magnitude of the exciting cause.

Weber determined to see whether these common experiences could be brought within the pale of a general law. His method consisted of making long series of experiments aimed at the determination, in each case, of what came to be spoken of as the least observable difference between the stimuli. Thus if one holds an ounce weight in each hand, and has tiny weights added to

one of them, grain by grain, one does not at first perceive a difference; but presently, on the addition of a certain grain, he does become aware of the difference. Noting now how many grains have been added to produce this effect, we have the weight which represents the least appreciable difference when the standard is one ounce.

Now repeat the experiment, but let the weights be each of five pounds. Clearly in this case we shall be obliged to add not grains, but drachms, before a difference between the two heavy weights is perceived. But whatever the exact amount added, that amount represents the stimulus producing a just-perceivable sensation of difference when the standard is five pounds. And so on for indefinite series of weights of varying magnitudes. Now came Weber's curious discovery. Not only did he find that in repeated experiments with the same pair of weights the measure of "just-{p}erceivable difference" remained approximately fixed, but he found, further, that a remarkable fixed relation exists between the stimuli of different magnitude. If, for example, he had found it necessary, in the case of the ounce weights, to add one-fiftieth of an ounce to the one before a difference was detected, he found also, in the case of the five-pound weights, that one-fiftieth of five pounds must be added before producing the same result. And so of all other weights; the amount added to produce the stimulus of "least-appreciable difference" always bore the same mathematical relation to the magnitude of the weight used, be that magnitude great or small.

Weber found that the same thing holds good for the stimuli of the sensations of sight and of hearing, the differential stimulus bearing always a fixed ratio to the total magnitude of the stimuli. Here, then, was the law he had sought.

Weber's results were definite enough and striking enough, yet they failed to attract any considerable measure of attention until they were revived and extended by Fechner and brought before the world in the famous work on psycho-physics. Then they precipitated a veritable melee. Fechner had not alone verified the earlier results (with certain limitations not essential to the present consideration), but had invented new methods of making similar tests, and had reduced the whole question to mathematical treatment. He pronounced Weber's discovery the fundamental law of psycho-physics. In honor of the discoverer, he christened it Weber's Law. He clothed the law in words and in mathematical formulae, and, so to say, launched it full tilt at the heads of the psychological world. It made a fine commotion, be assured, for it was the first widely heralded bulletin of the new psychology in its march upon the strongholds of the time-honored metaphysics. The accomplishments of the microscopists and the nerve physiologists had been but preliminary--mere border skirmishes of uncertain import. But here was proof that the iconoclastic movement meant to invade the very heart of the sacred territory of mind--a territory from which tangible objective fact had been supposed to be forever barred.

PHYSIOLOGICAL PSYCHOLOGY

Hardly had the alarm been sounded, however, before a new movement was made. While Fechner's book was fresh from the press, steps were being taken to extend the methods of the physicist in yet another way to the intimate processes of the mind. As Helmholtz had shown the rate of nervous impulsion along the nerve tract to be measurable, it was now sought to measure also the time required for the central nervous mechanism to perform its work of receiving a message and sending

out a response. This was coming down to the very threshold of mind. The attempt was first made by Professor Donders in 1861, but definitive results were only obtained after many years of experiment on the part of a host of observers. The chief of these, and the man who has stood in the forefront of the new movement and has been its recognized leader throughout the remainder of the century, is Dr. Wilhelm Wundt, of Leipzig.

The task was not easy, but, in the long run, it was accomplished. Not alone was it shown that the nerve centre requires a measurable time for its operations, but much was learned as to conditions that modify this time. Thus it was found that different persons vary in the rate of their central nervous activity--which explained the "personal equation" that the astronomer Bessel had noted a half-century before. It was found, too, that the rate of activity varies also for the same person under different conditions, becoming retarded, for example, under influence of fatigue, or in case of certain diseases of the brain. All details aside, the essential fact emerges, as an experimental demonstration, that the intellectual processes--sensation, apperception, volition--are linked irrevocably with the activities of the central nervous tissues, and that these activities, like all other physical processes, have a time element. To that old school of psychologists, who scarcely cared more for the human head than for the heels--being interested only in the mind--such a linking of mind and body as was thus demonstrated was naturally disquieting. But whatever the inferences, there was no escaping the facts.

Of course this new movement has not been confined to Germany. Indeed, it had long had exponents elsewhere. Thus in England, a full century earlier, Dr. Hartley had championed the theory of the close and indissoluble dependence of the mind upon the brain, and formulated a famous vibration theory of association that still merits careful consideration. Then, too, in France, at the beginning of the century, there was Dr. Cabanis with his tangible, if crudely phrased, doctrine that the brain digests impressions and secretes thought as the stomach digests food and the liver secretes bile. Moreover, Herbert Spencer's Principles of Psychology, with its avowed co-ordination of mind and body and its vitalizing theory of evolution, appeared in 1855, half a decade before the work of Fechner. But these influences, though of vast educational value, were theoretical rather than demonstrative, and the fact remains that the experimental work which first attempted to gauge mental operations by physical principles was mainly done in Germany. Wundt's Physiological Psychology, with its full preliminary descriptions of the anatomy of the nervous system, gave tangible expression to the growth of the new movement in 1874; and four years later, with the opening of his laboratory of physiological psychology at the University of Leipzig, the new psychology may be said to have gained a permanent foothold and to have forced itself into official recognition. From then on its conquest of the world was but a matter of time.

It should be noted, however, that there is one other method of strictly experimental examination of the mental field, latterly much in vogue, which had a different origin. This is the scientific investigation of the phenomena of hypnotism. This subject was rescued from the hands of charlatans, rechristened, and subjected to accurate investigation by Dr. James Braid, of Manchester, as early as 1841. But his results, after attracting momentary attention, fell from view, and, despite desultory efforts, the subject was not again accorded a general hearing from the scientific world until 1878, when Dr. Charcot took it up at the Salpetriere, in Paris, followed soon afterwards by Dr. Rudolf Heidenhain, of Breslau, and a host of other experimenters. The value of the method in the study of mental states was soon apparent. Most of Braid's experiments were repeated, and in the

main his results were confirmed. His explanation of hypnotism, or artificial somnambulism, as a self-induced state, independent of any occult or supersensible influence, soon gained general credence. His belief that the initial stages are due to fatigue of nervous centres, usually from excessive stimulation, has not been supplanted, though supplemented by notions growing out of the new knowledge as to subconscious mentality in general, and the inhibitory influence of one centre over another in the central nervous mechanism.

THE BRAIN AS THE ORGAN OF MIND

These studies of the psychologists and pathologists bring the relations of mind and body into sharp relief. But even more definite in this regard was the work of the brain physiologists. Chief of these, during the middle period of the century, was the man who is sometimes spoken of as the "father of brain physiology," Marie Jean Pierre Flourens, of the Jardin des Plantes of Paris, the pupil and worthy successor of Magendie. His experiments in nerve physiology were begun in the first quarter of the century, but his local experiments upon the brain itself were not culminated until about 1842. At this time the old dispute over phrenology had broken out afresh, and the studies of Flourens were aimed, in part at least, at the strictly scientific investigation of this troublesome topic.

In the course of these studies Flourens discovered that in the medulla oblongata, the part of the brain which connects that organ with the spinal cord, there is a centre of minute size which cannot be injured in the least without causing the instant death of the animal operated upon. It may be added that it is this spot which is reached by the needle of the garroter in Spanish executions, and that the same centre also is destroyed when a criminal is "successfully" hanged, this time by the forced intrusion of a process of the second cervical vertebra. Flourens named this spot the "vital knot." Its extreme importance, as is now understood, is due to the fact that it is the centre of nerves that supply the heart; but this simple explanation, annulling the conception of a specific "life centre," was not at once apparent.

Other experiments of Flourens seemed to show that the cerebellum is the seat of the centres that co-ordinate muscular activities, and that the higher intellectual faculties are relegated to the cerebrum. But beyond this, as regards localization, experiment faltered. Negative results, as regards specific faculties, were obtained from all localized irritations of the cerebrum, and Flourens was forced to conclude that the cerebral lobe, while being undoubtedly the seat of higher intellection, performs its functions with its entire structure. This conclusion, which incidentally gave a quietus to phrenology, was accepted generally, and became the stock doctrine of cerebral physiology for a generation.

It will be seen, however, that these studies of Flourens had a double bearing. They denied localization of cerebral functions, but they demonstrated the localization of certain nervous processes in other portions of the brain. On the whole, then, they spoke positively for the principle of localization of function in the brain, for which a certain number of students contended; while their evidence against cerebral localization was only negative. There was here and there an observer who felt that this negative testimony was not conclusive. In particular, the German anatomist Meynert, who had studied the disposition of nerve tracts in the cerebrum, was led to

believe that the anterior portions of the cerebrum must have motor functions in preponderance; the posterior positions, sensory functions. Somewhat similar conclusions were reached also by Dr. Hughlings-Jackson, in England, from his studies of epilepsy. But no positive evidence was forthcoming until 1861, when Dr. Paul Broca brought before the Academy of Medicine in Paris a case of brain lesion which he regarded as having most important bearings on the question of cerebral localization.

The case was that of a patient at the Bicetre, who for twenty years had been deprived of the power of speech, seemingly through loss of memory of words. In 1861 this patient died, and an autopsy revealed that a certain convolution of the left frontal lobe of his cerebrum had been totally destroyed by disease, the remainder of his brain being intact. Broca felt that this observation pointed strongly to a localization of the memory of words in a definite area of the brain. Moreover, it transpired that the case was not without precedent. As long ago as 1825 Dr. Boillard had been led, through pathological studies, to locate definitely a centre for the articulation of words in the frontal lobe, and here and there other observers had made tentatives in the same direction. Boillard had even followed the matter up with pertinacity, but the world was not ready to listen to him. Now, however, in the half-decade that followed Broca's announcements, interest rose to fever-beat, and through the efforts of Broca, Boillard, and numerous others it was proved that a veritable centre having a strange domination over the memory of articulate words has its seat in the third convolution of the frontal lobe of the cerebrum, usually in the left hemisphere. That part of the brain has since been known to the English-speaking world as the convolution of Broca, a name which, strangely enough, the discoverer's compatriots have been slow to accept.

This discovery very naturally reopened the entire subject of brain localization. It was but a short step to the inference that there must be other definite centres worth the seeking, and various observers set about searching for them. In 1867 a clew was gained by Eckhard, who, repeating a forgotten experiment by Haller and Zinn of the previous century, removed portions of the brain cortex of animals, with the result of producing convulsions. But the really vital departure was made in 1870 by the German investigators Fritsch and Hitzig, who, by stimulating definite areas of the cortex of animals with a galvanic current, produced contraction of definite sets of muscles of the opposite side of the body. These most important experiments, received at first with incredulity, were repeated and extended in 1873 by Dr. David Ferrier, of London, and soon afterwards by a small army of independent workers everywhere, prominent among whom were Franck and Pitres in France, Munck and Goltz in Germany, and Horsley and Schafer in England. The detailed results, naturally enough, were not at first all in harmony. Some observers, as Goltz, even denied the validity of the conclusions in toto. But a consensus of opinion, based on multitudes of experiments, soon placed the broad general facts for which Fritsch and Hitzig contended beyond controversy. It was found, indeed, that the cerebral centres of motor activities have not quite the finality at first ascribed to them by some observers, since it may often happen that after the destruction of a centre, with attending loss of function, there may be a gradual restoration of the lost function, proving that other centres have acquired the capacity to take the place of the one destroyed. There are limits to this capacity for substitution, however, and with this qualification the definiteness of the localization of motor functions in the cerebral cortex has become an accepted part of brain physiology.

Nor is such localization confined to motor centres. Later experiments, particularly of Ferrier and of Munck, proved that the centres of vision are equally restricted in their location, this time in the posterior lobes of the brain, and that hearing has likewise its local habitation. Indeed, there is every reason to believe that each form of primary sensation is based on impressions which mainly come to a definitely localized goal in the brain. But all this, be it understood, has no reference to the higher forms of intellection. All experiment has proved futile to localize these functions, except indeed to the extent of corroborating the familiar fact of their dependence upon the brain, and, somewhat problematically, upon the anterior lobes of the cerebrum in particular. But this is precisely what should be expected, for the clearer insight into the nature of mental processes makes it plain that in the main these alleged "faculties" are not in themselves localized. Thus, for example, the "faculty" of language is associated irrevocably with centres of vision, of hearing, and of muscular activity, to go no further, and only becomes possible through the association of these widely separated centres. The destruction of Broca's centre, as was early discovered, does not altogether deprive a patient of his knowledge of language. He may be totally unable to speak (though as to this there are all degrees of variation), and yet may comprehend what is said to him, and be able to read, think, and even write correctly. Thus it appears that Broca's centre is peculiarly bound up with the capacity for articulate speech, but is far enough from being the seat of the faculty of language in its entirety.

In a similar way, most of the supposed isolated "faculties" of higher intellection appear, upon clearer analysis, as complex aggregations of primary sensations, and hence necessarily dependent upon numerous and scattered centres. Some "faculties," as memory and volition, may be said in a sense to be primordial endowments of every nerve cell--even of every body cell. Indeed, an ultimate analysis relegates all intellection, in its primordial adumbrations, to every particle of living matter. But such refinements of analysis, after all, cannot hide the fact that certain forms of higher intellection involve a pretty definite collocation and elaboration of special sensations. Such specialization, indeed, seems a necessary accompaniment of mental evolution. That every such specialized function has its localized centres of co-ordination, of some such significance as the demonstrated centres of articulate speech, can hardly be in doubt--though this, be it understood, is an induction, not as yet a demonstration. In other words, there is every reason to believe that numerous "centres," in this restricted sense, exist in the brain that have as yet eluded the investigator. Indeed, the current conception regards the entire cerebral cortex as chiefly composed of centres of ultimate co-ordination of impressions, which in their cruder form are received by more primitive nervous tissues--the basal ganglia, the cerebellum and medulla, and the spinal cord.

This, of course, is equivalent to postulating the cerebral cortex as the exclusive seat of higher intellection. This proposition, however, to which a safe induction seems to lead, is far afield from the substantiation of the old conception of brain localization, which was based on faulty psychology and equally faulty inductions from few premises. The details of Gall's system, as propounded by generations of his mostly unworthy followers, lie quite beyond the pale of scientific discussion. Yet, as I have said, a germ of truth was there--the idea of specialization of cerebral functions--and modern investigators have rescued that central conception from the phrenological rubbish heap in which its discoverer unfortunately left it buried.

THE MINUTE STRUCTURE OF THE BRAIN

The common ground of all these various lines of investigations of pathologist, anatomist, physiologist, physicist, and psychologist is, clearly, the central nervous system--the spinal cord and the brain. The importance of these structures as the foci of nervous and mental activities has been recognized more and more with each new accretion of knowledge, and the efforts to fathom the secrets of their intimate structure has been unceasing. For the earlier students, only the crude methods of gross dissections and microscopical inspection were available. These could reveal something, but of course the inner secrets were for the keener insight of the microscopist alone. And even for him the task of investigation was far from facile, for the central nervous tissues are the most delicate and fragile, and on many accounts the most difficult of manipulation of any in the body.

Special methods, therefore, were needed for this essay, and brain histology has progressed by fitful impulses, each forward jet marking the introduction of some ingenious improvement of mechanical technique, which placed a new weapon in the hands of the investigators.

The very beginning was made in 1824 by Rolando, who first thought of cutting chemically hardened pieces of brain tissues into thin sections for microscopical examination--the basal structure upon which almost all the later advances have been conducted. Muller presently discovered that bichromate of potassium in solution makes the best of fluids for the preliminary preservation and hardening of the tissues. Stilling, in 1842, perfected the method by introducing the custom of cutting a series of consecutive sections of the same tissue, in order to trace nerve tracts and establish spacial relations. Then from time to time mechanical ingenuity added fresh details of improvement. It was found that pieces of hardened tissue of extreme delicacy can be made better subject to manipulation by being impregnated with collodion or celloidine and embedded in paraffine. Latterly it has become usual to cut sections also from fresh tissues, unchanged by chemicals, by freezing them suddenly with vaporized ether or, better, carbonic acid. By these methods, and with the aid of perfected microtomes, the worker of recent periods avails himself of sections of brain tissues of a tenuousness which the early investigators could not approach.

But more important even than the cutting of thin sections is the process of making the different parts of the section visible, one tissue differentiated from another. The thin section, as the early workers examined it, was practically colorless, and even the crudest details of its structure were made out with extreme difficulty. Remak did, indeed, manage to discover that the brain tissue is cellular, as early as 1833, and Ehrenberg in the same year saw that it is also fibrillar, but beyond this no great advance was made until 1858, when a sudden impulse was received from a new process introduced by Gerlach. The process itself was most simple, consisting essentially of nothing more than the treatment of a microscopical section with a solution of carmine. But the result was wonderful, for when such a section was placed under the lens it no longer appeared homogeneous. Sprinkled through its substance were seen irregular bodies that had taken on a beautiful color, while the matrix in which they were embedded remained unstained. In a word, the central nerve cell had sprung suddenly into clear view.

A most interesting body it proved, this nerve cell, or ganglion cell, as it came to be called. It was seen to be exceedingly minute in size, requiring high powers of the microscope to make it visible. It exists in almost infinite numbers, not, however, scattered at random through the brain and spinal cord. On the contrary, it is confined to those portions of the central nervous masses which to the naked eye appear gray in color, being altogether wanting in the white substance which makes up the chief mass of the brain. Even in the gray matter, though sometimes thickly distributed, the ganglion cells are never in actual contact one with another; they always lie embedded in intercellular tissues, which came to be known, following Virchow, as the neuroglia.

Each ganglion cell was seen to be irregular in contour, and to have jutting out from it two sets of minute fibres, one set relatively short, indefinitely numerous, and branching in every direction; the other set limited in number, sometimes even single, and starting out directly from the cell as if bent on a longer journey. The numerous filaments came to be known as protoplasmic processes; the other fibre was named, after its discoverer, the axis cylinder of Deiters. It was a natural inference, though not clearly demonstrable in the sections, that these filamentous processes are the connecting links between the different nerve cells and also the channels of communication between nerve cells and the periphery of the body. The white substance of brain and cord, apparently, is made up of such connecting fibres, thus bringing the different ganglion cells everywhere into communication one with another.

In the attempt to trace the connecting nerve tracts through this white substance by either macroscopical or microscopical methods, most important aid is given by a method originated by Waller in 1852. Earlier than that, in 1839, Nasse had discovered that a severed nerve cord degenerates in its peripheral portions. Waller discovered that every nerve fibre, sensory or motor, has a nerve cell to or from which it leads, which dominates its nutrition, so that it can only retain its vitality while its connection with that cell is intact. Such cells he named trophic centres. Certain cells of the anterior part of the spinal cord, for example, are the trophic centres of the spinal motor nerves. Other trophic centres, governing nerve tracts in the spinal cord itself, are in the various regions of the brain. It occurred to Waller that by destroying such centres, or by severing the connection at various regions between a nervous tract and its trophic centre, sharply defined tracts could be made to degenerate, and their location could subsequently be accurately defined, as the degenerated tissues take on a changed aspect, both to macroscopical and microscopical observation. Recognition of this principle thus gave the experimenter a new weapon of great efficiency in tracing nervous connections. Moreover, the same principle has wide application in case of the human subject in disease, such as the lesion of nerve tracts or the destruction of centres by localized tumors, by embolisms, or by traumatisms.

All these various methods of anatomical examination combine to make the conclusion almost unavoidable that the central ganglion cells are the veritable "centres" of nervous activity to which so many other lines of research have pointed. The conclusion was strengthened by experiments of the students of motor localization, which showed that the veritable centres of their discovery lie, demonstrably, in the gray cortex of the brain, not in the white matter. But the full proof came from pathology. At the hands of a multitude of observers it was shown that in certain well-known diseases of the spinal cord, with resulting paralysis, it is the ganglion cells themselves that are found to be destroyed. Similarly, in the case of sufferers from chronic insanities, with marked dementia, the ganglion cells of the cortex of the brain are found to have undergone degeneration.

The brains of paretics in particular show such degeneration, in striking correspondence with their mental decadence. The position of the ganglion cell as the ultimate centre of nervous activities was thus placed beyond dispute.

Meantime, general acceptance being given the histological scheme of Gerlach, according to which the mass of the white substance of the brain is a mesh-work of intercellular fibrils, a proximal idea seemed attainable of the way in which the ganglionic activities are correlated, and, through association, built up, so to speak, into the higher mental processes. Such a conception accorded beautifully with the ideas of the associationists, who had now become dominant in psychology. But one standing puzzle attended this otherwise satisfactory correlation of anatomical observations and psychic analyses. It was this: Since, according to the histologist, the intercellular fibres, along which impulses are conveyed, connect each brain cell, directly or indirectly, with every other brain cell in an endless mesh-work, how is it possible that various sets of cells may at times be shut off from one another? Such isolation must take place, for all normal ideation depends for its integrity quite as much upon the shutting-out of the great mass of associations as upon the inclusion of certain other associations. For example, a student in solving a mathematical problem must for the moment become quite oblivious to the special associations that have to do with geography, natural history, and the like. But does histology give any clew to the way in which such isolation may be effected?

Attempts were made to find an answer through consideration of the very peculiar character of the blood-supply in the brain. Here, as nowhere else, the terminal twigs of the arteries are arranged in closed systems, not anastomosing freely with neighboring systems. Clearly, then, a restricted area of the brain may, through the controlling influence of the vasomotor nerves, be flushed with arterial blood while neighboring parts remain relatively anaemic. And since vital activities unquestionably depend in part upon the supply of arterial blood, this peculiar arrangement of the vascular mechanism may very properly be supposed to aid in the localized activities of the central nervous ganglia. But this explanation left much to be desired--in particular when it is recalled that all higher intellection must in all probability involve multitudes of widely scattered centres.

No better explanation was forthcoming, however, until the year 1889, when of a sudden the mystery was cleared away by a fresh discovery. Not long before this the Italian histologist Dr. Camille Golgi had discovered a method of impregnating hardened brain tissues with a solution of nitrate of silver, with the result of staining the nerve cells and their processes almost infinitely better than was possible by the methods of Gerlach, or by any of the multiform methods that other workers had introduced. Now for the first time it became possible to trace the cellular prolongations definitely to their termini, for the finer fibrils had not been rendered visible by any previous method of treatment. Golgi himself proved that the set of fibrils known as protoplasmic prolongations terminate by free extremities, and have no direct connection with any cell save the one from which they spring. He showed also that the axis cylinders give off multitudes of lateral branches not hitherto suspected. But here he paused, missing the real import of the discovery of which he was hard on the track. It remained for the Spanish histologist Dr. S. Ramon y Cajal to follow up the investigation by means of an improved application of Golgi's method of staining, and to demonstrate that the axis cylinders, together with all their collateral branches, though sometimes extending to a great distance, yet finally terminate, like the other cell prolongations, in arborescent fibrils having free extremities. In a word, it was shown that each central nerve cell, with its fibrillar

offshoots, is an isolated entity. Instead of being in physical connection with a multitude of other nerve cells, it has no direct physical connection with any other nerve cell whatever.

When Dr. Cajal announced his discovery, in 1889, his revolutionary claims not unnaturally amazed the mass of histologists. There were some few of them, however, who were not quite unprepared for the revelation; in particular His, who had half suspected the independence of the cells, because they seemed to develop from dissociated centres; and Forel, who based a similar suspicion on the fact that he had never been able actually to trace a fibre from one cell to another. These observers then came readily to repeat Cajal's experiments. So also did the veteran histologist Kolliker, and soon afterwards all the leaders everywhere. The result was a practically unanimous confirmation of the Spanish histologist's claims, and within a few months after his announcements the old theory of union of nerve cells into an endless mesh-work was completely discarded, and the theory of isolated nerve elements--the theory of neurons, as it came to be called--was fully established in its place.

As to how these isolated nerve cells functionate, Dr. Cajal gave the clew from the very first, and his explanation has met with universal approval.

In the modified view, the nerve cell retains its old position as the storehouse of nervous energy. Each of the filaments jutting out from the cell is held, as before, to be indeed a transmitter of impulses, but a transmitter that operates intermittently, like a telephone wire that is not always "connected," and, like that wire, the nerve fibril operates by contact and not by continuity. Under proper stimulation the ends of the fibrils reach out, come in contact with other end fibrils of other cells, and conduct their destined impulse. Again they retract, and communication ceases for the time between those particular cells. Meantime, by a different arrangement of the various conductors, different sets of cells are placed in communication, different associations of nervous impulses induced, different trains of thought engendered. Each fibril when retracted becomes a non-conductor, but when extended and in contact with another fibril, or with the body of another cell, it conducts its message as readily as a continuous filament could do--precisely as in the case of an electric wire.

This conception, founded on a most tangible anatomical basis, enables us to answer the question as to how ideas are isolated, and also, as Dr. Cajal points out, throws new light on many other mental processes. One can imagine, for example, by keeping in mind the flexible nerve prolongations, how new trains of thought may be engendered through novel associations of cells; how facility of thought or of action in certain directions is acquired through the habitual making of certain nerve-cell connections; how certain bits of knowledge may escape our memory and refuse to be found for a time because of a temporary incapacity of the nerve cells to make the proper connections, and so on indefinitely.

If one likens each nerve cell to a central telephone office, each of its filamentous prolongations to a telephone wire, one can imagine a striking analogy between the modus operandi of nervous processes and of the telephone system. The utility of new connections at the central office, the uselessness of the mechanism when the connections cannot be made, the "wires in use" that retard your message, perhaps even the crossing of wires, bringing you a jangle of sounds far different from what you desire--all these and a multiplicity of other things that will suggest themselves to

every user of the telephone may be imagined as being almost ludicrously paralleled in the operations of the nervous mechanism. And that parallel, startling as it may seem, is not a mere futile imagining. It is sustained and rendered plausible by a sound substratum of knowledge of the anatomical conditions under which the central nervous mechanism exists, and in default of which, as pathology demonstrates with no less certitude, its functionings are futile to produce the normal manifestations of higher intellection.

X. THE NEW SCIENCE OF ORIENTAL ARCHAEOLOGY

HOW THE "RIDDLE OF THE SPHINX" WAS READ

Conspicuously placed in the great hall of Egyptian antiquities in the British Museum is a wonderful piece of sculpture known as the Rosetta Stone. I doubt if any other piece in the entire exhibit attracts so much attention from the casual visitor as this slab of black basalt on its telescope-like pedestal. The hall itself, despite its profusion of strangely sculptured treasures, is never crowded, but before this stone you may almost always find some one standing, gazing with more or less of discernment at the strange characters that are graven neatly across its upturned, glass-protected face. A glance at this graven surface suffices to show that three sets of inscriptions are recorded there. The upper one, occupying about one-fourth of the surface, is a pictured scroll, made up of chains of those strange outlines of serpents, hawks, lions, and so on, which are recognized, even by the least initiated, as hieroglyphics. The middle inscription, made up of lines, angles, and half-pictures, one might surmise to be a sort of abbreviated or short-hand hieroglyphic. The third or lower inscription is Greek--obviously a thing of words. If the screeds above be also made of words, only the elect have any way of proving the fact.

Fortunately, however, even the least scholarly observer is left in no doubt as to the real import of the thing he sees, for an obliging English label tells us that these three inscriptions are renderings of the same message, and that this message is a "decree of the priests of Memphis conferring divine honors on Ptolemy V. (Epiphenes), King of Egypt, B.C. 195." The label goes on to state that the upper inscription (of which, unfortunately, only part of the last dozen lines or so remains, the slab being broken) is in "the Egyptian language, in hieroglyphics, or writing of the priests"; the second inscription "in the same language is in Demotic, or the writing of the people"; and the third "the Greek language and character." Following this is a brief biography of the Rosetta Stone itself, as follows: "The stone was found by the French in 1798 among the ruins of Fort Saint Julien, near the Rosetta mouth of the Nile. It passed into the hands of the British by the treaty of Alexandria, and was deposited in the British Museum in the year 1801." There is a whole volume of history in that brief inscription--and a bitter sting thrown in, if the reader chance to be a Frenchman. Yet the facts involved could scarcely be suggested more modestly. They are recorded much more bluntly in a graven inscription on the side of the stone, which reads: "Captured in Egypt by the British Army, 1801." No Frenchman could read those words without a veritable sinking of the heart.

The value of the Rosetta Stone depended on the fact that it gave promise, even when casually inspected, of furnishing a key to the centuries-old mystery of the hieroglyphics. For two thousand years the secret of these strange markings had been forgotten. Nowhere in the world--quite as little in Egypt as elsewhere--had any man the slightest clew to their meaning; there were those who even doubted whether these droll picturings really had any specific meaning, questioning whether they were not rather vague symbols of esoteric religious import and nothing more. And it was the Rosetta Stone that gave the answer to these doubters and restored to the world a lost language and a forgotten literature.

The trustees of the museum recognized at once that the problem of the Rosetta Stone was one on which the scientists of the world might well exhaust their ingenuity, and promptly published to the world a carefully lithographed copy of the entire inscription, so that foreign scholarship had equal opportunity with the British to try at the riddle. It was an Englishman, however, who first gained a clew to the solution. This was none other than the extraordinary Dr. Thomas Young, the demonstrator of the vibratory nature of light.

Young's specific discoveries were these: (1) That many of the pictures of the hieroglyphics stand for the names of the objects actually delineated; (2) that other pictures are sometimes only symbolic; (3) that plural numbers are represented by repetition; (4) that numerals are represented by dashes; (5) that hieroglyphics may read either from the right or from the left, but always from the direction in which the animal and human figures face; (6) that proper names are surrounded by a graven oval ring, making what he called a cartouche; (7) that the cartouches of the preserved portion of the Rosetta Stone stand for the name of Ptolemy alone; (8) that the presence of a female figure after such cartouches in other inscriptions always denotes the female sex; (9) that within the cartouches the hieroglyphic symbols have a positively phonetic value, either alphabetic or syllabic; and (10) that several different characters may have the same phonetic value.

Just what these phonetic values are Young pointed out in the case of fourteen characters representing nine sounds, six of which are accepted to-day as correctly representing the letters to which he ascribed them, and the three others as being correct regarding their essential or consonant element. It is clear, therefore, that he was on the right track thus far, and on the very verge of complete discovery. But, unfortunately, he failed to take the next step, which would have been to realize that the same phonetic values which were given to the alphabetic characters within the cartouches were often ascribed to them also when used in the general text of an inscription; in other words, that the use of an alphabet was not confined to proper names. This was the great secret which Young missed and which his French successor, Jean Francois Champollion, working on the foundation that Young had laid, was enabled to ferret out.

Young's initial studies of the Rosetta Stone were made in 1814; his later publication bore date of 1819. Champollion's first announcement of results came in 1822; his second and more important one in 1824. By this time, through study of the cartouches of other inscriptions, Champollion had made out almost the complete alphabet, and the "riddle of the Sphinx" was practically solved. He proved that the Egyptians had developed a relatively complete alphabet (mostly neglecting the vowels, as early Semitic alphabets did also) centuries before the Phoenicians were heard of in history. What relation this alphabet bore to the Phoenician we shall have occasion to ask in another

connection; for the moment it suffices to know that those strange pictures of the Egyptian scroll are really letters.

Even this statement, however, must be in a measure modified. These pictures are letters and something more. Some of them are purely alphabetical in character and some are symbolic in another way. Some characters represent syllables. Others stand sometimes as mere representatives of sounds, and again, in a more extended sense, as representations of things, such as all hieroglyphics doubtless were in the beginning. In a word, this is an alphabet, but not a perfected alphabet, such as modern nations are accustomed to; hence the enormous complications and difficulties it presented to the early investigators.

Champollion did not live to clear up all these mysteries. His work was taken up and extended by his pupil Rossellini, and in particular by Dr. Richard Lepsius in Germany, followed by M. Bernouf, and by Samuel Birch of the British Museum, and more recently by such well-known Egyptologists as MM. Maspero and Mariette and Chabas, in France, Dr. Brugsch, in Germany, and Dr. E. Wallis Budge, the present head of the Department of Oriental Antiquities at the British Museum. But the task of later investigators has been largely one of exhumation and translation of records rather than of finding methods.

TREASURES FROM NINEVEH

The most casual wanderer in the British Museum can hardly fail to notice two pairs of massive sculptures, in the one case winged bulls, in the other winged lions, both human-headed, which guard the entrance to the Egyptian hall, close to the Rosetta Stone. Each pair of these weird creatures once guarded an entrance to the palace of a king in the famous city of Nineveh. As one stands before them his mind is carried back over some twenty-seven intervening centuries, to the days when the "Cedar of Lebanon" was "fair in his greatness" and the scourge of Israel.

The very Sculptures before us, for example, were perhaps seen by Jonah when he made that famous voyage to Nineveh some seven or eight hundred years B.C. A little later the Babylonian and the Mede revolted against Assyrian tyranny and descended upon the fair city of Nineveh, and almost literally levelled it to the ground. But these great sculptures, among other things, escaped destruction, and at once hidden and preserved by the accumulating debris of the centuries, they stood there age after age, their very existence quite forgotten. When Xenophon marched past their site with the ill-starred expedition of the ten thousand, in the year 400 B.C., he saw only a mound which seemed to mark the site of some ancient ruin; but the Greek did not suspect that he looked upon the site of that city which only two centuries before had been the mistress of the world.

So ephemeral is fame! And yet the moral scarcely holds in the sequel; for we of to-day, in this new, undreamed-of Western world, behold these mementos of Assyrian greatness fresh from their twenty-five hundred years of entombment, and with them records which restore to us the history of that long-forgotten people in such detail as it was not known to any previous generation since the fall of Nineveh. For two thousand five hundred years no one saw these treasures or knew that they existed. One hundred generations of men came and went without once pronouncing the name of kings Shalmaneser or Asumazirpal or Asurbanipal. And to-day, after these centuries of oblivion,

these names are restored to history, and, thanks to the character of their monuments, are assured a permanency of fame that can almost defy time itself. It would be nothing strange, but rather in keeping with their previous mutations of fortune, if the names of Asurnazirpal and Asurbanipal should be familiar as household words to future generations that have forgotten the existence of an Alexander, a Caesar, and a Napoleon. For when Macaulay's prospective New Zealander explores the ruins of the British Museum the records of the ancient Assyrians will presumably still be there unscathed, to tell their story as they have told it to our generation, though every manuscript and printed book may have gone the way of fragile textures.

But the past of the Assyrian sculptures is quite necromantic enough without conjuring for them a necromantic future. The story of their restoration is like a brilliant romance of history. Prior to the middle of this century the inquiring student could learn in an hour or so all that was known in fact and in fable of the renowned city of Nineveh. He had but to read a few chapters of the Bible and a few pages of Diodorus to exhaust the important literature on the subject. If he turned also to the pages of Herodotus and Xenophon, of Justin and Aelian, these served chiefly to confirm the suspicion that the Greeks themselves knew almost nothing more of the history of their famed Oriental forerunners. The current fables told of a first King Ninus and his wonderful queen Semiramis; of Sennacherib the conqueror; of the effeminate Sardanapalus, who neglected the warlike ways of his ancestors but perished gloriously at the last, with Nineveh itself, in a self-imposed holocaust. And that was all. How much of this was history, how much myth, no man could say; and for all any one suspected to the contrary, no man could ever know. And to-day the contemporary records of the city are before us in such profusion as no other nation of antiquity, save Egypt alone, can at all rival. Whole libraries of Assyrian books are at hand that were written in the seventh century before our era. These, be it understood, are the original books themselves, not copies. The author of that remote time appeals to us directly, hand to eye, without intermediary transcriber. And there is not a line of any Hebrew or Greek manuscript of a like age that has been preserved to us; there is little enough that can match these ancient books by a thousand years. When one reads Moses or Isaiah, Homer, Hesiod, or Herodotus, he is but following the transcription--often unquestionably faulty and probably never in all parts perfect--of successive copyists of later generations. The oldest known copy of the Bible, for example, dates probably from the fourth century A.D., a thousand years or more after the last Assyrian records were made and read and buried and forgotten.

There was at least one king of Assyria--namely, Asurbanipal, whose palace boasted a library of some ten thousand volumes--a library, if you please, in which the books were numbered and shelved systematically, and classified and cared for by an official librarian. If you would see some of the documents of this marvellous library you have but to step past the winged lions of Asurnazirpal and enter the Assyrian hall just around the corner from the Rosetta Stone. Indeed, the great slabs of stone from which the lions themselves are carved are in a sense books, inasmuch as there are written records inscribed on their surface. A glance reveals the strange characters in which these records are written, graven neatly in straight lines across the stone, and looking to casual inspection like nothing so much as random flights of arrow-heads. The resemblance is so striking that this is sometimes called the arrow-head character, though it is more generally known as the wedge or cuneiform character. The inscriptions on the flanks of the lions are, however, only makeshift books. But the veritable books are no farther away than the next room beyond the hall of Asurnazirpal. They occupy part of a series of cases placed down the centre of this room. Perhaps it

is not too much to speak of this collection as the most extraordinary set of documents of all the rare treasures of the British Museum, for it includes not books alone, but public and private letters, business announcements, marriage contracts--in a word, all the species of written records that enter into the every-day life of an intelligent and cultured community.

But by what miracle have such documents been preserved through all these centuries? A glance makes the secret evident. It is simply a case of time-defying materials. Each one of these Assyrian documents appears to be, and in reality is, nothing more or less than an inscribed fragment of brick, having much the color and texture of a weathered terra-cotta tile of modern manufacture. These slabs are usually oval or oblong in shape, and from two or three to six or eight inches in length and an inch or so in thickness. Each of them was originally a portion of brick-clay, on which the scribe indented the flights of arrowheads with some sharp-cornered instrument, after which the document was made permanent by baking. They are somewhat fragile, of course, as all bricks are, and many of them have been more or less crumbled in the destruction of the palace at Nineveh; but to the ravages of mere time they are as nearly invulnerable as almost anything in nature. Hence it is that these records of a remote civilization have been preserved to us, while the similar records of such later civilizations as the Grecian have utterly perished, much as the flint implements of the cave-dweller come to us unchanged, while the iron implements of a far more recent age have crumbled away.

HOW THE RECORDS WERE READ

After all, then, granted the choice of materials, there is nothing so very extraordinary in the mere fact of preservation of these ancient records. To be sure, it is vastly to the credit of nineteenth-century enterprise to have searched them out and brought them back to light. But the real marvel in connection with them is the fact that nineteenth-century scholarship should have given us, not the material documents themselves, but a knowledge of their actual contents. The flight of arrow-heads on wall or slab or tiny brick have surely a meaning; but how shall we guess that meaning? These must be words; but what words? The hieroglyphics of the Egyptians were mysterious enough in all conscience; yet, after all, their symbols have a certain suggestiveness, whereas there is nothing that seems to promise a mental leverage in the unbroken succession of these cuneiform dashes. Yet the Assyrian scholar of to-day can interpret these strange records almost as readily and as surely as the classical scholar interprets a Greek manuscript. And this evidences one of the greatest triumphs of nineteenth-century scholarship, for within almost two thousand years no man has lived, prior to our century, to whom these strange inscriptions would not have been as meaningless as they are to the most casual stroller who looks on them with vague wonderment here in the museum to-day. For the Assyrian language, like the Egyptian, was veritably a dead language; not, like Greek and Latin, merely passed from practical every-day use to the closet of the scholar, but utterly and absolutely forgotten by all the world. Such being the case, it is nothing less than marvellous that it should have been restored.

It is but fair to add that this restoration probably never would have been effected, with Assyrian or with Egyptian, had the language in dying left no cognate successor; for the powers of modern linguistry, though great, are not actually miraculous. But, fortunately, a language once developed is not blotted out in toto; it merely outlives its usefulness and is gradually supplanted, its successor

retaining many traces of its origin. So, just as Latin, for example, has its living representatives in Italian and the other Romance tongues, the language of Assyria is represented by cognate Semitic languages. As it chances, however, these have been of aid rather in the later stages of Assyrian study than at the very outset; and the first clew to the message of the cuneiform writing came through a slightly different channel.

Curiously enough, it was a trilingual inscription that gave the clew, as in the case of the Rosetta Stone, though with very striking difference withal. The trilingual inscription now in question, instead of being a small, portable monument, covers the surface of a massive bluff at Behistun in western Persia. Moreover, all three of its inscriptions are in cuneiform characters, and all three are in languages that at the beginning of our century were absolutely unknown. This inscription itself, as a striking monument of unknown import, had been seen by successive generations. Tradition ascribed it, as we learn from Ctesias, through Diodorus, to the fabled Assyrian queen Semiramis. Tradition was quite at fault in this; but it is only recently that knowledge has availed to set it right. The inscription, as is now known, was really written about the year 515 B.C., at the instance of Darius I., King of Persia, some of whose deeds it recounts in the three chief languages of his widely scattered subjects.

The man who at actual risk of life and limb copied this wonderful inscription, and through interpreting it became the veritable "father of Assyriology," was the English general Sir Henry Rawlinson. His feat was another British triumph over the same rivals who had competed for the Rosetta Stone; for some French explorers had been sent by their government, some years earlier, expressly to copy this strange record, and had reported that it was impossible to reach the inscription. But British courage did not find it so, and in 1835 Rawlinson scaled the dangerous height and made a paper cast of about half the inscription. Diplomatic duties called him away from the task for some years, but in 1848 he returned to it and completed the copy of all parts of the inscription that have escaped the ravages of time. And now the material was in hand for a new science, which General Rawlinson himself soon, assisted by a host of others, proceeded to elaborate.

The key to the value of this unique inscription lies in the fact that its third language is ancient Persian. It appears that the ancient Persians had adopted the cuneiform character from their western neighbors, the Assyrians, but in so doing had made one of those essential modifications and improvements which are scarcely possible to accomplish except in the transition from one race to another. Instead of building with the arrow-head a multitude of syllabic characters, including many homophones, as had been and continued to be the custom with the Assyrians, the Persians selected a few of these characters and ascribed to them phonetic values that were almost purely alphabetic. In a word, while retaining the wedge as the basal stroke of their script, they developed an alphabet, making the last wonderful analysis of phonetic sounds which even to this day has escaped the Chinese, which the Egyptians had only partially effected, and which the Phoenicians were accredited by the Greeks with having introduced to the Western world. In addition to this all-essential step, the Persians had introduced the minor but highly convenient custom of separating the words of a sentence from one another by a particular mark, differing in this regard not only from the Assyrians and Egyptians, but from the early Greek scribes as well.

Thanks to these simplifications, the old Persian language had been practically restored about the beginning of the nineteenth century, through the efforts of the German Grotefend, and further advances in it were made just at this time by Renouf, in France, and by Lassen, in Germany, as well as by Rawlinson himself, who largely solved the problem of the Persian alphabet independently. So the Persian portion of the Behistun inscription could be at least partially deciphered. This in itself, however, would have been no very great aid towards the restoration of the languages of the other portions had it not chanced, fortunately, that the inscription is sprinkled with proper names. Now proper names, generally speaking, are not translated from one language to another, but transliterated as nearly as the genius of the language will permit. It was the fact that the Greek word Ptolemaics was transliterated on the Rosetta Stone that gave the first clew to the sounds of the Egyptian characters. Had the upper part of the Rosetta Stone been preserved, on which, originally, there were several other names, Young would not have halted where he did in his decipherment.

But fortune, which had been at once so kind and so tantalizing in the case of the Rosetta Stone, had dealt more gently with the Behistun inscriptions; for no fewer than ninety proper names were preserved in the Persian portion and duplicated, in another character, in the Assyrian inscription. A study of these gave a clew to the sounds of the Assyrian characters. The decipherment of this character, however, even with this aid, proved enormously difficult, for it was soon evident that here it was no longer a question of a nearly perfect alphabet of a few characters, but of a syllabary of several hundred characters, including many homophones, or different forms for representing the same sound. But with the Persian translation for a guide on the one hand, and the Semitic languages, to which family the Assyrian belonged, on the other, the appalling task was gradually accomplished, the leading investigators being General Rawlinson, Professor Hincks, and Mr. Fox-Talbot, in England, Professor Jules Oppert, in Paris, and Professor Julian Schrader, in Germany, though a host of other scholars soon entered the field.

This great linguistic feat was accomplished about the middle of the nineteenth century. But so great a feat was it that many scholars of the highest standing, including Joseph Erneste Renan, in France, and Sir G. Cornewall Lewis, in England, declined at first to accept the results, contending that the Assyriologists had merely deceived themselves by creating an arbitrary language. The matter was put to a test in 1855 at the suggestion of Mr. Fox-Talbot, when four scholars, one being Mr. Talbot himself and the others General Rawlinson, Professor Hincks, and Professor Oppert, laid before the Royal Asiatic Society their independent interpretations of a hitherto untranslated Assyrian text. A committee of the society, including England's greatest historian of the century, George Grote, broke the seals of the four translations, and reported that they found them unequivocally in accord as regards their main purport, and even surprisingly uniform as regards the phraseology of certain passages--in short, as closely similar as translations from the obscure texts of any difficult language ever are. This decision gave the work of the Assyriologists official status, and the reliability of their method has never since been in question. Henceforth Assyriology was an established science.

APPENDIX

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